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Margaret Floy Washburn

1871-1939

Margaret Floy Washburn, whose services to this JOURNAL extended over a period of thirty-six years (as coöperating editor from 1903-1925 and co-editor since 1926), died after a long illness at Poughkeepsie, New York, on the afternoon of October 29, 1939. She was in her sixty-ninth year, having been born to Rev. Francis and Elizabeth Floy (Davis) Washburn on July 25, 1871, in Harlem, New York City. Her last illness dated from March 18, 1937, when she suffered a cerebral hemorrhage.

Professor Washburn's ancestry, as she relates in the *History of Psychology in Autobiography,* were Dutch and English: Long Island and Westchester County Quakers and Maryland Cavaliers, with a dash of Connecticut Yankee. With the exception of one great-grandparent, Michael Floy, all of her ancestors were in America before 1720. Michael Floy, after whom she was named, came from Devonshire, England. A successful florist and nurseryman, he founded, upon the Harlem farm where she was born and where she passed her childhood, the family fortune "which enabled me," she writes, "to finish my professional training without having to earn my own living."

Professor Washburn was an only child, and the first eight years of her life were spent in association with adults amid the flowers and gardens of the ancestral home. She nowhere mentions childhood companions of her own age, but writes instead of the "blessed privilege of an only child to be undisturbed when at leisure." She was as a consequence precocious and self-sufficient, an omnivorous reader as a child and throughout her life. Though she did not enter school until she was seven years of age, she had learned to read and to write long before. Her advancement, when she did enter school, was rapid. Her first school was private—conducted in the home of a retired Presbyterian minister by his three accomplished daughters. In a year and a half with these teachers she learned the rudiments of mathematics and acquired a good foundation in French, German, and music. When she was nine years of age her father entered the Episcopal ministry and accepted a charge at Walden, a village in Orange County, New York. Here her school, though still a private one, was housed like a district school in a single-room building, and was poorly conducted. Her time was not fully occupied and she turned in her leisure to the writing of stories.

When she was eleven years old, her father was called to the rectorship of the Episcopal Church in Kingston, New York. There for the first time she attended the public schools. Her training had been good, hence her progress was rapid. She entered high school at twelve years of age and was graduated in June, 1886, at fifteen. The following fall found her at Vassar College. Because of the lack of Latin and French, which she had not studied since her earliest school days, her first year was spent in the preparatory department. Then followed four years in the College from which she was graduated in 1891 with an abiding interest in science and philosophy.

Having learned, during her senior year, of Cattell's newly established laboratory of psychology at Columbia University, she determined to become his pupil. Though women were not admitted to graduate study at Columbia in those days, she persevered, characteristically, in her determination and after three months of effort she was by special dispensation of the trustees permitted to register in Cattell's classes as a "hearer." Thus she was a pioneer in the struggle for equal educational opportunities for women. Despite her special status, Cattell treated her as a regular student and she attended his lectures and seminary and worked in his laboratory along with the men. From him she acquired her interest in psychology and the inspiration to make it a life career.

At the end of that year, since Cattell had no scholarships available for women, she applied at his suggestion to the newly established Sage School of Philosophy at Cornell University. A scholarship was granted her and in the fall of 1892 she went to Cornell. She found the Sage School an inspiring place to work. The men on the faculty were young and enthusiastic; all were beginning, as time has shown, highly successful careers.

She majored in psychology with Titchener, who, fresh from Leipzig, had joined the Cornell staff that fall; and minored in ethics with Schurman and in philosophy with Albee, Hammond, and Thilly. After a year at Cornell, Vassar College awarded her in 1893 an M.A. degree *in absentia* for work done under Titchener. A year later, in June, 1894, she obtained her Ph.D. degree from Cornell—the first Ph.D. that Titchener recommended. Her doctoral dissertation, Ueber den Einfluss der Gesichtsassociation auf die Raumwahrnehmung der Haut, was published in Wundt's *Philosophische Studien*—the first foreign study to appear there.

In the fall following her doctorate she went to Wells College as Professor of Psychology, Philosophy, and Ethics. She remained there six years, keeping up her contacts with Cornell by weekly visits to use the library and to attend seminaries and lectures of interest. Then followed two years (1900-1902) at Cornell as Warden of Sage College—that is, head resident of the women's dormitory—and, during the second year, as Lecturer in Psychology. She gave two courses in Titchener's department: social psychology, based on Wundt's *Völkerpsychologie;* and animal psychology, which was to remain a life-long interest. Finding the duties of Warden—"concerning oneself with the behavior of other people," as she described them—highly uncongenial, she accepted at the end of two years (in 1902) the offer of an Assistant Professorship with full charge of the Department of Psychology at the University of Cincinnati. Though conditions there were to her liking, she could not resist a call from her *Alma Mater,* and the fall of 1903 found her back at Vassar College as Associate Professor of Philosophy. At first her duties were divided between psychology and philosophy, but in 1908 the subjects were separated and she became Professor of Psychology and head of the newly created department. She remained at Vassar the rest of her life: when retired as Emeritus Professor of Psychology in June 1937 she was already in her last illness.

Miss Washburn was an indefatigable and persistent worker. Except for the summers of 1913-1917 when she taught psychology in the summer sessions of Columbia University; the spring of 1928 when, on her only sabbatical leave, she took a Mediterranean cruise; and the summers of 1929 and 1932 during which she traveled to England and Copenhagen—she never was far nor long away from her work in her laboratory and study at Poughkeepsie. That she was an inspiring teacher is evidenced by the growth of her department, by the many studies that were published by the girls from her laboratory, and by the number of her students who continued in psychology. Hers was strictly an undergraduate department, for she was, as far as graduate study was concerned, a proponent of coeducation. "I deprecate," she wrote in her autobiography, "graduate study for women at any but coeducational universities." True to this conviction, she encouraged her students to go to such schools for their final training. Many did, and wherever they went—to California, Columbia, Cornell, Harvard (Radcliffe), Illinois, Indiana, Iowa, Michigan, Ohio State, Yale—they made splendid records. She was also a kindly teacher, revered and beloved by the various generations of students working with her, as attested by the fact that they presented her in June 1928, on the completion of twenty-five years of service to Vassar College, with a purse containing a considerable sum of money. The donors hoped that she would use the money for her own pleasure—for books and travel; instead she disbursed it as scholarship-aids for students of psychology—for undergraduates at Vassar or Vassar graduates taking advanced work elsewhere.

Miss Washburn's contributions and services to psychology were many and outstanding. Besides publishing over 200 scientific articles and reviews, she translated Wundt's *Ethical Systems,* 1897, and wrote two books: *The Animal Mind,* 1908; and *Movement and Mental Imagery,* 1916. The first of these books went through three more editions—1917, 1926, and 1936—every one thoroughly rewritten and enlarged. The first edition of this book was translated in 1918 in Japanese. The second book, a development of her article in G. Stanley Hall's *Festschrift* in 1903, marked her break with Titchenerian doctrine and her espousal of the motor theory of consciousness for which she was thereafter widely known. Between 1905 and 1938, she published sixty-eight studies from the Vassar Psychological Laboratory—an undergraduate laboratory, be it remembered—with 117 students as joint authors. In all the studies she formulated the problems and set the method and procedure. After these matters were carefully explained, the students collected and worked up the data under her supervision and direction, and then she wrote up the experiment. During the same period she also gave unsparingly of her time and effort to editorial work. In addition to her editorship of this JOURNAL, she was coöperating editor of the *Psychological Bulletin,* 1909-1915; associate editor of the *Journal of Animal Behavior,* 1911-1917; advisory editor of the *Psychological Review,* 1916-1930; and associate editor of the *Journal of Comparative Psychology,* 1921-1935.

In recognition of her services to psychology she received the highest honors and awards at the disposal of her confrères. In 1919-1920, when the Division of Psychology and Anthropology of the National Research Council was formed, she was a representative of psychology and again in 1925-

1928. In 1921, she was president of the American Psychological Association; that same year, she was awarded a prize of $500 by the Edison Phonograph Company for the best research on the effects of music—a study of "The Emotional Effects of Instrumental Music" in collaboration with a colleague in the Department of Music at Vassar. In 1927 she was elected vice-president and chairman of Section I (Psychology) of the American Association for the Advancement of Science, and also was the recipient of a *Festschrift* and of an honorary degree of D.Sc. The *Festschrift,* volume 39 of this JOURNAL, was dedicated to her by its authors, thirty-two colleagues from the various editorial boards upon which she had served, "in recognition of thirty-three years of distinguished service to psychology." The degree, *honoris causa,* was conferred upon her by Wittenberg College during the International Symposium of Feeling and Emotion that was held there at the dedication of the new psychological laboratory. In 1929 she was elected to the International Committee on Psychology—the governing body of the International Congresses—and to the Society of Experimental Psychologists; and, in 1931, to membership in the National Academy of Sciences—the second woman to receive that honor, Dr. Florence Sabin, the anatomist and pathologist, having been elected before her. In this same year, 1931, she was president of the New York Branch of the American Psychological Association (now the Eastern Psychological Association) and chairman of the Society of Experimental Psychologists. In 1932, she was the U.S. delegate to the International Congress of Psychology in Copenhagen. She was also a member of the American Philosophical Society, the National Institute of Psychology, the New York Academy of Sciences, Sigma Xi and Phi Kappa Phi.

In addition, she served in many important committees and conferences; chairman of the committee of the American Psychological Association that negotiated the purchase and transfer of the Psychological Review Publications from Professor Howard C. Warren to the Association; chairman of the sub-committee of the Division of Psychology and Anthropology of the National Research Council that secured a large subsidy from the Laura Spelman Rockefeller Foundation for the publication of the Psychological Abstracts; chairman of the A.P.A. committee for the certification of consulting psychologists; chairman of the N.R.C. committee on the experimental investigation of emotion; and a member of the Wittenberg conference on feeling and emotion, of the Carlisle conference of experimental psychologists, and of the Washington conference of psychological editors.

Her death removes from American Psychology one of the most active and honored members of the *Fach.* Psychology will not see her like again.

KARL M. DALLENBACH

The Editorial Board

Editorial responsibility in the Journal has always—except for two short periods—been a coöperative affair. G. Stanley Hall founded the Journal and served it alone for six volumes (1887-1895), and Titchener was the sole editor of five volumes (1921-1925), but otherwise there has always been a board of editors. First there were Hall, Sanford, and Titchener (1895-1920) with Baird added later (1912), and then, after Titchener's resignation in 1925, ourselves and Miss Washburn, whose death now breaks a happy and fortunate association. We three propose to carry on, but we are seeking the support of an active coöperating board.

In the past the Coöperating Boards have tended to become honorary. The basis of Hall's selection was international reputation; the basis of Titchener's, friendship. In 1925 we chose a Board that assumed certain activities in advice and even met on occasion to discuss policies. Now it seems desirable, with the loss of Miss Washburn and with the increasing complexity of psychology, for us to ask a board of experts to serve the Journal with us in passing upon the suitability of manuscripts for publication. We shall continue to receive the manuscripts ourselves but shall refer them, as the occasion indicates, to one or another of the Coöperating Editors for assessment.

For the eight years preceding his death (1929-1935), Joseph Peterson was the very active and effective review editor of the Journal. During the last four years one of us has carried on his work, but we are now happy to announce that Dr. John G. Jenkins has consented to coöperate with us as review editor.

The new Coöperating Board is:

S. W. Fernberger, Univ. Pennsylvania
J. P. Guilford, University of Nebraska
Harry Helson, Bryn Mawr College
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W. S. Hunter, Brown University
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J. A. McGeoch, University of Iowa
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The members of this Board have been selected because of their expertness in special fields. We are grateful to them for their readiness to coöperate with us, and we hope with their aid to maintain the Journal at a standard worthy of its past.

Madison Bentley
Edwin G. Boring
Karl M. Dallenbach

THE PSYCHOLOGICAL CONTRIBUTIONS OF MARGARET FLOY WASHBURN

By MABEL F. MARTIN, Northampton State Hospital

Best known for her work in animal psychology, Professor Washburn made original contributions in an incredible variety of other fields that might have seemed unrelated to a mind of less encyclopedic scope. If Spearman is right in identifying general intelligence with the capacity to discover relationships, Miss Washburn had it to a remarkable degree. She found relationships between such diverse phenomena as organic sensations and social consciousness.

Her originality early attracted attention, her keen critical ability commanded respect, and her warm personality won lasting friends. Students and younger psychologists found Miss Washburn always approachable and encouraging. In meetings of the American Psychological Association, it was her custom to speak to any younger member who appeared lonely or shy and to help the newcomer to become acquainted. While the world mourns the scientist, those who knew her personally mourn a gracious, charming woman of wide sympathy and unfailing tact.

Miss Washburn's strenuous teaching and administrative duties never interfered with her own research. Productive scholarship was her vocation from her student days to the very end of her busy professorship. Her earliest publication reported experiments on the perception of distance in the inverted landscape.[1] A year later came an article on the influence of visual associations on the space perceptions of the skin.[2] Next came a study of the process of recognition,[3] and shortly thereafter a discussion of the psychology of deductive logic.[4]

Trained by Titchener, Miss Washburn was always interested in the subjective side of experience, including images. In 1899, she published the first of her many investigations of after-images.[5] Her article in the Hall *Festschrift,* in 1903,[6] foreshadowed four areas of interest in which she was to make significant contributions to psychology—the problems of social consciousness, the problems of revived and ideated emotions, the rôle of movement in the development of mental life, and the field of animal psychology. It is impossible to label them first, second, third, and fourth, because they are so intimately intertwined in this one short, pregnant article.

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[1] The perception of distance in the inverted landscape, *Mind,* 3, 1894, 438-440.

[2] Über den Einfluss der Gesichtsassociationen auf die Raumwahrnehmungen der Haut, *Philos. Stud.,* 11, 1895, 190-225.

[3] The process of recognition, *Philos. Rev.,* 6, 1897, 267-274.

[4] The psychology of deductive logic, *Mind,* 7, 1898, 523-530.

[5] Subjective colors and the after-image: Their significance for the theory of attention, *ibid.,* 8, 1899, 25-34.

[6] The genetic function of movement and organic sensations for social consciousness, this JOURNAL, 14, 1903, 337-342.

As early as 1900 she had begun to doubt the validity of the extreme structuralist position. In 1903, she wrote,

> "It is impossible, assuming only the mental structures discoverable by our present introspection, to give a continuous and coherent explanation of individual mental growth. There are breaks; the effect is more than the causes; the whole is greater than the structural parts we thought went to compose it. A striking example is to be found in the rise of social consciousness in the individual. By social consciousness, it is generally agreed, is meant 'ejective' consciousness, the reference of a certain mental process to another mind. Clear, fully realized social consciousness is a late product both in individual development and in the history of the animal mind; its existence in lower animals is more than doubtful and its defects in the human child are responsible for the cruelty often displayed by children. . . . How does the child come to have any power at all of thinking of experience as belonging to other minds? Nothing that we can find in our own conscious life at the present time will bridge the gap."[7]

At the time that these words were written, social consciousness was so generally taken for granted that it required Miss Washburn to call attention to the problem of its origin. Baldwin had sought to explain its rise in the individual child on the basis of imitation. Miss Washburn conceded that imitation and association may explain the *particular* social interpretation of a bit of behavior but not social interpretation in general.[8] To account for this, she proposed the interesting hypothesis that in the social animals certain motor reactions of coming to the rescue of a comrade in danger preceded the development of ejective consciousness,[9] and that the latter developed from the former. The cries of their comrade serve as stimuli, arousing other members of the herd to attack whatever other animal is threatening him. Thus instinctive, unreflective helpfulness forms the primitive substratum from which both the desire and the possibility of mutual understanding develop. She explains,

> "It is through the social action stimulated by the behavior of others that conscious creatures have been led to social interpretation of that behavior. Let us go back to our animal capable of forming representations on the one hand of its own past alarm, suggested perhaps by revisiting the scene of it; and on the other hand, of another animal's alarm, suggested by the sound of cries. The whole motor attitude is different in the two cases. These two ideas, necessarily similar in their internal

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[7] *Ibid.*, 73-74.

[8] *Ibid.*, 75.

[9] *Ibid.*

constitution, would differ in the escort of organic and movement sensations accompanying them. . . . From the dawn of the power to form ideas, the consciousness produced by manifestations of mental processes in another animal would contain different elements from those going to make up other representative consciousness; and these elements, the genetic elements of which we were in search, are the movements and organic sensations produced by motor reactions of social utility, already on the field before social consciousness develops."[10]

Thus, during the ascendency of extreme introspectionism, it was Miss Washburn who pointed out that social activity precedes social consciousness and that the animal responds to the *behavior* of its fellows rather than to any idea of their mental states. Later, during the ascendency of extreme behaviorism, psychologists began to conceive of the social behavior of *man* as a reaction to the *behavior* of others. It was then Miss Washburn who pointed out the one-sidedness of this conception. She showed that man reacts not so much to the overt behavior of his fellow men as to what he conceives their mental states to be.[11]

At the outset, her motor theory of consciousness was merely a brilliant hypothesis; but in later years, numerous experimental studies, undertaken primarily for other purposes, furnished evidence in support of it. In 1916, the theory was fully elaborated in her book entitled *Movement and Mental Imagery.*

In this book she gave evidence for the view that an internally anticipated movement is always present when in human consciousness we have a memory idea, and also that this internal anticipation of movements means actual slight contractions of the muscles involved in performing the movements.

Miss Washburn's original and fruitful theories were so numerous and varied that she could not possibly have elaborated all of them into books. Many are tucked away in short articles or mere brief notes in scientific and philosophical journals.

In 1904, two years before Sherrington published *The Integrative Action of the Nervous System,* in which he expounded the rôle of distance receptors in the development of mind, Miss Washburn had already pointed out that one essential condition for the development of higher mental processes is the possession of sense-organs for the reception of stimuli at a distance.[12] She showed that an important difference exists between stimuli from objects directly in contact with an organism's body and those which proceed from objects at a distance. Reaction must be almost instantaneous, if an organism is to escape an enemy in actual contact with its body. When the stimulus is at a distance, the danger is not so imminent. The full motor response may be delayed for a short interval without imperiling the life-interests of the animal. The possibility of delay brings in the further possibility of anticipation, and ultimately, the possibility of choice, learning, etc.

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[10] *Ibid.,* 78.

[11] Ejective consciousness as a fundamental factor in social psychology, *Psychol. Rev.,* 39, 1932, 395-402.

[12] A factor in mental development, *Philos. Rev.,* 13, 1904, 622-626.

As we have seen, Miss Washburn's earliest contributions to the field of animal psychology were theoretical; she sought to bridge the gap in our understanding of the human consciousness by a speculative excursion into the probable beginnings of such consciousness in animals antedating man; but her scientific habit of mind could not let her rest content with hypothesis unsupplemented by experiment. Beginning in 1906,[13] she published a large number of experimental studies of the mental life of animals. In 1908, she published *The Animal Mind.* The book met with wide and lasting popularity. The second edition appeared in 1917, the third in 1926, and the fourth in 1936.

From the beginning, her interest was in seeking to learn (indirectly of course) something about the subjective experience of animals, their sensations, their capacity for sensory discrimination, their power to form associations. Because the animals could not report their experience in words, it was necessary to observe their behavior and to infer their subjective experience from the way they behaved. Miss Washburn, always a keen logician, was aware of the inferential character of all knowledge about the mental life of animals, but she held that a large part of scientific knowledge is similarly inferential. The solipsistic dilemma was perfectly familiar to her. She wrote:

> "That the mind of each human being forms a region inaccessible to all save its possessor, is one of the commonplaces of reflection. . . . Each of us can judge his fellow-men only on the basis of his own thoughts and feelings in similar circumstances. . . . The science of human psychology has to reckon with this unbridgeable gap between minds as its chief difficulty."[14]

Her clear recognition of the fact that all psychology suffers from essentially the same difficulty kept her from despairing of insight into the animal mind and from becoming content with the superficiality of behaviorism. Some of the most delightful passages in her writings consist of efforts to bridge by imagination the gap between human consciousness and that of animals whose differing anatomical structure would prevent them from having the same sensations as people. She says,

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[13] M. F. Washburn and I. M. Bentley, The establishment of an association involving color discrimination in the creek chub, *Semotilus atromaculetus, J. Comp. Neur. & Psychol.,* 16, 1906, 113-125.

[14] *The Animal Mind,* 4th ed., 1936, 1.

> "A bodily structure entirely unlike our own must create a background of organic sensation which renders the whole mental life of an animal foreign and unfamiliar to us. We speak, for example, of an 'angry' wasp. Anger, in our own experience, is largely composed of sensations of quickened heart beat, of altered breathing, of muscular tension, of increased blood pressure in the head and face. The circulation of a wasp is fundamentally different from that of any vertebrate. The wasp does not breathe through lungs, it wears its skeleton on the outside, and it has the muscles attached to the inside of the skeleton. What is anger like in the wasp's consciousness? We can form no adequate idea of it."[15]

This unfailing recognition of the difference between overt activity and the consciousness behind it made her a keen critic of those who ignored consciousness.

Miss Washburn was much interested in methodology. She never forgot that the task of the psychologist is twofold: first to obtain the facts; and secondly to interpret them. Her emphasis shifted slightly from time to time to counterbalance the changing fashions in contemporary practice. At the beginning of her career, it was necessary to stress the importance of obtaining the facts. Nearly all the textbooks then available still relied too largely on the anecdotal method. Even Wundt lapsed into anecdote when it came to animal psychology. Miss Washburn pointed out the fallacy of relying on casual observers, who are not trained to distinguish what they see from what they infer. She showed the importance of knowing the habits of the species and even the past experience of the particular animal concerned. She also showed the need of precautions to avoid such complicating factors as hunger, fright, bewilderment, and loneliness. It was years before any large proportion of animal experimenters approximated her ideals for reliable experimental work; but at least a beginning had been made. In 1898, Thorndike's monograph on animal intelligence[16] (based on rigorous experimentation) had appeared and in the intervening ten years an increasing number of experimental studies had been published in a wide variety of journals. Miss Washburn felt the need of making this material more accessible to the ordinary student. She limited her book to the facts that had been determined by experiment, resisting the temptation to round out by anecdote those aspects of the animal mind not yet explored experimentally. In successive editions she was able to enlarge the scope of the book, because the steady advance of the science made more facts available. Never, however, did she fall into the error of substituting hearsay for fact. She always drew a clear-cut line between observation and inference.

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[15] *Ibid.*, 3.

[16] E. L. Thorndike, Animal intelligence, *Psychol. Monog.*, 2, 1898, (no. 8), 1-109.

On the other hand, she never forgot that observation is only the first part of the psychologist's task. His ultimate goal is an understanding of consciousness. The rise of behaviorism did not alter Miss Washburn's orientation. Instead, it sharpened her critical faculties. In the preface to the last edition, she wrote,

> "The principle change in the attitude of investigators of animal behavior since the third edition of this work appeared is the decay of behaviorism as an interpretation and the revival of animal psychology. The conclusions drawn from experiments are now expressed in subjective terms. Undoubtedly one cause of this change is the rise of the configurational school. But extreme behaviorism, which ignored the existence of all qualitative differences in sensations, would not have long endured."[17]

In this last edition of *The Animal Mind,* she added a chapter on higher mental processes. With characteristic fairness she reviewed a vast amount of experimental evidence which had been held to cast doubt on the existence of memory ideas in the lower animals. Her scientific impartiality in giving full weight to this evidence is the more remarkable when we remember that the postulate that such memory ideas *do* exist in at least *some* animals below man underlay several of her original theoretical contributions. It would have been easy to slight some of the negative evidence in view of the sheer bulk of experimental literature to be summarized within the one chapter. But Miss Washburn was now as always an impartial searcher after truth. She gave the negative evidence full weight at the outset. Fortunately for her original theories, the years intervening between the third and fourth editions of *The Animal Mind* brought forth a flood of experimental evidence for the position that she had held in the beginning. After reviewing one of D. K. Adams's experiments, she made the comment,

> "The decay of extreme behaviorism is illustrated by the experimenter's statement that the basis of these reactions 'was visual recognition, such as humans are accustomed to, with whatever identifying imaginal constituents that implies.' "[18]

Climaxing a vast accumulation of experimental evidence in favor of higher mental processes in certain animals, Professor Washburn described Krechevsky's rats and their hypotheses.

> "The abandonment of a hypothesis is abrupt. Learning, says Krechevsky, consists of changing from one systematic, generalized, purposive way of behaving to another and another until the problem is solved, and this at every point. One wonders what has become of the aimless scratching of Thorndike's dogs and cats. Can it be that the utter hunger of his subjects prevented their using their minds? It is quite possible."[19]

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[17] *The Animal Mind,* 4th ed., 1936, v.
[18] *Ibid.,* 360.
[19] *Ibid.,* 361.

In a number of publications, the affective life excelled all Miss Washburn's other interests. In this as in so many of the fields that she explored her earliest contributions were theoretical.[20] For the next twenty-five years, she produced a steady stream of articles dealing with various causes of feelings and various consequences. Even before sensation she considered ideational causes. Her natural clarity of mind, enhanced by her philosophical training, made her poignantly aware of logical fallacies wherever she encountered them. So it was perhaps quite natural that this particular poignant awareness should form the subject of one of her early investigations.[21] The following year she was co-author of four articles on the affective value of colors,[22] a problem which she continued to investigate for more than a decade.[23]

During the same period, she also studied the affective value of articulate sounds.[24] She suggested as a measure of affective sensativeness (that is, the tendency to be strongly affected both in the direction of pleasantness and in that of unpleasantness) the ratio of the sum of the number of judgments of extreme pleasantness and extreme unpleasantness to the number of judgments of indifference.[25]

In 1925, she tried this method out on 'poets' and 'scientists.'[26] The

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[20] Wundtian feeling analysis and the genetic significance of feeling, *Philos. Rev.*, 14, 1905, 21-29; The term feeling, *J. Philos., Psychol., & Sci. Methods*, 3, 1906, 62-63.

[21] M. F. Washburn and A. H. Taylor, The sources of the affective reaction to fallacies, this JOURNAL, 21, 1910, 157-161.

[22] M. F. Washburn, E. L. Norris, and A. G. Twiss, An effect of fatigue on judgment of the affective value of colors, this JOURNAL, 22, 1911, 112-114; M. F. Washburn, A note on the affective value of colors, *ibid.*, 22, 1911, 114-115; M. F. Washburn, D. Clark, and M. S. Goodell, The effect of area on the pleasantness of colors, *ibid.*, 22, 1911, 578-579; and M. F. Washburn and D. Crawford, Fluctuations in the affective value of colors during fixation for one minute, *ibid.*, 22, 1911, 579-582.

[23] M. F. Washburn and I. Powelson, The effect of verbal suggestion on judgments of the affective value of colors, *ibid.*, 24, 1913, 267-269; M. F. Washburn, M. Babbitt, and M. Woods, Affective sensitiveness to colors, tone intervals, and articulate sounds, *ibid.*, 26, 1915, '289-291; M. F. Washburn, H. Robbins, and D. Smith, The influence of fatigue on affective sensitiveness to colors, *ibid.*, 26, 1915, 291; M. F. Washburn, D. Haight, and J. Regensburg. The relation of the pleasantness of color combinations to that of the colors seen singly, *ibid.*, 32, 1921, 145-146; M. F. Washburn and S. L. Gross, Voluntary control of likes and dislikes; the effect of an attempt voluntarily to change the affective value of colors, *ibid.*, 32, 1921, 284-289; and M. F. Washburn, M. T. MacDonald, and D. Van Alstyne, Voluntary controlled likes and dislikes of color combinations, *ibid.*, 32, 1922, 426-428.

[24] M. F. Washburn and L. Roblee, The affective value of articulate sounds, *ibid.*, 23, 1912, 579-583; and M. F. Washburn, L. Garver, and J. M. Gleason, The source of affective reaction to articulate sounds, *ibid.*, 26, 1915, 292-295.

[25] M. F. Washburn, H. Clark, and N. Quackenbush, A suggested coefficient of affective sensitiveness, this JOURNAL, 24, 1913, 583-585.

[26] M. F. Washburn, E. Hatt, and E. B. Holt, Affective sensitiveness in poets and in scientific students, *ibid.*, 34, 1925, 105-106.

'poets' were 33 undergraduate students recommended by the English Department of Vassar as having distinct poetic gift. With characteristic humor, Miss Washburn observed that they were "not all budding Shelleys, but recognized by their instructors as having some measure of real gift for poetic expression." The 'scientists' were 34 students who were majoring in science. The 'poets,' as might be expected, showed higher affective sensitiveness than the 'scientists,' indicating that her technique of measurement might have practical value in investigating an individual's bent.

She did not take the obvious next step of standardizing and publishing a test for selecting embryo poets and scientists. Her mind was so creative, her interests so varied, her theoretical preoccupations so numerous, that she simply could not spend the time to standardize a test sufficiently for practical purposes. She left that to others—she was content to throw out an incredible number of stimulating suggestions.

She attempted to measure temperamental characteristics of different individuals by comparing the average reaction time required to recall pleasant experiences associated with stimulus-words with the time required to recall unpleasant experiences.[27] Friends of the experimental subjects confirmed the validity of estimates secured by her tests.

Though these experiments were undertaken primarily as a contribution to individual psychology, Miss Washburn discovered in the data much that was significant for general psychology, particularly social psychology. She found that it took slightly longer to recall unpleasant experiences than pleasant ones, that physical unpleasantness was more readily recalled than mental unpleasantness; and mental pleasantness than physical pleasantness. She explained these findings by several hypotheses, the most important being that mental unpleasantness is likely to be involved with complexes and hence suppressed, and that convention regards physical pleasures as unworthy and undignified.

In the years that followed, she and her students did a vast amount of experimentation with revived emotions and their meaning for the problems of mood and temperament.[28]

Those who heard her at the Wittenberg symposium in 1927 will never forget her brilliant solution of the dilemma of emotion and thought. Her central thesis was that emotion interferes with thought only when the movements made in emotion are incompatible with the movements and attitudes essential to thinking.[29]

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[27] M. F. Baxter, K. Yamada, and M. F. Washburn, Directed recall of pleasant and unpleasant experiences, this JOURNAL, 28, 1917, 151-155.

[28] M. F. Washburn, R. Field, and E. D. Wolf, A study of revived emotions, this JOURNAL, 34, 1923, 99-103; M. F. Washburn, D. Deyo, and D. Marks, A further study of revived emotions, *ibid.*, 35, 1924, 113-120: M. F. Washburn, E. Morgan, and H. K. Mull, An attempt to test moods or temperaments of cheerfulness and depression by directed recall of emotionally toned experiences, *ibid.*, 30, 1919, 302-

Indeed, ever since its inception, her motor theory of consciousness had illuminated nearly every area she touched including her earliest fields of investigation—space perception and imagery. In 1909, she showed that retinal rivalry in the after-image could best be interpreted on the hypothesis that "motor processes condition rivalry."[30]

In 1931, she found a motor explanation for fluctuations of perspective.[31] In 1933, she called attention to the importance of retinal rivalry in stereoscopic vision, both in the original impression and in the after-image.[32] Shortly before her retirement in 1937, she collaborated with F. H. Verhoeff in developing a new theory of binocular vision.[33]

Besides her major research and theoretical interests which she pursued consistently through the years, Miss Washburn experimented with innumerable problems that to her were incidental but have subsequently attained prominence in the hands of other investigators. Had her philosophical orientation been different, she might easily have been hailed as one of the pioneers of E.S.P. In 1908, under the modest title "A Study in Guessing,"[34] she published data showing that certain subjects, prevented from actually reading the letters on certain cards, were able to guess them

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304; M. F. Washburn, A questionary study of certain national differences in emotional traits, *J. Comp. Psychol.*, 3, 1923, 413-430; M. F. Washburn, L. Harding, H. Simons, and D. Tomlinson, Further experiments on directed recall as a test of cheerful and depressed temperaments, this JOURNAL, 36, 1925, 454-456; M. F. Washburn, F. Giang, M. Ives, and M. Pollock, Memory revival of emotions as a test of emotional and phlegmatic temperaments, *ibid.*, 36, 1925, 456-459; M. F. Washburn and F. Pisek, Galvanometer experiments with revived emotions as a test of emotional and phlegmatic temperaments, *ibid.*, 36, 1925, 459-461; M. F. Washburn, M. E. Booth, E. Glicksman, and S. Strocker, A comparison of directed and free recalls of pleasant and unpleasant experiences as tests of cheerful and depressed temperaments, *ibid.*, 37, 1926, 278-280; and M. F. Washburn, J. Rowley, and G. Winter, A further study of revived emotions as related to emotional and calm temperaments, *ibid.*, 37, 1926, 280-281.

[29] Emotion and thought: A motor theory of their relations, Feelings and emotions: *The Wittenberg Symposium*, 1928, 104-115.

[30] A. de Vries and M. F. Washburn, A study of retinal rivalry in the after-image, this JOURNAL, 20, 1909, 131-135.

[31] M. F. Washburn, H. Malley, and A. Naylor, The influence of the size of an outline cube on the fluctuations of its perspective, *ibid.*, 43, 1931, 484-489.

[32] M. F. Washburn, Retinal rivalry as a neglected factor in stereoscopic vision, *Proc. Nat. Acad. Sci.*, 19, 1933, 773-777; M. F. Washburn and A. Gillette, Motor factors in voluntary control of cube perspective fluctuations and retinal rivalry fluctuations, this JOURNAL, 45, 1933, 315-319; and M. F. Washburn and D. L. Smith, Stereoscopic binocular fusion in the original impression and in the after-image, *ibid.*, 45, 1933, 320-321.

[33] M. F. Washburn and F. H. Verhoeff, A new theory of binocular vision, *Arch. Ophthal.*, 15, 1936, 1117-1118.

[34] M. Stroh, A. M. Shaw, and M. F. Washburn, A study in guessing, this JOURNAL, 19, 1908, 243-245.

more frequently than would be predictable by chance, but finding no adequate explanation within the framework of scientific psychology, she was content to report her data unexplained.

Though her interest was primarily in pure science, she was not unmindful of the responsibility of the psychologist to contribute to an understanding of student personnel. She studied the interests, talents, and difficulties of Vassar undergraduates by a wide variety of methods, including the questionary. Mindful of the pitfalls of questionaries, Professor Washburn confined hers to simple concrete items "readily accessible to untrained introspection, and not likely to be taken as a joke by the irreverent undergraduate."[35] She compared test-results with academic marks, teacher's estimates,[36] reports that the girls gave concerning their likes and dislikes, their work methods, etc. She found a large number of individuals who said that they disliked mathematics and liked to write. The chances proved to be about even that a person who liked mathematics would like to write and that one who disliked writing would like mathematics. Among the unexpected results of her inquiry was the fact that it revealed no correlation between an interest in science and a tendency to observe one's surroundings.[37]

Her early studies of vision, imagery, and association formed an excellent background for an investigation of the difficulties of poor spellers. The English Department of Vassar College had the custom of selecting conspicuously bad spellers for special training. Miss Washburn investigated the psychological characteristics of this selected group in comparison with an equal number of good spellers. Both groups were given tests of visual-verbal and auditory-verbal memory, and also tests of ability to recall nonsense figures composed of straight lines, and a "Reading Backwards Test." Both groups of subjects proved nearly equal in memory for verbal material, whether it was presented visually or in auditory-motor terms, but the good spellers proved to be decidedly more accurate in recalling visual material that could not be translated into words and they were also superior in the speed with which words presented visually could be associated with auditory-motor terms.[38]

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[35] S. D. White, S. May, and M. F. Washburn, A study of freshmen, this JOURNAL, 28, 1917, 152.

[36] M. F. Washburn, H. Baum, and M. Litchfield, The results of certain standard mental tests as related to the academic records of college seniors, *ibid.*, 30, 1919, 307-310; and M. F. Washburn, A note on the Terman Superior Adult Tests as applied to Vassar freshmen, *ibid.*, 30, 1919, 310.

[37] M. Montague, M. M. Reynolds, and M. F. Washburn, A further study of freshmen, *ibid.*, 29, 1918, 327-330.

[38] A. Howell, L. Hopson, and M. F. Washburn, Accuracy of visual memory and

Miss Washburn developed her tests for the control of visual imagery into a group test which she gave to two groups of Vassar College freshmen, whose natural ability in geometry had been estimated by their instructor. By computing rank difference coefficients, she showed that there is good correlation between the teacher's estimate of natural ability in geometry and the control of visual imagery as measured objectively by the test.[39]

In Miss Washburn herself, "ejective consciousness" was developed to an unusual degree. It was characteristic of her power to recognize the capacities and insights of other people that she regularly utilized the judgment of her colleagues in other departments when she wished to select as experimental subjects groups of students specially gifted in a particular field, such as poetry, science, or mathematics. Though she created a few psychological tests and experimented with many others, she never forgot for a moment that the validity of all tests rests ultimately on the subjective judgments of persons. Fully acknowledging the value of tests for quantitative and comparative purposes, she herself was more interested in the subjective judgements that antedate and validate them.

Her experiments on absolute judgments of character traits in self and in others,[40] showed that observers tended to underestimate their own carefulness, industry, and orderliness, and a majority underestimated their own punctuality, enthusiasm, patience, and thrift, whereas many overestimated their own impulsiveness and aggressiveness. She concluded that there are two important influences affecting the relation between absolute judgments of the self and others.

> "The first relates to whether the trait judged is one that is habitually repressed in society. People do not give free social expression, as a rule, to their pessimism, their impatience, their impulsiveness, and their aggressiveness. Hence, as our results show, when judging themselves absolutely they regard themselves as more pessimistic, more impatient, more impulsive and more aggressive than their companions think them. Secondly, traits like carefulness, punctuality, industry, and orderliness refer to an ideal standard, which will be higher in proportion as a person's performance judged by others is more excellent. An industrious, orderly, and punctual person is not satisfied with his own performance. Hence our results show a tendency to underestimate one's own possession of these traits when one's own judgments are compared with those of one's companions on oneself."

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speed of verbal perception in poor spellers, *ibid.*, 28, 1917, 157-158; M. E. Cobb, M. Kincaid, and M. F. Washburn, Further tests of the verbal ability of poor spellers, *ibid.*, 29, 1918, 331-332.

[39] M. F. Washburn, E. Hatt, and E. B. Holt, The correlation of a test of visual imagery with estimated geometrical ability, this JOURNAL, 34, 1925, 103-105.

[40] M. F. Washburn and V. Stepanova, Absolute judgments of character traits in self and others, *ibid.*, 34, 1925, 96-99.

After more than a generation of teaching, Miss Washburn still seemed a comparatively young woman in her alertness, vivacity, social charm, and above all, her continued zeal for research.

At the end of her busy career, the problems of students were of more concern to her than in the earlier years. Her last publication dealt with mathematical ability, reasoning, and academic standing. It appeared in the Golden Jubilee volume of this JOURNAL.[41]

She found that high academic standing does not necessarily guarantee good reasoning ability but good reasoning ability is an excellent guarantee of high academic standing. Good mathematical ability does not guarantee good reasoning ability as much as good reasoning guarantees good mathematical ability. According to the standard set by her test, it is harder to be good in reasoning than in mathematics. She points out that the process of reasoning with nonmathematical, nonquantitative relations is more likely to be interfered with by preconceived ideas extraneous to the reasoning itself. If her health had been spared a little longer, her penetrating insight and her creative capacity would almost certainly have drawn from such studies as these, educational conclusions of far-reaching significance. This study was made with the coöperation of Miss Virginia Pearce and Miss Katherine Payne. Before its theoretical and practical implications could be fully elaborated, Professor Washburn was already in her last illness. Dr. Polyxenie Kambouropoulou transcribed her notes.

In the hands of the students she taught and the colleagues she inspired, her work will go on.

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[41] Mathematical ability, reasoning, and academic standing, this JOURNAL, 50, 1937, 484-488.

A BIBLIOGRAPHY OF THE WRITINGS OF MARGARET FLOY WASHBURN: 1928–1939

By POLYXENIE KAMBOUROPOULOU, Vassar College

The list below brings up to date the bibliography of Margaret Floy Washburn which was begun by Mull.[1] For the sake of consistency, Mull's arrangement and numbering are followed here.

I. BOOKS

(8) *The Animal Mind: A Text-Book of Comparative Psychology.* Fourth Revised Edition. New York, Macmillan Co., 1936. Pp. xii, 536.

II. EDITORIAL

(5) Advisory Editor. *The Psychological Review.* From January 1916 (Vol. 23, No. 1) through May 1930 (Vol. 37, No. 3).

(6) Associate Editor. *The Journal of Comparative Psychology.* From February 1921 (Vol. 1, No. 1) through December 1935 (Vol. 20, No. 3).

(7) Co-editor. (With K. M. Dallenbach, M. Bentley, and E. G. Boring.) *The American Journal of Psychology.* From January 1926 (Vol. 37, No. 1) through October 1939 (Vol. 52, No. 4).

(8) Member of Advisory Board. *Dictionary of Psychology,* edited by Howard C. Warren, 1934.

III. ARTICLES[2]

(108) Purposive action (Address of the vice-president and chairman of Section I—Psychology—American Association for the Advancement of Science, Nashville, December, 1927), *Science,* 67, 1928, (no. 1724), 24-28.

(109) Emotion and thought: A motor theory of their relations, in *Feelings and Emotions: The Wittenberg Symposium,* edited by Martin L. Reymert, 1928, 104-115.

(110) (With K. Keeler, K. B. New, and F. M. Parshall) Experiments on the relation of reaction-time, cube fluctuations and mirror drawing to temperamental differences, *Amer. J. Psychol.,* 41, 1929, 112-117.

(111) Energy, engines and the engineer: a critique of C. Spearman, *Amer. J. Psychol.,* 41, 1929, 322-326.

(112) The study of pure science, *Vassar Quarterly,* 14, 1929, 88-89.

(113) (With E. Hughes, C. Stewart, and G. Sligh) Reaction-time, flicker, and affective sensitiveness as tests of extraversion and introversion, *Amer. J. Psychol.,* 42, 1930, 412-413.

(114) (With R. Ebersbach) The effects of the direction of initial pathways on the orientation of white mice in a maze, *Amer. J. Psychol.,* 42, 1930, 413-414.

(115) (With E. Jacobs and M. Mackenzie) The effect on orientation in the circular maze of the presence or absence of food at the goal during running, *Amer. J. Psychol.,* 42, 1930, 414.

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[1] H. K. Mull, A bibliography of the writings of Margaret Floy Washburn, this JOURNAL, 39, 1927, 428-436.

[2] In the complete bibliography of the writings of Margaret Floy Washburn sixty-eight of the articles published in this JOURNAL are studies from the Psychological Laboratory of Vassar College.

(116) Abstracts of four papers: Köhler's discussion of association; The nature of *g;* Some tests of introversion and extraversion; Orientation and purpose in learning, *Proc. Ninth Inter. Cong. Psychol.,* 1930, 470-472.

(117) A system of motor psychology, *Psychologies of 1930,* 1930, 81-94.

(118) (With H. Mallay and A. Naylor) The influence of the size of an outline cube on the fluctuations of its perspective, *Amer. J. Psychol.,* 43, 1931, 484-489.

(119) (With E. L. Collens and E. Upjohn) The relation to hunger and activity drives of the factor of habituation to the maze, *J. Comp. Psychol.,* 12, 1931, 421-427.

(120) (With Marjorie Broer) The relation of hunger and activity drives in maze running by mice to the factor of habituation, II, *J. Comp. Psychol.,* 13, 1932, 403-407.

(121) Ejective consciousness as a fundamental factor in social psychology (Address given by the honorary President at the meeting of the New York Branch of the American Psychological Association at Philadelphia, April 9, 1932), *Psychol. Rev.,* 39, 1932, 395-402.

(122) Some recollections, *A History of Psychology in Autobiography,* 2, 1932, 333-358.

(123) (With Annette Gillette) Motor factors in voluntary control of cube perspective fluctuations and retinal rivalry fluctuations, *Amer. J. Psychol.,* 45, 1933, 315-319.

(124) (With Dorothy Lucile Smith) Stereoscopic binocular fusion in the original impression and in the negative after-image, *Amer. J. Psychol.,* 45, 1933, 320-321.

(125) Retinal rivalry as a neglected factor in stereoscopic vision, *Proc. Nat. Acad. Sci.,* 19, 1933, 773-777.

(126) Howard Crosby Warren, *Science,* 79, 1934, (no. 2038), 48.

(127) (With Priscilla Manning) Retinal rivalry in free vision of a solid object, *Amer. J. Psychol.,* 46, 1934, 632-633.

(128) (With C. Faison and R. Scott) A comparison between the Miles A-B-C method and retinal rivalry as tests of ocular dominance, *Amer. J. Psychol.,* 46, 1934, 633-636.

(129) (With C. Reagan and E. Thurston) The comparative controllability of the fluctuations of simple and complex ambiguous perspective figures, *Amer. J. Psychol.,* 46, 1934, 636-638.

(130) (With K. G. McLean and A. Dodge) The effect of area on the pleasantness and unpleasantness of colors, *Amer. J. Psychol.,* 46, 1934, 638-640.

(131) James Mark Baldwin: 1861-1934, *Amer. J. Psychol.,* 47, 1935, 168-169.

(132) Titchener, Edward Bradford, *Dictionary of American Biography,* 18, 1936, 564-565.

(133) Mathematical ability, reasoning, and academic standing, *Amer. J. Psychol.,* 50, 1937, 484-488.

(134) (With Constance Wright) The comparative efficiency of intensity, perspective, and the stereoscopic factor in producing the perception of depth, *Amer. J. Psychol.,* 51, 1938, 151-155.

IV. REVIEWS AND NOTICES

(65) Social Science Research Council's grants-in-aid, *Amer. J. Psychol.,* 41, 1929, 688-689.

(66) Correspondence. A new theory of binocular vision, with F. H. Verhoeff, *Arch. Ophthal.,* 15, 1936, 1117-1118.

THE LIMITS OF TOLERATION FOR SIMULTANEITY IN THE COMPLICATION EXPERIMENT

By CLARENCE D. LEATHERMAN, Johns Hopkins University

The pioneer work on the complication experiment was devoted almost entirely to the investigation of the personal equation[1] of the astronomers and of the theory of prior entry. Subsequent work, with revised apparatus and methods of collecting and treating the data, directed investigators to new aspects of the problem.

Some variations in the complication experiment which were previously ignored or uncontrolled seemed to be important for the ultimate solution of the problem. The apparatus used in the present investigation, facilitated further study of the limits of toleration and possible change in the judged positions of simultaneity. The investigation also includes an analysis of the number and type of errors and a determination of the limits of the errors.

The purpose of this study was to determine (1) the limits of error; (2) the relation of these limits to the characteristic errors of the observer and to the general average error; (3) to test by new methods the 'law' of prior entry; and (4) to discover the influence of a warning signal.

METHOD

Apparatus. The basic apparatus for this research was first used by Dunlap.[2] The purposes of the present problem, however, necessitated numerous changes. The apparatus, shown in Fig. 1, consists of a cast iron base 45 cm. long, 20 cm. wide, and 2 cm. thick which supported two cast iron uprights 26 cm. high and 26 cm. apart. On top of each upright was a brass journal in which a shaft revolved parallel to the base. A dial, 35 cm. in diam., was centered over the front end of the shaft in a fixed position. The dial shown in Fig. 1 was of brass, nickle-plated and highly polished, and etched in 5° intervals which were unlabeled. An index-hand fastened to the shaft rotated just in front of the dial. Two different types of index-hands were used in different parts of the work, each 18 cm. long and of blackened brass.

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[1] F. W. Bessel, Personal equation, *Astronom. Beobacht. Königsberg,* 8, 1823; *Abhandlungen,* 3, 1876, 300-304.

[2] Knight Dunlap, A new complication apparatus, *J. Exper. Psychol.,* 2, 1917, 89-91.

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The first index-hand was tapered gradually for 12 cm. and tipped with a straight piece 2 mm. wide, and 6 cm. long. A second index-hand was tapered gradually to the tip. The dial was enclosed in a shallow rectangular box 66 cm. high, 64 cm. wide, and 11 cm. deep. A circular aperture, 36 cm. in diam. in the front of the box permitted full vision of the dial by the seated *S*.

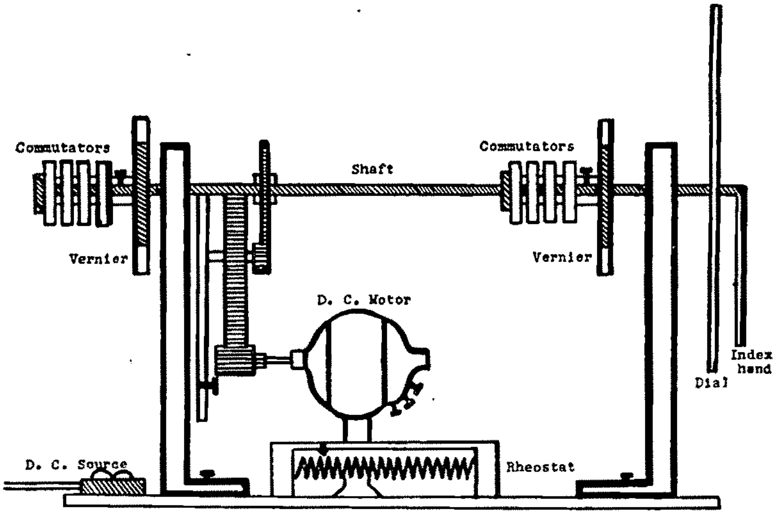

FIG. 1. LATERAL VIEW OF THE APPARATUS

Directly back of the enclosed dial, three adjacent commutators attached to the arm of a circular vernier were centered on a sleeve of the shaft which carried the index-hand. Each commutator was separately adjustable on the sleeve carrying the arm of the vernier by means of a set-screw. The circular scale, which was centered on, and rotated directly with, the shaft, was marked in 10°-intervals from 0°–360°. The arm of the vernier could be locked by a thumb-screw in any desired position with reference to the scale.

Each of the commutators was 1½ cm. wide, and 3 cm. in diam. Each had two brushes bearing on it. Six brass brushes controlling the auditory stimulus were fastened to a bakelite arm which extended from the upright nearest the dial. The commutators were constructed of brass and formica, diametrically divided. Consequently when the brushes touched the brass portion of the commutator the circuit was complete and when the brushes were on formica the circuit was open. Each commutator was insulated from its adjacent one by a thin bushing of bakelite placed between them and centered with them on the shaft.

The auditory stimulus, heard by *S* in two telephone receivers, was a single sharp click of constant intensity. It was secured by interrupting the primary circuit of a small transformer through the commutators. The primary circuit was closed first through the middle commutator; the secondary circuit was then closed through one leg of the telephone circuit by the third commutator. Following this, the other leg of the telephone circuit was closed through the first commutator. The primary circuit was broken while the secondary one was still complete and the latter was then interrupted through successive stages.[3] The intensity of the auditory stimulus was governed by the amount of resistance used in the primary circuit of the transformer. A one-half watt neon bulb proved most satisfactory for this purpose.

A direct-current variable-speed motor was mounted securely on the base which supported the uprights. A series of three reduction gears, connected with the motor, and an adjustable rheostat, in series with the motor, made it possible to secure the different rotation speeds of the index-hand.

The vernier for the flash of the neon tube was mounted on a sleeve and rotated with the shaft directly back of the second upright (see Fig. 1), and was identical with the vernier for the auditory stimulus. The commutators and brushes were also constructed in a similar manner to the commutators and brushes for the telephone click. Only two commutators and two brushes, however, were used in completing the circuit for the flash.[4]

The flash was secured by passing direct current through the commutators which were in series with the electro-magnet of a small telephone relay (Fig. 2). The secondary circuit of the transformer was 5000 volts at 18 ma., and was connected with the neon tube. The telephone relay was placed in the circuit due to an arc, caused by breaking the high voltage of the secondary induced current, which burned the formica on the commutators.

The neon tube encircling the dial was 47 cm. in diam. and was concealed from *S*'s field of vision by the shallow rectangular box described above. The flash illuminated the dial clearly and instantaneously with a red-orange light.

Procedure. In an attempt to maintain constant conditions throughout the experimentation, every *S* was instructed separately as to the nature of the problem, and the functions of the apparatus. The following are the instructions given.

> *Instructions.* This test is designed to determine your skill and accuracy in estimating the position of the pointer with reference to the dial when the auditory stimulus occurs. The auditory stimulus will be a single sharp click in the telephone receivers. While the index-hand is revolving you will hear the stimulus at regular intervals. Make your estimate, if possible, in 8 revolutions of the index-hand, but do not sacrifice accuracy for speed of response. When you have made your judgment, tell the experimenter the number of degrees from zero at which the auditory stimulus and index-hand are simultaneous.

The *S* was seated 3 ft. from the dial which was placed approximately on a level with his eyes. *E* was seated to the right of the apparatus where the control switches and verniers were easily accessible. In experiments under general illumination, two lamps of 75 w. each, situated above and in back of *S*, were used. In the experiments in darkness, a cardboard booth was constructed around *E* and the apparatus. Thus the light needed to operate the switches and verniers, was completely shielded from *S*'s vision. *S* could then see the dial and index-hand only when the neon tube flashed.

Three speeds were used: one revolution of the index-hand in 0.75 sec.; one in

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[3] Thus the break-current was employed in securing the auditory stimulus. The 'make' of the current could easily have been used by adjusting the commutators appropriately, but due to the variability of the make-current it was advisable to use the break-current.

[4] It should be noted that oil and small particles of dust, which collect on the commutators, cause, unless frequently removed, faulty contacts and a poorly defined stimulus.

1.00 sec.; and one in 1.25 sec. Each speed was carefully checked before every experimental period.

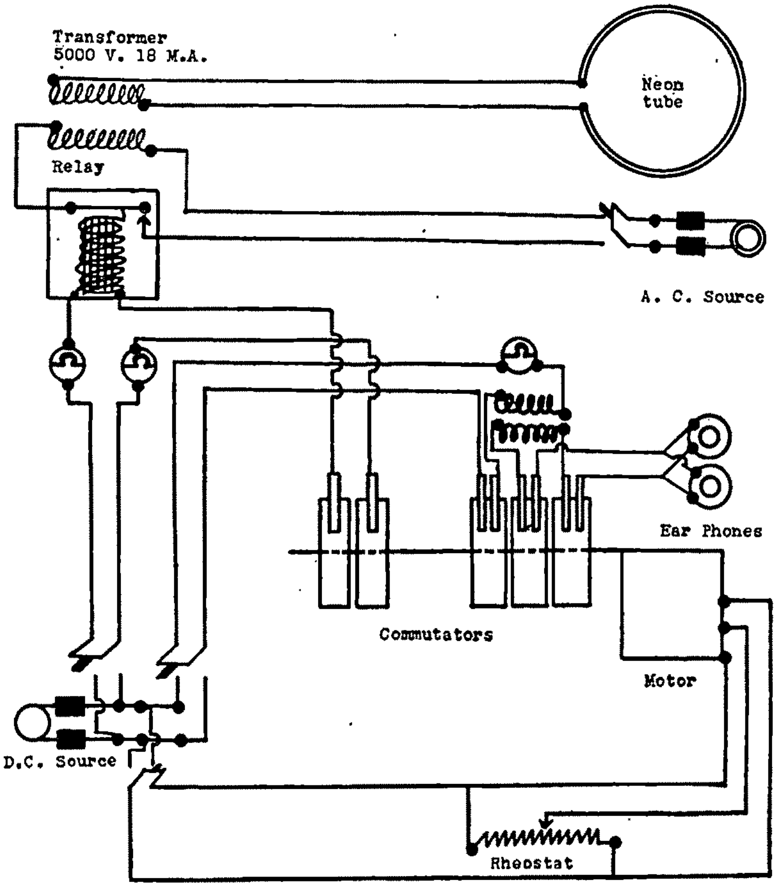

FIG. 2. WIRING DIAGRAM OF THE APPARATUS

The experimental periods lasted one hour during which 36 judgments were secured. Between the 18th and 19th judgments, a 10-min. rest period was given the *Ss*. The *Ss* used in this investigation were undergraduate and graduate students at Johns Hopkins University.

PRELIMINARY EXPERIMENTS

The purpose of the preliminary experiments was to standardize the apparatus and technique and to familiarize *S* with the apparatus; and also, incidentally, to check the results of earlier experimenters.

Every *S* was required to judge the location of the index-hand at a click, while it revolved at the rate of 1 r.p.sec. Judgments were classified as negative, positive, or neutral. We expected, because of earlier findings, that negative errors would predominate.[5]

Judgments were made by the *Ss* on 72 positions of the dial. The order of presentation was planned haphazard and the same plan was used for every *S*. The *Ss* knew that the positions would follow in chance order.

The data for frequency and types of errors for the first series of judgments are listed in Table I. The limits of errors listed in columns 2 and 3 are the maximum negative and maximum positive errors. The average negative and positive errors listed in columns 7 and 9 were obtained by dividing the arithmetic sum of negative or positive errors by the corresponding number of judgments. Total average errors were obtained by dividing the algebraic sum of the errors by the total number of judgments.

The data for each *S* in the preliminary series are given in the horizontal rows. The averages for the group are listed horizontally under their appropriate headings. The limits of errors are merely indicative of the wide range of variation for the individual *Ss*. For example, the maximum errors range from one to four times as large as the average negative or positive errors with but one exception, (*S* 4).

All but one of the *Ss* have a large majority of negative (anticipated) responses; a small number of positive responses, and a still smaller number of correct judgments. *S*1, the exception, conforms to the results of Angell and Pierce,[6] in that the majority of his judgments are positive. Although the average negative, positive, and total average errors are rather large, as compared with those cited in the literature, it should be noted that a large majority of the errors are found within a 20°-limit on either side of actual simultaneity. Some of the earlier experimenters have excluded unusually large errors since they were considered adventitious.

Negative displacements range from 14.6° to 32.2° (41 to 90 ms.) and the positive displacements range from 5° to 15° (13 to 38 ms.). The range for the total or general average spatial displacement is from +2.9° to —25° (+8 to —70 ms.). Limits of error and ranges of error are distinguished from each other in that the limits represent the largest and smallest *single* error whereas the ranges represent the maximum and minimum *average* error.

Table I shows that negative errors, or anticipatory responses, were predominant. The preliminary results, therefore, present the usual picture of the complication experiment.

EFFECT OF SPEED AND TYPE OF STIMULATION

In the second part of the study three rates of rotation were used: one revolution of the index-hand in 0.75 sec.; one in 1.00 sec.; and one in 1.25 sec. Judgments were made for each rate, and for each of the three disparate stimuli: flash, touch, and sound.

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[5] It should be recalled that negative errors were said to be the result of an anticipated response, positive errors the result of a delayed response, and that neutral or zero errors were correct judgments.

[6] J. R. Angell and A. H. Pierce, Experimental research upon phenomena of attention, this JOURNAL, 4, 1892, 528-541.

The flash stimulus was the instantaneous illumination of the dial by the neon tube described above. The intensity of illumination upon the dial was approximately 3-ft. candles. The light-intensity was held constant at this level to insure adequate perception of the flash-stimulus since illumination of greater intensity would have interfered with perception of the flash due to the light adaptation of the eye.

TABLE I

LIMITS, FREQUENCY, AND AVERAGE ERROR WITH SOUND STIMULI
(1.00 sec. per revolution)

	Errors					Av. error				Total av. error	
S	limits		frequency			Deg.	ms.	Deg.	ms.	Deg.	ms.
	−	+	−	0	+	−	−	+	+		
1	50	30	15	13	44	19.0	53	11.3	31	+ 2.92	8
2	90	40	47	12	13	25.2	80	13.5	38	−14.0	39
3	110	45	65	2	5	27.7	77	15.0	42	−24.0	67
4	40	5	62	8	2	16.0	44	5.0	14	−13.9	39
5	35	20	52	12	8	14.6	41	12.5	35	− 9.2	26
6	95	20	60	1	11	32.2	90	11.0	31	−25.0	70
7	120	20	39	4	29	27.8	77	11.6	32	−10.4	29
Av.	77.1	25.7	48.6	7.4	16.0	23.2	66	11.4	32	−17.1	48
S_m						2.5	7	1.2	03	2.6	10

Tactual stimuli were received by *S*s through the diaphragms of two earphones. Every *S* chose his most sensitive finger of each hand (usually the index or the ring finger) and then placed these fingers lightly over the metal disk of the earphones. Adequate stimulation was secured by adjusting the resistance in series with the earphones. The tactual stimulus was the preferred type of stimulation for many of the *S*s. Auditory stimuli were also obtained through the use of earphones.

This series of experiments was conducted to ascertain whether there was an appreciable difference in the spatial or temporal displacement of judgments for the various conditions. Should light produce predominantly positive errors this phenomenon could be explained on the basis of a visual lag; whereas no such lag is known for sound and touch.

Table II contains the results for an auditory stimulus; Table III, for the visual stimulus; and Table IV, for the tactual stimulus.

Auditory experiments. In Table II, the results are shown for 24 judgments for each of the different positions on the dial for every *S*, after a plateau had been reached in the practice series. With a sharp click in the earphones as the auditory stimulus, the individual limits were found to vary in an irregular fashion. If the judgments were made on a temporal basis the individual limits would increase or decrease spatially depending upon the rate employed. If the judgments, however, were made on a basis of spatial displacements alone, the different rates would have little or no effect on the judgments in terms of degrees. Both possibilities are substantiated in part by the results obtained. These results may be explained by shifts in the *S*'s attitude and type of observation.

Negative judgments predominated throughout this series of experiments, although there were a few exceptions for the fastest rate. None of the *S*s showed a dominance of positive judgments with the slowest rate. For a few *S*s (*S*s 3 and 4 for fast rate and *S* 6 for medium rate), the number of correct judgments was greater than the number of negative judgments. The latter cases, however, are atypical and should not be considered as indicative of the general trend of the data.

TABLE II

Limits, Frequency, and Average of Errors with Sound Stimuli

Rate	*S*	Errors					Av. error				Total averages	
		limits		frequency			Deg.	ms.	Deg.	ms.	Deg.	ms.
		−	+	−	0	+	−	−	+	+		
0.75 sec. per revol.	1	30	30	14	9	1	15.0	31	30.0	62	− 7.5	−16
	2	60	0	20	4	0	25.5	53	0	0	−21.3	−44
	3	15	60	3	5	16	3.9	8	25.3	53	+15.6	+32
	4	10	20	3	9	12	8.3	18	10.0	21	+ 4.0	+08
	5	20	0	17	7	0	15.3	32	0	0	−10.8	−23
	6	35	15	7	13	4	12.1	26	7.5	16	− 2.3	− 5
	7	75	30	18	2	4	30.0	62	18.8	39	−15.2	−32
	8	30	5	8	14	2	21.3	44	5.0	10	− 6.7	−14
	9	120	15	9	10	5	46.1	96	11.0	23	−15.0	−31
	10	30	25	14	7	3	16.4	34	18.3	38	− 7.3	−15
1.00 sec. per revol.	1	45	25	10	12	2	18.0	50	17.5	48	− 6.1	−17
	2	45	15	18	5	1	30.0	83	15.0	41	−19.4	−54
	3	60	35	7	8	9	25.0	69	15.0	41	− 1.7	−05
	4	30	20	9	6	9	13.3	37	10.0	28	− 1.3	− 4
	5	30	0	21	3	0	18.3	51	0	0	−16.0	−44
	6	15	5	10	2	12	11.0	30	5.0	14	− 4.2	−12
	7	45	10	18	4	2	20.3	56	7.5	21	−14.6	−40
	8	30	15	9	12	3	21.1	58	8.3	23	− 6.9	−19
	9	75	10	11	9	4	25.9	72	10.0	28	−10.2	−28
	10	20	15	17	6	1	14.1	39	15.0	41	− 9.4	−26
1.25 sec. per revol.	1	15	10	10	13	1	13.5	47	10.0	38	− 5.6	−19
	2	55	0	22	2	0	27.5	95	0	0	−25.2	−87
	3	60	15	14	7	3	35.7	124	11.7	41	−15.2	−53
	4	20	5	18	4	2	11.1	39	5.0	17	− 7.9	−27
	5	30	0	23	1	0	16.5	57	0	0	−15.8	−55
	6	40	15	11	5	8	16.8	58	24.4	85	+ .4	+ 1
	7	45	0	21	3	0	19.8	69	0	0	−17.3	−60
	8	60	5	14	7	3	30.0	104	5.0	17	−12.7	−44
	9	75	0	19	5	0	35.5	123	0	0	−28.1	−98
	10	30	0	19	5	0	16.1	56	0	0	−12.7	−44

The ranges of errors, as shown by the data for the sound stimulus (when all the *S*s were considered with reference to different rates of speed) are rather large and may be accounted for by individual differences.

The *S*s' tendency to modify their negative spatial judgments of the auditory stimulus will be observed in Table II. One *S* (*S* 10) showed a consistent increase in spatial errors with a decrease in speed or rotation; two (*S*s 1 and 8) showed a consistent decrease and seven were inconsistent. Five *S*s (*S*s 1, 3, 7, 9, and 10) showed a decrease in their positive spatial errors with a decrease in rate of rotation; four (*S*s 2, 4, 6, and 8) were inconsistent and one (*S* 5) had no positive errors.

The results for the spatial displacements show evidence of decrease or inconsistency with a decrease in rate. While the data (columns 7, 9, and 11) manifest a decrease or inconsistency in spatial errors, they are not statistically reliable. The temporal displacements will be discussed later when the results for the three sensory stimuli are treated.

TABLE III

Limits, Frequency, and Average of Errors with Visual Stimuli

Rate	S	Errors					Av. eror				Total averages	
		limits		frequency			deg.	ms.	deg.	ms.	deg.	ms.
		−	+	−	0	+	−	−	+	+		
0.75 sec. per revol.	1	30	5	8	15	1	15.6	32	5.0	10	− 5.0	−10
	2	75	25	17	2	5	37.9	79	16.0	33	−23.5	−49
	3	45	75	8	4	12	23.8	49	26.7	56	+ 5.4	+11
	4	25	30	13	6	5	17.7	37	19.0	40	− 5.6	−12
	5	30	0	17	7	0	17.4	36	0	0	−12.3	−26
	6	75	60	13	6	5	29.6	62	19.0	40	−11.7	−24
	7	75	30	15	4	5	35.0	73	21.0	44	−17.5	−36
	8	45	30	9	13	2	26.7	56	17.5	36	− 8.5	−18
	9	105	15	14	7	3	36.8	77	10.0	21	−20.2	−42
	10	10	10	2	15	7	7.5	16	5.7	12	+ 1.0	+ 1
1.00 sec. per revol.	1	15	0	11	13	0	14.5	40	0	0	− 6.7	−19
	2	55	15	18	5	1	31.1	86	15.0	42	−23.3	−64
	3	60	45	18	0	6	27.5	76	19.1	53	−15.8	−44
	4	40	20	14	5	5	20.4	56	15.0	42	− 8.8	−24
	5	30	15	20	3	1	18.0	50	15.0	42	−14.4	−40
	6	80	85	14	5	5	31.4	87	39.0	108	−10.2	−28
	7	50	30	16	3	5	21.3	59	17.0	47	−11.0	−30
	8	45	0	15	9	0	20.7	57	0	0	−12.9	−36
	9	135	105	9	10	5	41.7	115	26.0	72	−10.2	−28
	10	20	10	4	18	2	10.0	28	7.5	21	− 1.0	− 2
1.25 sec. per revol.	1	25	0	14	10	0	15.7	55	0	0	− 9.2	−32
	2	45	0	22	2	0	26.6	92	0	0	−24.4	−85
	3	45	30	12	1	11	26.3	91	21.4	74	− 3.3	−12
	4	30	20	15	4	5	17.3	60	12.0	42	− 8.3	−29
	5	30	0	19	5	0	21.1	73	0	0	−16.7	−58
	6	70	5	19	4	1	25.5	89	5.0	17	−20.0	−69
	7	60	15	19	3	2	31.1	108	12.5	43	−23.5	−81
	8	45	0	18	6	0	22.5	78	0	0	−17.0	−59
	9	70	10	10	9	5	33.0	115	7.5	26	−12.5	−43
	10	15	5	2	18	4	12.5	43	5.0	17	− .2	− 1

Visual experiments. The results shown in Table III for the neon flash-stimulus are almost identical with those obtained when sound was employed as the disparate stimulus. It is interesting to note that *S*s 1 and 10 had a majority of correct judgments. It is also of interest, although not statistically reliable, that the number of negative judgments, in all but two cases (*S*s 9 and 10), increased with a decrease in rate, *i.e.* *S* 1 made 8 negative judgments with the fast rate and 14 negative judgments with the slow rate. An opposite change occurred with the positive judgments in that there was a decrease in the number of positive judgments, as shown by *S*s 1, 2, and 8, with a decrease in the rate of revolution, *i.e.* from fast to slow.

This tendency in the number of correct judgments was not so pronounced, but there was a similar trend shown on the part of *Ss* 1 and 8.

The increase in the number of negative judgments with a decrease in rate may be accounted for by the fact that *S* adopts a specific tempo, faster than the actual rate of stimulation, in attempting to synchronize. If this tempo persists, then the slower the rate of actual stimulation the greater will be the probability of anticipating the stimulus. Increases in negative judgments necessarily decrease the positive, the correct, or both kinds of judgments.

TABLE IV

Limits, Frequency, and Average of Errors with Tactual Stimuli

		Errors					Av. error				Total averages	
Rate	*S*	limits		frequency			deg.	ms.	deg.	ms.	deg.	ms.
		−	+	−	0	+	−	−	+	+		
0.75 sec. per revol.	1	15	5	15	7	2	11.7	24	5.0	10	− 6.9	−14
	2	35	30	13	5	6	21.2	44	15.0	31	− 7.7	−16
	3	35	15	8	2	14	17.5	36	10.7	22	+ .4	+ 1
	4	15	30	4	3	17	10.0	21	11.5	24	+ 6.5	+14
	5	30	0	21	3	0	18.3	38	0	0	−16.0	−33
	6	40	15	15	4	5	21.7	45	13.0	27	−10.8	−23
	7	65	30	14	0	10	32.1	67	17.5	36	−11.5	−24
	8	30	15	22	1	1	19.3	40	15.0	31	−17.1	−36
	9	60	30	12	7	5	31.7	66	18.0	37	−11.3	−24
	10	45	15	20	2	2	20.5	43	15.0	31	−15.8	−33
1.00 sec. per revol.	1	15	15	14	7	3	10.0	28	11.7	32	− 4.4	−12
	2	45	15	15	6	3	19.7	54	15.0	42	−10.4	−29
	3	65	15	9	8	7	23.3	64	12.9	36	− 5.0	−14
	4	30	30	7	1	16	16.4	45	15.9	44	+ 5.8	+16
	5	30	0	20	4	0	18.0	50	0	0	−15.0	−42
	6	35	15	14	5	5	19.3	53	8.0	22	− 9.6	−27
	7	45	15	21	1	2	24.3	67	15.0	42	−20.0	−55
	8	30	15	12	9	3	15.4	43	11.3	31	− 6.9	−19
	9	75	20	19	2	3	25.0	69	16.7	46	−17.7	−49
	10	30	0	21	3	0	14.8	48	0	0	−12.9	−36
1.25 sec. per revol.	1	20	5	16	7	1	10.9	38	5.0	17	− 7.1	−25
	2	35	0	18	6	0	17.0	59	0	0	−12.7	−44
	3	30	10	19	1	4	16.6	58	7.5	26	−11.9	−41
	4	20	20	8	2	14	11.3	39	7.1	25	+ .4	+ 1
	5	30	0	21	3	0	18.6	65	0	0	−16.3	−57
	6	30	15	16	2	6	17.8	62	10.8	38	− 9.2	−32
	7	45	15	21	1	2	21.7	75	15.0	52	−17.7	−61
	8	30	15	18	1	5	16.9	59	12.0	42	−10.2	−35
	9	60	30	18	1	5	23.1	80	19.0	66	−13.3	−46
	10	30	0	21	3	0	16.2	56	0	0	−14.2	−49

The limits of error, based on only one judgment, are used merely to indicate the wide variability of judgments. Inconsistency is demonstrated when the data are compared for the negative, positive and general average spatial errors. With the flash-stimulus, two *Ss* (*Ss* 5 and 10) increased their negative spatial errors with a decrease in rate, one (*S* 2) decreased his errors and the remaining seven *Ss* were inconsistent. For positive judgments, three *Ss* (*Ss* 2, 4, and 7) decreased their spatial errors with a decrease in rate and the remaining seven were inconsistent. The above comparison will be illustrated more clearly when the temporal factor is discussed. The differences mentioned above were statistically unreliable, and even in those cases where a definite trend in the errors is noticed the results are not reliable. Only two cases remained consistent for two rates. *S* 5 had no positive judgments for the fast and slow rates with the flash and *S* 8 had no positive judgments for the medium or slow rate.

Tactual experiments. Table IV presents the data for each of the three rates with the tactual stimulus. The negative and positive limits of error for touch are less than the corresponding limits for flash or sound; the differences are not sufficiently large, however, to warrant further interpretation. A dominant trend is shown in the negative errors both quantitatively and qualitatively. This again is in accordance with earlier findings[7]—Angell and Pierce excepted.[8]

There is a slight increment in the frequency of negative judgments with a decrease in rate; this increment, however, was proven statistically unreliable. *S* 5, an exception, gave 21 negative judgments for both the fast and slow rates. The general trend with a decrease in rate for most *S*s was to manifest a majority of negative judgments, fewer positive judgments, and still fewer correct judgments. *S* 4, the exception in this case, had a majority of positive errors with each of the rates.

The spatial judgments with the tactual stimulus as shown by the data are largely inconsistent. None of the *S*s showed an increase in negative spatial judgments with a decrease in rate; whereas four *S*s (*S*s 2, 6, 7, and 9), showed a decrease and six (*S*s 1, 3, 4, 5, 8, and 10) were inconsistent. No *S* showed a consistent increase or decrease in his positive spatial judgments with a decrease in rate. *S* 5 was consistent in that he had no positive judgments throughout this series.

The ranges of error for the tactual stimulus are less in most cases than the corresponding ranges for flash or sound. There is no statistical significance evidenced for the differences, but they are indicative of a trend toward smaller ranges. These facts will be more clearly illustrated with a consideration of the general averages for all *S*s.

The average limits, frequencies and errors for all of the *S*s with each of the three rates and for each sensory stimulus treated separately, are presented in Table V. The averages are used only to indicate a trend with different conditions of stimulation and with different rates of speed.

The negative limits show a gradual decrement with a change in stimulation in the order of flash, sound, and touch; and the same consistency is evidenced for the positive limits with the exception of touch at the slowest rate. The tendency toward a decrement in the individual limits is not evidenced with a decrease in the rate of speed.

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[7] N. T. Burrow, The determination of the position of a momentary impression in the temporal course of a moving visual impression, *Psychol. Monog.*, 11, 1909, (no. 47), 1-63; Knight Dunlap, The complication experiment and related phenomena, *Psychol. Rev.*, 17, 1910, 157-191; Les mouvements de l'oeil et la simultanéité d'impressions disparates periodiques, *Arch. Psychol.*, 14, 1914, 210-213; Moritz Geiger, Neue Komplikationsversuche, *Philos. Stud.*, 18, 1902, 347-436; Otto Klemm, Versuche mit dem Komplikationspendel nach der Method der Selbsteinstellung, *Psychol. Stud.*, 2, 1907, 324-357; C. D. Pflaum, Neue Untersuchungen über die Zeitverhältniss der Apperception einfacher Sinneseindrücke am Komplikationspendel, *Philos. Stud.*, 15, 1900, 139-148; and Bessel, *op. cit.*

[8] J. R. Angell and A. H. Pierce, *op. cit.*

TABLE V

Average Limits, Frequencies, and Errors for 10 Ss

	Rate	Limits		Number			Minus		Plus		Total	
		−	+	−	0	+	deg.	ms.	deg.	ms.	deg.	ms.
Sound	0.75	42.5	20.0	11.3	8.0	4.7	19.4	40	12.6	26	− 6.7	−14
	S_m						3.9	8	3.4	7	2.3	5
	1.00	39.5	15.0	13.0	6.7	4.3	19.7	55	10.3	29	− 9.0	−25
	S_m						1.9	4	1.7	5	1.9	5
	1.25	43.0	5.0	17.1	5.2	1.7	22.3	7	5.6	20	−14.0	−49
	S_m						2.9	10	2.5	9	2.7	9
Flash	0.75	51.5	28.0	11.6	7.9	4.5	24.8	52	14.0	29	− 8.9	−21
	S_m						3.2	7	2.7	6	2.9	3
	1.00	53.0	32.5	13.9	7.1	3.0	23.7	66	15.4	43	−12.3	−32
	S_m						2.9	10	3.7	10	1.5	5
	1.25	43.5	8.5	15.0	6.2	2.8	23.2	80	6.3	22	−13.7	−47
	S_m						2.1	8	2.3	10	2.6	9
Touch	0.75	37.0	18.5	14.4	3.4	6.2	20.4	42	12.1	25	− 9.0	−19
	S_m						2.3	5	1.8	4	2.2	5
	1.00	40.0	14.0	15.2	4.6	4.2	18.6	52	10.7	30	− 9.6	−17
	S_m						1.5	4	2.0	11	2.4	6
	1.25	33.0	11.0	17.6	2.7	3.7	17.1	59	7.6	27	−11.2	−39
	S_m						1.2	4	2.1	7	1.6	2

Average frequencies of negative errors show a progressive increment with a decrement in the rate of speed. The frequencies of positive and correct judgments show a progressive decrease with a decrease in the speed of rotation. It is interesting to note the opposite change between the number of negative and number of correct and positive judgments. This phenomenon is to be expected, however, since an increase in negative errors with a decrease in rate would necessarily decrease either the positive or correct judgments. Both the correct and positive judgments are affected in this instance.

The negative averages of the judgments for all the *Ss* show a tendency to decrease in spatial errors with a decrease in rate. The same tendency is also evidenced for positive errors with sound and touch but is not true for flash since an inconsistency is shown.

It should be pointed out here that Wundt's contention was "resultant judgments of the observer could not be considered accurate when the index-hand revolved at a speed faster than one revolution in two seconds."[9] The fastest rate employed in the present research was one revolution in 0.75 sec. (1.25 sec. per revolution less than Wundt's fastest rate), and the smallest spatial general averages were obtained in every case with the fastest rate employed (column 11, Table V). These data are in direct opposition to Wundt's statement that a rate of speed greater than one revolution in two seconds produced inaccurate judgments.

This brings us to a comparison which has thus far been purposely omitted from the discussion; namely, spatial *vs.* temporal displacements, *i.e.* degrees *vs.* milliseconds. In the variations, discussed above, the data have been treated with reference to spatial displacement. For example, with the auditory stimulus we found no significant changes when the spatial results were considered, *but* when the same data, after being transposed to milliseconds, were considered, a reliable difference was found between the fast and slow rates for the negative judgments and also for the total average errors.

Table VI presents a condensation of the data given in Tables III, IV, and V, in order to compare more clearly the spatial and temporal errors. This comparison is made for the three rates of speed progressively, *i.e.* from fast through medium to slow for sound, flash, and touch. It is also made between the fast and slow rate. The results under 'deg.' give the number of *S*s who showed a progressive increase or decrease; or who evidenced consistency or inconsistency in spatial errors. The results under "time" give the number of *S*s who showed the above changes when the same data for degrees was transposed to milliseconds.

With the sound stimulus, one *S* (*S* 10, see Table II) showed a consistent increase in his negative spatial errors with a decrease in the speed of rotation; two (*S*s 1 and 8) showed a consistent decrease and the remaining seven were inconsistent. An opposite situation is found with the same data when transposed to milliseconds. Seven of the *S*s (*S*s 2, 3, 4, 5, 6, 7, and 10) showed a consistent increase in magnitude of errors with a decrease in the rate of speed, and three (*S*s 1, 8, and 9) were inconsistent. This trend was not as evident when the positive errors were considered. This may be attributed to the fact that the frequency of positive errors was much less than that of the negative errors. For the general averages, two *S*s (*S*s 8 and 10) increased, one (*S* 1) decreased and seven were inconsistent in their spatial errors with a decrease in the rate but when the same data were considered with reference to changes in milliseconds, eight *S*s (*S*s 1, 2, 4, 5, 7, 8, 9, and 10) showed a progressive increment with a decrement in the rate of rotation and only two *S*s (*S*s 3 and 6) were inconsistent.

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[9] W. Wundt, *Grundzüge der physiologischen Psychologie,* 5 ed., 3, 1903, 70 ff.

TABLE VI

Frequency of Progressive Change in Degrees and Time for 10 Ss

Rates			Negative		Positive		Average	
			deg.	time	deg.	time	deg.	time
0.75 to 1.00 to 1.25 sec.	Sound	Increase	1	7	—	—	2	8
		Decrease	2	—	5	2	1	—
		Inconsistent	7	3	4	7	7	2
		Consistent	—	—	1	1	—	—
	Flash	Increase	2	9	—	—	3	6
		Decrease	1	—	3	—	—	—
		Inconsistent	7	1	7	10	7	4
		Consistent	—	—	—	—	1	—
	Touch	Increase	—	9	—	3	1	6
		Decrease	4	—	—	—	2	—
		Inconsistent	6	1	9	6	7	4
		Consistent	—	—	1	1	—	—
0.75 to 1.25 sec.	Sound	Increase	6	10	1	2	7	9
		Decrease	4	—	6	6	3	1
		Inconsistent	—	—	—	—	—	—
		Consistent	—	—	3	2	—	—
	Flash	Increase	3	10	—	4	7	8
		Decrease	7	—	9	5	3	2
		Inconsistent	—	—	—	—	—	—
		Consistent	—	—	1	1	—	—
	Touch	Increase	1	10	1	7	6	8
		Decrease	9	—	7	2	4	2
		Inconsistent	—	—	—	—	—	—
		Consistent	—	—	2	1	—	—

When *only* the fast and slow rates were compared with reference to spatial and temporal errors, six *S*s (*S*s 2, 3, 4, 5, 6, and 9) increased and four (*S*s 1, 7, 8, and 10) decreased their negative spatial errors with a decrease in speed of rotation. All of the *S*s showed an increase in time errors with a decrease in rate of speed. The general average errors showed a similar tendency to that of the negative errors since seven *S*s increased and three decreased their errors in degrees, while nine *S*s increased and only one (*S* 6) decreased his errors in milliseconds, with a decrease of only 0.5 sec. in the speed of rotation. The positive errors did not show the same relationship but this was attributed to the relatively small number of positive judgments.

With the neon flash as the stimulus the data are directly comparable to the data obtained for sound. Spatial errors in observation evidenced no significant difference between the rates but when the same errors were transposed to milliseconds, the data showed a statistically reliable difference between the fast and slow rates.

Using the touch stimulus the results were still comparable to those obtained with flash and sound. Significant differences were not found for the errors in degrees but when the same data were transposed to milliseconds, statistically reliable differences were shown. One example will suffice to illustrate the contention; the others may be readily seen in Table VI, or from the data directly as given in Table IV. When the extreme rates (0.75 and 1.25 sec. per revolution of the index-hand) are compared, one *S* (*S* 2) showed an increase in his negative spatial errors with a decrease in rate and the remaining nine *S*s showed a consistent decrease. When, however, the data were transposed to milliseconds, *all* of the *S*s showed a statistically reliable *increase* in milliseconds with a decrease of 0.5 sec. per revolution of the index-hand.

The general averages for all *S*s, evidence an increase in spatial errors for a decrease in rate and the same tendency is also manifested for the time values. The spatial errors in judgments are not, however, statistically significant, whereas the temporal increases are.

DIFFERENCE THRESHOLDS AND POINTS OF SIMULTANEITY

The third part of the problem was designed to determine the just noticeable differences for simultaneity of two stimuli. 'Simultaneity' is used here to denote the appearance of the sound and flash stimuli at exactly the same instant. Toleration designates the spatial difference of non-simultaneous stimulations of sound and flash, or vice-versa, which are perceived as simultaneous by the *S*s. Toleration is also designated as the difference threshold, whether it be a negative or positive threshold. Difference thresholds and toleration limits will be used synonymously.

All judgments of simultaneity were secured in the following manner: the *S*s sat before a totally dark dial, and after becoming dark adapted, reacted to sound and flash stimuli. The index-hand revolved as before but was visible only when the neon tube flashed. With the flash stimulus at a constant position, a sound stimulus was introduced either prior to, subsequent to, or simultaneous with the light stimulus. The opposite procedure was also employed, *i.e.* the sound was a constant factor and the flash varied before, after, or simultaneously with the sound. The perception of the flash stimulus and the index-hand are assumed to be identical perceptions temporally for this experiment, the differences being slight enough to be negligible. The index-hand was visible in only one position on the dial and that position although varied, was determined wholly by the neon flash. The rate was one revolution of the index-hand per second.

The *Ss* were asked to report whether the sound was heard before or after the index-hand was seen, or simultaneously with it. Similarly when the sound was a constant factor and the flash was varied. Their judgments were never given in degrees, but simply by the words, "before," "after," or the "same."

Four basic positions for the constant stimulus were chosen, one from each quadrant at the center of its arc, *i.e.* 45°, 135°, 225°, and 315° respectively for each vernier setting. These positions were chosen to diminish or eliminate any predilection on the part of *S* for the cardinal positions on the dial. Disparate stimuli were introduced at six 10°-intervals on the positive and negative sides of actual simultaneity. The 13 positions (6 positive, 6 negative and 1 simultaneous) for each group were presented to the various *Ss* on a chance-order basis, although the same presentation order was used for each subject. The positions were presented so that 1080 judgments of simultaneity (or non-simultaneity) were secured for each of 7 *Ss*.

In addition, the usual complication experiment was employed as a stabilizing factor, since there was a tendency for the *Ss* to become disorientated during the progress of the experiment. The complication-trials were used as a control factor in the following manner: 36 simple complication judgments preceded the first and second series of difference threshold judgments for simultaneity; 36 judgments preceded the third and fourth series; 36 judgments preceded the fifth and sixth series and 36 judgments followed the last two series. The complication data are listed, under the data for difference thresholds.

The difference thresholds for simultaneity were determined from the raw data in four ways and are designated as *A, B, C,* and *D,* in Table VII. This table represents the data obtained for *S* 1 who may be used as a sample of the six other *Ss* used in this series. Each method is presented in degrees and in milliseconds. *A* represents the positive threshold for simultaneity; in other words, any position simultaneous with, or to the right of, the constant position which appears non-simultaneous to the *S*. The criterion for non-simultaneity was 75% of the judgments. *B* represents the negative threshold for simultaneity under the same conditions. *C* represents simultaneity as determined by the point at which *S* gave the same number of positive and negative judgments (taken from the raw data). *D* represents the point of simultaneity obtained by averaging the negative and positive thresholds for non-simultaneity. The difference threshold for simultaneity and the threshold for non-simultaneity are used interchangeably. Each of the thresholds mentioned above was first secured in degrees from the vernier setting and then transposed to milliseconds.

The six variations in procedure (Series 1-6) were used in the following order.

Series 1. The flash appeared regularly at a specified position on the dial, and the sound stimulus was introduced before, after, or simultaneous with the flash, depending upon the procedure. The illumination in the room permitted *S* to see the revolving index-hand after he had become thoroughly dark adapted. This condition will be spoken of henceforth as "some light on the dial." An index-hand with a thin straight tip was used for this series.

Series 2. The conditions of experimentation were similar to those for Series 1, but the sound stimulus was constant and the flash stimulus was varied.

Series 3. Since "some light on the dial" was an unanticipated variation, a shield was constructed which excluded all light from the dial except that of the neon tube. The flash was constant and the sound varied. There was no light on the dial, and the index-hand was the same as in Series 1.

Series 4. Sound was the constant factor and the flash was varied with no light on the dial. Other conditions were similar to Series 3.

Series 5. Flash was the constant factor and the sound was varied before, after, and simultaneous with the visual stimulus. Several *Ss* complained of being unable to see the tip of the first pointer when the flash occurred. This complaint was remedied, by substituting a second index-hand which was tapered gradually to the tip. The data show no unusual variations with the change in index-hands. There was no light on the dial.

Series 6. Sound was the constant factor and the flash stimulus was varied; other conditions were the same as those for Series 5.

The results of *S* 1 show that his positive threshold for simultaneity occurred in Series 1 at +67°, *i.e.* with the flash constant the sound was perceived as non-simultaneous only when it occurred 67° or more after actual simultaneity. His negative threshold was +33°. His point of subjective simultaneity was +50° as obtained from the raw data and it was also +50° as computed from the algebraic average of his negative and positive thresholds.

In Series 2, in which the sound stimulus was constant and the flash varied, both thresholds and his point of subjective simultaneity shifted to the negative side. His positive threshold occurred at —5°, his negative at —37.5°, and his point of subjective simultaneity occurred at —32° from actual experiments and at —21.25° from the algebraic average of the positive and negative thresholds. Similar results were obtained from *S* 1 with each of the remaining variations, *i.e.* Series 3-6. When the sound stimulus was constant and the flash varied, the positive and negative limits of toleration for simultaneity as well as points of subjective simultaneity appeared on the negative side.

One would expect that the negative positions for simultaneity would occur earlier than actual coincidence; *S* 1 in this case proved a consistent exception.

It should be noted here that for purposes of simplification the tables representing the collective data for the other six *Ss* are not included in this publication. Their data in the main were similar to those for *S* 1 given in Table VII. Several exceptions will be pointed out as such.

Two negative thresholds for non-simultaneity were found for S 5 which fell on the positive side. *Ss* 2 and 3 gave positive limits of toleration which were on the negative side of actual simultaneity (sound constant and flash varied). These results are given as exceptions to expectation since the positive threshold for simultaneity appears on the negative side and vice-versa. The other limits for the various *Ss* remained true to expectation in that negative limits were anticipated and positive limits were delayed.

Dunlap stated that limits of simultaneity would be expected to occur symmetrically,[10] *i.e.* if the positive limit for one *S* was 10°, the negative limit would also be 10°, or at least approximately the same. Allowing a 5° deviation for the approximation of symmetry the results of this study show that only 15 of 42 cases show symmetry. (It should be noted that 10 of the 15 cases showing symmetry were found in the results for *Ss* 4 and 6.) These data are given in summary form for all *Ss* at the end of this section.

TABLE VII

DIFFERENCE THRESHOLDS FOR SIMULTANEITY FOR *S* 1

a=positive thresholds for non-simultaneity; b=negative thresholds for simultaneity; c=points of simultaneity from data directly; and d=point of simultaneity (average a and b).

Series	a		b		c		d	
	deg.	ms.	deg.	ms.	deg.	ms.	deg.	ms.
1	+67	186	+33	92	+50	139	+50	139
2	− 5	14	−37.5	104	−32	89	−21.3	59
3	+43	120	+ 9.6	27	+27	75	+26.3	73
4	− 9.3	26	−17.2	48	−16.6	46	−13.3	37
5	+26.3	73	0	0	+21	58	+13.2	37
6	− 1.7	5	−22.5	63	−12	33	−12.1	34

Sound (Control) Rate 1.00

Limits and Average Errors for *S* 1

Series	Limits		Negative		Positive		Total	
	neg.	pos.	deg.	ms.	deg.	ms.	deg.	ms.
1	20	15	8.5	24	7.9	22	−2.5	7
2	35	15	9.3	26	6.4	18	−8.1	23
3	25	20	13.8	38	7.1	20	−7.1	20
4	15	10	11.7	33	8.3	23	−5.6	16

The points of simultaneity as determined by averaging the positive and negative thresholds and also as determined from the data directly, show a marked difference when the stimuli were changed. Series 1, 3, and 5, in which the flash was constant and the sound varied show similar results in every case in that 40 of the 42 points of subjective simultaneity appeared definitely on the positive side. The results of *Ss* 5 and 6 in Series 1 are probably accidental exceptions since their points of subjective simultaneity as obtained from the raw data are found at zero. 'Accidental' has been applied here because their other points of subjective simultaneity are in accordance with the characteristic trend mentioned above. For the same conditions but with the sound constant and the flash varied the point of subjective simultaneity shifted to the negative side with a change in the constant stimulus. There are a few exceptions but they are to be expected because of individual differences or perhaps because of shifts of attention on the part of the *Ss*.

Two explanations may be offered for the changes in the point of subjective simultaneity. The *Ss'* attention evidently was primarily directed to the perception of the index-hand when the flash was constant. They were aware of the fact that the index-hand would be seen at the same position every time and as a result tended to wait for the visual perception. Physiologically the perception of the visual stimulus would be expected later than the auditory perception since the latent time for light is, theoretically at least, greater than that for sound.

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[10] Dunlap, The shortest perceptible time-interval between two flashes of light, *Psychol. Rev.*, 22, 1915, 226-250.

When the sound was constant the flash varied (Series 2, 4, and 6), *S* was obliged to locate the sound in space, *i.e.* on an invisible dial, or to rhythmically synchronize the two stimuli. Since locating the sound on an invisible dial was almost an impossibility, synchronization was probably attempted. This would result in anticipated simultaneity as has been shown by all of the previous experimenters, excluding Angell and Pierce. Negative simultaneity points in the synchronizing reaction may be explained by *S* making a judgment at the time the sound occurs without waiting for the flash. Positive simultaneity points may be accounted for either on a basis of the difference in time-lag between the two stimuli or the time required for judgment after the flash occurred. The latter explanation is substantiated by the milliseconds listed for *S* 1.

Dunlap states that simultaneous stimuli 'were always' perceived as simultaneous and further that those stimuli which were not simultaneous were perceived as simultaneous, providing the temporal difference was not too great.[11] The points of subjective simultaneity as shown by the data for the difference thresholds give partial evidence to the contrary. Some *Ss* do not usually perceive simultaneous stimulations as such. Furthermore, the *Ss* used in the third section of this investigation, almost never judged actual simultaneity correctly. Simultaneity was judged later in a large majority of the cases with the neon-flash constant and anticipated in a majority of the cases when the sound was constant. Additional proof that simultaneous stimuli are not perceived as coincidental was furnished by the raw data. Preliminary experiments showed, however, that when the two stimuli used were presented to *S* first as simultaneous and then one or the other varied for subsequent judgments, the simultaneous stimuli were frequently, but not in a majority of cases, judged as simultaneous.

It might be stated here that the chance-order presentation in the latter experiments placed the simultaneous stimuli in the fifth position. Accurate perceptions of simultaneity should have resulted in simultaneous stimuli being judged as such whether in first or fifth place, but these data give opposite results. Judgments of simultaneity seemed to be definitely influenced by the type of practice immediately preceding such judgments.

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[11] Dunlap, The complication experiment and related phenomena, *Psychol. Rev.*, 17, 1910, 157-191; The shortest perceptible time-interval between two flashes of light, *ibid.*, 22, 1915, 226-250.

It is apparent from the data that non-simultaneously occurring stimuli may be and are almost always perceived as simultaneous when the time discrepancy is not too great. The time errors of difference thresholds for simultaneity may be seen by consulting the columns to the right of the difference thresholds in degrees. The ranges of errors in degrees and milliseconds will illustrate clearly the temporal and spatial discrepancy necessary for the perception of non-simultaneity. Since a difference was evidenced between flash as a constant factor and sound as a constant factor the two stimuli will be dealt with separately.

The ranges for all of the *S*s with the flash constant and the sound varied are as follows.

(a) Positive difference threshold for simultaneity: +75° to +6.6°, or +209 to +19 ms.

(b) Negative difference threshold for simultaneity: +33° to —20°, or +92 to —56 ms.

(c) Point of subjective simultaneity as obtained from the data directly: +50° to 0°, or +139 to 0 ms.

(d) Point of subjective simultaneity as obtained by averaging the positive and negative thresholds algebraically: +50° to —0.6°, or +139 to —2 ms.

In the ranges for the group with auditory stimulus constant, the flash varied (a, b, c, and d, as listed above) as follows.

(a) +15.0° to —9.3°, or +42 to —26 ms.
(b) +2.6° to —45.0° or +7 to —125 ms.
(c) —32.0° to +8.2° or —89 to +23 ms.
(d) —27.5° to +9.1° or —76 to +25 ms.

All the ranges listed above were obtained from the *S*s' averages of 156 judgments for each of the six variations (Series 1-6). Standard errors were not calculated for the results because the judgments were never made in degrees or in time. The results in degrees were obtained by *E* from the corresponding vernier settings and then transposed to milliseconds.

No significant changes were produced in the control complication reactions inserted prior to and subsequent to the other series. The same general trend was evidenced in the frequency of errors as before, *i.e.* negative errors predominated.

EFFECT OF A WARNING SIGNAL ON SUBJECTIVE SIMULTANEITY

The fourth part of the problem was designed to show the influence, if any, of a warning signal upon the characteristic errors in judgment of the several *S*s, and to test the validity of the reaction-time explanation offered by Dunlap.[12]

The experimental conditions were similar to those in the preliminary series with one exception. In addition to the usual auditory stimulus, a second sound was presented regularly to the *S*s. The two stimuli were presented with the same temporal interval between them regardless of the vernier setting. The difference between the two stimuli was 50° or 139 ms.; the latter being representative of reaction-time for sound. The second stimulus occurred at a position on the dial which corresponded to the vernier setting, and the first sound, therefore, served as a warning signal. The rate employed was one revolution of the index-hand per sec.

TABLE VIII

LIMITS, FREQUENCY, AND AVERAGE OF ERRORS WITH WARNING SIGNAL

(Sound stimulus. Rate 1.00 sec. per revolution)

	Errors					Negative		Positive		Total	
S	limits		frequency			deg.	ms.	deg.	ms.	deg.	ms.
	−	+	−	0	+						
1	45	55	30	13	101	14.2	39	16.5	46	+ 8.6	+24
2	30	45	47	12	85	12.9	36	13.1	36	+ 3.5	+10
3	5	50	1	2	141	5.0	14	23.2	64	+22.7	+63
4	15	30	19	10	115	6.3	18	10.3	29	+ 7.4	+21
5	15	45	17	9	118	7.6	21	13.9	39	+10.6	+29
6	20	35	20	6	118	11.8	33	13.5	38	+ 9.4	+26
7	20	40	16	9	119	9.7	27	10.8	30	+ 7.9	+22
Av.	21.4	47.1	21.4	8.7	113.9	11.2	31	14.8	41	+10.0	+28

The *S*s were instructed as follows: "You will hear two sharp clicks in the telephone receivers. Using the first sound as a warning signal report where the index-hand is, in degrees, when you hear the second sound. Make your judgments as accurate as possible."

Every 5°-etching was presented twice for the *S*s' estimate so that 144 judgments were obtained from every *S*. The positions were chosen haphazardly but the presentation was identical for all the *S*s.

The results showing the limits, frequency, and average errors are given in Table VIII. The limits of spatial errors were obtained in the same fashion as that indicated in the preceding sections. Positive limits of errors were from 10° to 45° greater than negative limits. It should be remembered that the limits are only used to illustrate the extent of a single negative or positive error.

The frequencies of the judgments give an entirely different picture. Contrary to the negative frequencies for a single auditory stimulus, every

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[12] Dunlap, The personal equation and reaction time, *Science*, 57, 1923, (no. 1480), 557-559.

S gave positive results in a large majority of the judgments, when two auditory stimuli were employed. Negative and correct judgments comprised a very small minority, with the exception of *S* 2, who gave 47 negative, and 85 positive judgments. Negative judgments were given by the group in 14.8% of the cases, positive judgments in 79.1% and correct judgments in 6.1%.

The spatial differences between the average negative and average positive errors were rather small. *S* 3 may be considered atypical since his average negative error was 5° and his average positive error was 23.2°. It should be noted, however, that *S* 3 had only one negative error. Other *S*s gave differences which ranged from 0.2° to 6.3° between the average negative and positive errors.

The ranges of error in degrees and milliseconds are as follows: negative errors ranged from 5.0° to 14.2° (14 to 39 ms.); positive errors ranged from 10.3° to 23.2° (29 to 64 ms.) and total average errors ranged from +3.5° to +22.7° (+10 to +63 ms.). Although the ranges listed are not significantly different from those given in the first and second sections, it should be pointed out that the usual negative error which was obtained with a single disparate stimulus, shifted markedly to a positive error when two disparate stimuli were employed in the same modal sense.

Some investigators have contended that practice caused characteristically negative errors to shift in a positive direction or vice-versa, depending upon the *S*.[13] That explanation is not applicable in this instance since all of the *S*s were thoroughly familiar with the experiment, thoroughly practiced, and gave a majority of negative errors with a single stimulus whether it was light, sound, or touch. The same *S*s, however, gave a majority of positive errors with two disparate stimuli in the same modal sense.

One explanation offered earlier by Dunlap for the dominance of positive errors with a warning signal may be made on the basis of rhythmic synchronization.[14] Anticipatory responses or negative errors have been explained as resulting from a tendency to augment the tempo in a rhythmic reaction to a single disparate stimulus. By pursuing the above theory further, we may explain the reason for the majority of positive results obtained when two disparate sound stimuli were employed. The

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[13] Burrow, *op. cit.,* 61 f.; Geiger, *op. cit.,* 435 f.; C. D. Pflaum, *op. cit.;* T. H. Haines, Subjective and objective simultaneity, *Harvard Psychol. Stud.,* 2, 1906, 321 f.; 346 f.

[14] Dunlap, Reactions to rhythmic stimuli with attempt to synchronize, *Psychol. Rev.,* 17, 1910, 399-416.

*S*s were instructed to react to the second auditory stimulus, which under the usual conditions of a single stimulus would have elicited a negative judgment. The warning signal (first auditory stimulus) seemed to regularly disrupt the customary rhythm and the *S*s tended to wait for the second auditory stimulus while attempting to synchronize. Consequently, in an attempt to rhythmically synchronize and yet wait for the second stimulus, the tempo was regularly broken and objective simultaneity occurred at a position prior to their estimate of coincidence. In those cases in which simultaneity was judged before it actually occurred the *S*s more than likely disregarded the warning signal and reacted in their customary manner by inadvertently augmenting the tempo. Correct judgments of simultaneity were the result of exact synchronization whether by accident or skilled response.

Another explanation for the errors may be offered on the basis of Titchener's principle of attention or his explanation of the prior entry theory. According to Titchener the object of attention comes to consciousness more quickly than the object to which we are not attending.[15] With a single auditory stimulus and a revolving index-hand the sound stimulus usually had greater attention value. By the law of prior entry, the perception of the sound stimulus reached the level of consciousness first. The scale marks and the index-hand received secondary attention, so that they were reaching the level of consciousness at some time later than the auditory stimulus. As a result, subjective simultaneity was perceived at some point on the scale preceding the actual sound because either the index-hand or the scale etching had not yet reached the level of consciousness and, therefore, could not be perceived. As a result subjective coincidence was negative.

When two auditory stimuli were employed the opposite effect was produced, *i.e.*, the index-hand had passed the point of objective simultaneity before subjective simultaneity had occurred. The *S*s were instructed to report in degrees where the index-hand was when the second stimulus was heard. Apparent simultaneity was reported at a position subsequent to objective simultaneity (positive errors) in a majority of cases (see Table VIII).

According to the law of prior entry, we may conclude that the warning signal detracted from the attention-value of the second auditory stimulus. Since the *S*s were not *predisposed for* the second stimulus it reached the level of consciousness later than the warning signal and the scale etching for objective simultaneity. Attention was centered on, let us say, the warning signal or the index-hand and its perceived relation to the scale etching. As a result the warning signal, the index-hand and its perceived relation to the scale etching or both reached the level of consciousness before the second auditory stimulus. Subjective simultaneity would occur, therefore, at a scale position subsequent to objective simultaneity.

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[15] E. B. Titchener, *Lectures on the Elementary Psychology of Feeling and Attention*, 1908, 251-260.

A disrupted sequence of stimuli might be the basic reason for the second auditory stimulus losing its attention-value. If, however, the warning signal augmented the attention-value of the index-hand we would still have subjective simultaneity ascribed to a position later than that of objective simultaneity, because the secondary stimulus reached the level of consciousness later than the index-hand, or the warning signal.

The stimulus which was first to reach the level of consciousness (awareness) would of necessity relegate other stimuli present to a later position of importance in consciousness. By simple juxtaposition of the stimulus pattern reaching consciousness first we may explain either type of error, negative or positive depending upon which stimulus pattern obtained prior entry.

While either theory may explain the phenomena in this study, the prior entry theory is not as susceptible to experimental verification, as is the explanation of rhythmic synchronization. It is the writer's belief that rhythmic synchronization with individual differences evidenced by the subjects offers an adequate explanation.

SUMMARY

The investigation presented here was designed primarily to determine the limits of toleration for simultaneity in the complication experiment. The effects of practice on the limits and ranges of errors, the effects of a change in rate as well as the type of sensory stimulation and the effect of a warning signal on the characteristic errors were among the essential problems studied. New apparatus and a new technique for obtaining judgments of the complication phenomena were also included.

Since a new apparatus and technique were employed, the complication experiment was repeated to determine what influence the apparatus itself would have. Further application of the apparatus and technique involved the investigation of the three stimuli and the three rates. No essential differences were found in the spatial errors for the three stimuli or for the three rates. Consequently, any of the rates or stimuli were considered applicable in the determination of limits of toleration for simultaneity.

An instantaneous flash of a neon tube and a clear auditory stimulus were chosen as the stimuli to be used in determining the difference thresholds for simultaneity of sound with flash, or flash with sound. The data were obtained for judgments of light or sound as being prior to, subsequent to, or simultaneous with, the constant stimulus. Judgments were read by the experimenter, in degrees, from the vernier settings and then transposed to milliseconds for purposes of comparison. The *S*s were unable to judge simultaneity in degrees since there was no light on the dial.

Proceeding with the knowledge that a characteristic error, whether negative or positive, persisted for different *S*s, an attempt to reduce or even obliterate the usual errors was introduced. The warning signal was chosen for this purpose because in simple reaction-time experiments a warning signal had the effect of reducing the variation in reaction-time. The warning signal, therefore, was placed 140 ms. prior to the stimulus for judgment. The usual reaction-time to sound is 140 ms.

CONCLUSIONS

(1) Practice tended to decrease the *S*s spatial errors in judging simultaneity, although habituation did not entirely eliminate the errors. Large variations in errors were attributed to individual differences or shifts of attention. Appreciable differences in the errors were not evidenced for the various types of stimulation, *i.e.* flash, sound and touch.

(2) Anticipatory responses predominated throughout with a single sensory stimulus, regardless of the type. The number of negative errors generally increased with a decrease in rate whereas the positive or correct judgments decreased quantitatively with a decrease in rate.

(3) Negative judgments in degrees which were inconsistent or decreased with a decrease in rate showed a consistent increase in milliseconds with a decrease in rate. The spatial errors were statistically unreliable but when the same data were transposed to milliseconds they were found to be statistically significant.

(4) Positive difference thresholds were larger qualitatively than negative thresholds with flash as a constant stimulus and the sound varied. Negative difference thresholds were larger than positive threshold qualitatively with sound constant, and the flash varied. Negative or positive difference thresholds for simultaneity appeared on either side of actual simultaneity, *i.e.* positive thresholds were perceived on the negative side of actual simultaneity and vice-versa. Difference thresholds or limits of toleration for simultaneity were asymmetrical in a large majority of the cases.

(5) Positive points of simultaneity were perceived in a large majority of cases with the flash constant and the sound varied. A similar frequency of negative points was perceived with the sound constant and the flash varied. Actual simultaneity was perceived as non-simultaneous in a majority of the cases.

(6) Positive judgments of simultaneity with flash or sound as the constant stimulus were concluded to result from the faulty direction of attention to the sound stimulus, and a visual lag in the perception of the flash stimulus or both. Negative judgments were concluded to result from an augmented reaction tempo while attempting to synchronize. Correct judgments were possible with either alternative.

(7) Positive judgments were obtained in a large majority of the cases when two sensory stimuli in the same modal sense were employed. These positive errors were attributed to inadvertent shifts of attention and a disrupted method of synchronization, due to the warning signal's prior affect on attention.

CONFIGURATIONAL PROPERTIES CONSIDERED 'GOOD' BY NAÏVE SUBJECTS

By MARIAN B. HUBBELL, Wilson College

The concept of the 'good' configuration has been widely used since Wertheimer proposed it as a descriptive term to indicate the tendency of certain types of grouping to appear more spontaneously than others.[1] Experimenters have noted that in apparent movement, memory changes, and forms at threshold values, configurations tend toward symmetry, closure, and simplicity, and they have implied that these properties characterize all good configurations wherever found.[2] But to answer the question as to what constitute good configurations for the *subjects*, a new approach should be made to the problem; namely, an approach in which the *S*s try to produce good configurations. Analysis of the configurations created by a large number of *S*s should reveal whether or not the properties regarded as good by the configurational experimenters also characterize the productions of *S*s given free rein to make as good configurations as possible.

MATERIALS AND PROCEDURE

Method. The method of production, in which the *S*s are free to change given material in any way, was used in order to give the widest range of possibilities in the results. Thus a great variety of changes were produced, exemplifying goodness of form in many different ways.

Materials. The material consisted of forty geometrical figures, which appear in Fig. 1. A few were adapted from Fehrer's material,[3] and a few from Wulf's,[4] while the large majority were improvised in accordance with the following principles. One half of the figures were symmetrical on one or more axes, and one half were asymmetrical. Eight figures consisting of dots were used, and 32 consisting of lines.

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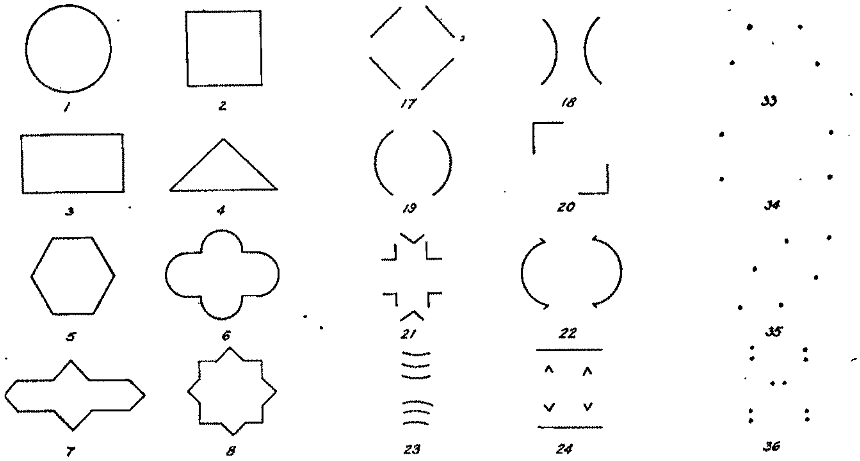
[1] M. Wertheimer, Untersuchungen zur Lehre von der Gestalt, II, *Psychol. Forsch.*, 4, 1923, 301-350.

[2] These experiments may be found in H. Helson, The Psychology of *Gestalt*, this JOURNAL, 36, 1925, 342-370, 494-526; 37, 1926, 25-62, 189-223; and in K. Koffka, *Principles of Gestalt Psychology*, 1935, 129-132, 139-147, 493-506.

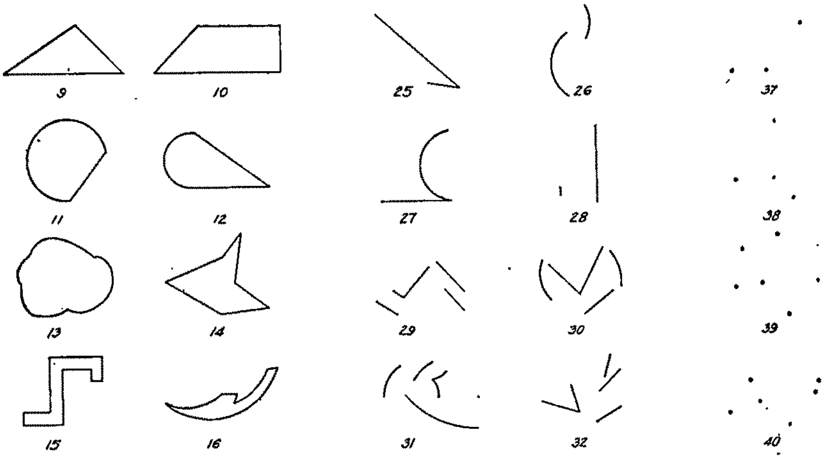
[3] E. V. Fehrer, An investigation of the learning of visually perceived forms, this JOURNAL, 47, 1935, 187-221.

[4] F. Wulf, Beiträge zur Psychologie der Gestalt: VI. Über die Veränderung von Vorstellungen (Gedächtnis und Gestalt), *Psychol. Forsch.*, 1, 1922, 333-373.

Of the latter, half had a closed or continuous outline, the other half consisting of unconnected lines or of one line with one or more changes of direction. While the figures contain a varying number of lines or dots, no figure is so chaotic that



Closed Symmetrical Figures *Open Symmetrical Line Figures* *Open Symmetrical Dot Figures*



Closed Asymmetrical Figures *Open Asymmetrical Line Figures* *Open Asymmetrical Dot Figures*

FIG. 1. THE ORIGINAL FIGURES

it may not be perceived in one glance. The figures were drawn in pencil (in order to allow for erasures) on separate sheets of white paper, 5½ x 8½ in., the longer dimension representing the horizontal as the sheets were presented. The figures were no larger than 2 in. in any dimension. This left adequate space on all sides for additions if desired. The *Ss* were provided with pencils and erasers so that deletions as well as additions could be made.

Subjects. The *Ss* were 50 young women students at Bryn Mawr College, having an average age of 19 yr. All but two were in the first semester of their first course in psychology. The other two had not taken more than an elementary course in psychology. All were ignorant of the problem under investigation.

Procedure. Every *S* was presented individually with a set of the 40 figures in random order, the order differing for each *S*. The sheets were placed face down on the table, with mimeographed instructions, as follows.

> This is an experiment to find out what kinds of figures are most pleasing. When you have finished reading the directions, turn over the pages one at a time. Look at each figure. If you do not think it is as 'good' a figure as it might be in any respect, change it in any way, adding or erasing as much as you like. There is no right or wrong in this experiment. When you have finished the first figure, place it face down before taking up the next. Once you have placed a figure down, do not refer to it again.
>
> You may have as much as 55 min. for the experiment if necessary. But do not pause too long over any of the figures, since we want your first impression.
>
> Please do not talk to anyone about the experiment until after it is over.

The *Ss* were also told that if they liked a figure the way it was given, or thought it good, they were to leave it that way, and were not to feel that they must change all the figures. On the last page appeared the following directions.

> On this page give your reasons for changing the figures as you did. Do not try to give a reason for each separate figure, but give any general principles which you may have followed. Do not refer back to the figures for this.

The *Ss* finished the experiment in varying lengths of time, ranging from 15 to 60 min. Some showed a disposition to change most of the figures painstakingly; others made very few or very slight changes.[5]

RESULTS

(A) Frequency of changes in various types of figures. Since the instructions allowed the *Ss* to make no changes if they so desired, those figures which remained unchanged can be considered good, for the *Ss*, in their original form. Each figure was presented to each of the 50 *Ss*, and of the total of 2000 figures, 1560, or 78% were in some way changed. A consideration of the frequency of changes in the various types of figure reveals striking differences pointing to certain properties considered good and poor.

(1) Figures. The individual figures least frequently altered are given in Table I where it is found that the isosceles triangle and the circle were left unchanged

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[5] Another experiment in which a similar method was used is that of F. H. Lund and A. Anastasi, Aesthetic experience, this JOURNAL, 40, 1928, 434-448. Their *Ss* were asked to complete unfinished figures. But their purpose was to find "the most aesthetic effect," and they do not present their results quantitatively.

by the largest percentage of *Ss*, 72% and 70% respectively, paralleling the finding of Helson and Fehrer[6] that the triangle tended to be seen as a form more frequently than other forms under reduced illumination, and the contention of Köhler[7] that the circle is the best *Gestalt*. The other figures frequently left unchanged, the hexagon, rectangle, and square are likewise closed, symmetrical, simple and regular forms, and confirm the high aesthetic ratings given these figures by Birkhoff's[8] formula for aesthetic measure.

TABLE I

FIGURES LEAST FREQUENTLY CHANGED BY THE Ss

No.	Figure	% Unchanged
4	Isosceles triangle	72
1	Circle	70
5	Hexagon	62
3	Rectangle	62
2	Square	60

At the opposite extreme are figures 28, 29, 37, 38 and 39 (Fig. 1) which were most frequently changed, changed by all but one of the *Ss*.[9] These figures are seen to be open, asymmetrical, and irregular figures. These properties must therefore be considered poor.

TABLE II

PERCENTAGES OF ALL TYPES OF FIGURES CHANGED

Type of figure	Total number*	Percentage changed
Open asymmetrical dot figures	200	98
Open asymmetrical line figures	400	95
Av.	600	96
Open symmetrical dot figures	200	92
Open symmetrical line figures	400	88
Av.	600	89
Closed asymmetrical figures	400	73
Closed symmetrical figures	400	40
All open figures	1200	92
All closed figures	800	56
All asymmetrical figures	1000	87
All symmetrical figures	1000	70
All figures	2000	78

* These values are the product of number of figures by number of Ss (50).

(2) Types. In Table II the percentages of change are given for the various figures, by types. The type of figure showing the highest percentage of changes is

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[6] H. Helson and E. V. Fehrer, The rôle of form in perception, this JOURNAL, 44, 1932, 79-102.

[7] W. Köhler, The problem of form in perception, *Brit. J. Psychol.*, 14, 1923, 262-268.

[8] G. W. Birkhoff, *Aesthetic Measure*, 1933, 16-48.

[9] The same *S* was responsible for the one unchanged figure in all these cases. This S changed none of the figures. One might ask if she really entered into the spirit of the experiment, or if she was totally lacking in esthetic sensibility. In any case, her results were kept.

the group of open asymmetrical dot figures, of which 98% were changed. Combining this group with the open asymmetrical line figures, of which 95% were changed, we find that 96% of all open asymmetrical figures were changed. This indicates clearly that this type of figure is not considered good. While open symmetrical figures are changed in 89% of all cases, this percentage is significantly[10] below (Table III) that for open asymmetrical figures (96%). The figures embodying opposite characteristics, *i.e.* the closed symmetrical figures, are changed least frequently or in only 40% of the cases, while closed asymmetrical figures are changed in 73% of all cases, as is shown in Table II. The difference between these two types of figure is evident from the critical ratio of 9.8 (Table III).

TABLE III

Critical Ratios of the Differences between the Percentages of Change in Various Types of Figure

Type of figure	Critical ratio (Diff./$\sigma_{\text{diff.}}$)
Open asymmetrical dot *vs.* open asymmetrical line figures	1.8
Open symmetrical dot *vs.* open symmetrical line figures	1.4
Open asymmetrical figures *vs.* open symmetrical figures	4.3
Closed asymmetrical figures *vs.* closed symmetrical figures	9.8
All open figures *vs.* all closed figures	18.8
All asymmetrical figures *vs.* all symmetrical figures	9.4

Comparing all closed with all open figures, 56% of the former as against 92% of the latter were changed, a difference of enormous significance, (C.R. 18.8, Table III) both statistically and psychologically. The difference of 17% between the symmetrical and the asymmetrical groups, while not as large, is still a significant one. A consideration of Table II reveals the steadily decreasing proportion of figures changed with an increase in the symmetry and closure of the figures. That the differences in *configurational* properties are significant and not the differences in *components* is shown by the fact that the critical ratios holding between the dot and the line figures are *not* statistically significant (1.8 and 1.4 in Table III).

The results so far show that symmetry and even more strongly closure are considered good properties, as judged by the relatively few changes in these groups. The significance of these properties will be discussed below in connection with the types of changes made.

(B) Consistency and variety in the changes made. In observing the extent to which the *S*s agreed in their changes, we find that in no case were all the changes for a given figure exactly alike. Some of the simple

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[10] Sigma in all cases is found by the formula σ rel $= [pq/n]^{1/2}$ where p is the probability of occurrence, (here the percentage of occurrence in question), q is the probability of non-occurrence, or $1 - p$, and n is the number of cases. The sigma of the difference between two groups is found by the formula $\sigma_{\text{diff}} = [\sigma_1^2 + \sigma_2^2]^{1/2}$. When the difference between the two averages is divided by the sigma of the difference, the critical ratio D/σ_{diff} is found. For a difference to be statistically significant, the critical ratio is here taken as 3. See D. C. Jones, *A First Course in Statistics*, 1924, 159.

dot figures in which many of the *Ss* simply connected the dots with lines, showed the greatest consistency, *e.g.* No. 33 (Fig. 1), in which 21 of the *Ss* did exactly the same thing. On the other hand, No. 26 (Fig. 1) was changed by 48 of the *Ss* in 48 different ways, some of the changes being similar but in no case exactly alike. Though the majority of figures were changed in identical ways only by small groups of *Ss*, and in individual ways by the rest, nevertheless clear-cut principles are found to govern most of the changes. This fact is remarkable in view of the great variety of individual differences, and shows that changes which seem different from an atomistic point of view are actually in the same direction from the viewpoint of configurational properties.

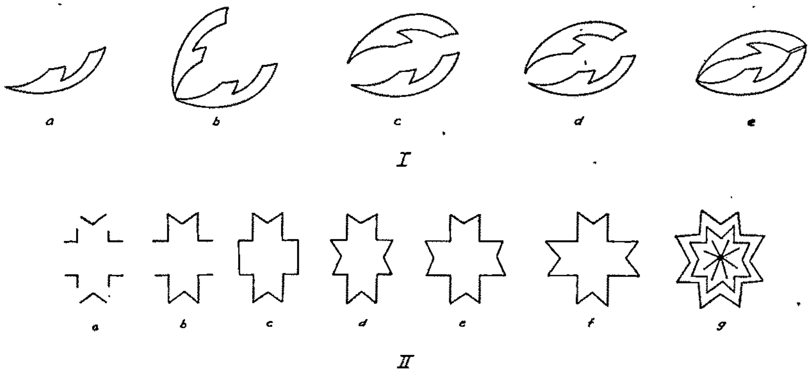

FIG. 2. TWO SERIES OF PROGRESSIVE CHANGES

(1) Series of changes. Interesting is the fact that frequently *Ss*, working independently, produced a group of changes in a figure which seem to be members of a continuous series. Fig. 2 shows two such series, giving the changes of several *Ss*.[11] In the first figure, 4 *Ss* made a mirror image of the original. As we progress from change b to change d the symmetry and closure increase. In the second figure, change b partially closes the figure, change c completes the closure. Change d not only closes the outline but creates some correspondence between the upright and the arms of the cross. Changes e and f increase this correspondence and change g elaborates the whole. It is difficult to realize that these changes are made by *Ss* working independently of one another, for the figure itself seems to be gradually changing to achieve the final goal. Such series recall the findings of Wulf[12] and of Allport,[13] who report progressive changes in the memory trace for forms at successive intervals of time. These are interpreted as evidence for the dynamic character of the trace. In the two examples of Fig. 2, and in others not shown, the principles

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[11] In all the following illustrations, *a* indicates the original figure, *b, c, d, etc.,* the figures as changed by the *Ss*.

[12] *Op. cit.*

[13] G. W. Allport, Change and decay in the visual memory image, *Brit. J. Psychol.,* 21, 1930, 133-148.

of symmetry and closure seem to be operating in varying degrees in the different *Ss.*[14]

(C) Types of changes made. We have next to answer the question: Are there any common properties or principles underlying the changes made in the 40 figures by the 50 *Ss*? The principles found to govern the changes form the most significant results of the experiment.

(1) Greater differentiation and simplification. An important question for the problem of goodness of form is the relative importance of simplicity and complexity. That a good configuration is simple, and that our perceptual and memorial processes tend toward simplification of forms, has been frequently observed, for example by Lindemann,[15] Allport,[16] and Perkins.[17] On the other hand such findings as Fehrer's point to complication of forms under certain conditions.[18] Our results may offer evidence toward a reconciliation of these seemingly opposed facts.

TABLE IV

PERCENTAGES OF FIGURES CHANGED BY THE ADDITION AND BY THE SUBTRACTION OF LINES AND DOTS

Type of change	Percentage
Addition of lines or dots	94
Subtraction of lines or dots	3
Number of lines or dots not affected	3

One approach to the problem is the purely atomistic method of considering the extent to which lines or dots were added to the original figures or subtracted by erasing. The proportions of figures to which parts were added, those from which parts were removed and those in which the changes did not affect the number of lines or dots appear in Table IV. The large majority (94%) of all changes consist in the addition of lines or dots to the given figure, while parts are removed in only 3% of the changes.[19]

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[14] Although N. G. Hanawalt, Memory trace for figures in recall and recognition, *Arch. Psychol.,* 36, 137, (no. 216), 81-82, concludes from his results that configurational changes do not take place in the memory trace, one may suggest that the categories into which he classifies his changes are too atomistic to serve as evidence either for or against changes toward good configuration. He uses, for instance, "added or dropped units or characteristics," "curved line changed to straight," "general outline, shape or relation lost." A reconsideration of his results according to the categories used here would yield very different interpretations, we are sure.

[15] E. Lindemann, Experimentelle, Untersuchungen über das Entstehen und Vergehen von Gestalten, *Psychol. Forsch.,* 2, 1922, 5-60.

[16] *Op. cit.*

[17] F. Perkins, Symmetry in visual recall, this JOURNAL, 44, 1932, 473-490.

[18] *Op. cit.*

[19] In explanation of this difference, it has been suggested that it is less trouble to add lines than to erase them. The fact that none of the original figures was extremely chaotic may also account for the small percentage of subtractions.

The addition of lines and dots, however, does not prove that the figures are made more complicated in such cases. For only a very atomistic view of complication would hold it to be the result of the mere presence of a larger number of parts. A figure of many parts arranged in an orderly manner may give an impression of less complexity than a figure with a smaller number of parts arranged in a haphazard manner, as Fig. 3 well shows. In Fig. 3, eight added lines result in a more complex figure than eleven lines added in c. Certainly c is simpler in appearance than b. The difference seems to depend upon the orderly symmetrical arrangement of parts in c, which gives a unitary appearance. The lines of b, on the other hand, result in so many subdivisions that the figure possesses little unity. Indeed b appears like several figures merely "hitched" together. Thus the *organization* of the added lines may be more important than mere number in determining simplicity or complexity.

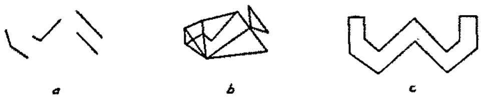

FIG. 3. NUMBER OF LINES AND COMPLEXITY
The addition of 8 lines in b results in greater complexity than the addition of 11 lines in c.

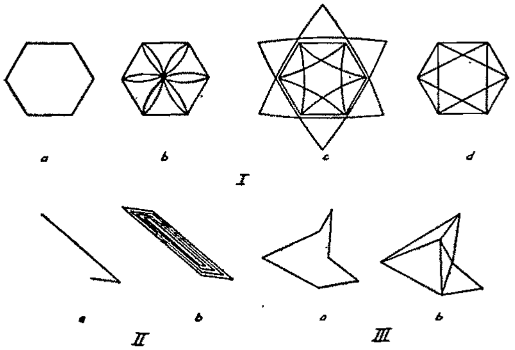

FIG. 4. EXAMPLES OF CHANGES TOWARD GREATER DIFFERENTIATION

(a) Relation of differentiation and simplification. Complication, even when it results in greater number of 'parts' may nevertheless reveal configurational properties. The term differentiation rather than complication is used to apply to changes consisting of the addition of lines within or outside the figure, excluding lines added merely to close the figure. Fig. 4 gives some examples of differentiation in

three different figures. Although lines were sometimes added in a haphazard way, most of the productions resulted in well-articulated yet unified perceptual wholes. For in general the following configurational features characterized the figures showing greater differentiation.

(i) The lines were added at definite points of change of direction in the figure. Even the chaotic example (Fig. 3 b) will be found upon close scrutiny to conform to this rule, as most of the added lines connect the ends of the original lines. No. III b of Figure 4 is another illustration of this rule.

(ii) The lines do not begin or end at points which would break up the natural units of the figure. All the changes of No. I, Fig. 4, preserve the six sides and six points of the original hexagon.

(iii) The added lines frequently converge upon the center of the figure, and emphasize the axes of symmetry. No. I b, Fig. 4, affords a good example of this type of differentiation.

(iv) The added lines often run parallel to the given lines, as they do in No. II b, Fig. 4.

Thus the principal changes resulting in greater differentiation of the figures serve to enhance the whole-qualities of the original figure. One might interpret these changes as exemplifying the law of *Prägnanz* in that certain characteristics of the figure become more fully realized.[20]

Let us look for a moment at the number of changes of this type as they relate to the classes of figure. The results show that 68% of all changes, as indicated in Table V, caused increase of differentiation in the figure—a change occurring more frequently than any other type of change (except the mere addition of lines).

TABLE V

PERCENTAGES OF VARIOUS TYPES OF FIGURES MADE MORE DIFFERENTIATED AND SIMPLIFIED

Type of figure	Percentage of changed figures	
	Differentiated	Simplified
Open asymmetrical	64	44
Open symmetrical	61	40
Closed asymmetrical	72	32
Closed symmetrical	94	9
All open figures	62	42
All closed figures	80	24
All symmetrical figures	69	33
All asymmetrical figures	67	40
All figures	68	37

Closed symmetrical figures are most prone to this type of change, 94% of all changes made in this group being in the direction of greater differentiation (Table V). In general, the closed figures became differentiated in greater proportions than the open figures, and this difference of 17% in favor of the former was found to be significant (C.R., 7.2). The difference between symmetrical and asymmetrical figures in this respect was not significant (C.R., 0.84). Apparently if the outline of a given figure is closed, there is a tendency to add 'interest' by differentiating the figure from within.

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[20] More evidence for the fact that these changes were in accordance with good whole-properties will appear in the following sections on symmetry, closure, and good continuation.

The question of the rôle of complication and simplification cannot be settled until we have considered the extent to which the *Ss* simplified the original figures. Some difficulty was experienced in setting up a rigid criterion for this type of change. Figures were called simplified if the change possessed more unity than the original, as would be the case when isolated dots were brought together (No. I, Fig. 5), or if the figure were given more coherence by greater correspondence between parts (No. II, Fig. 5), or if its general shape were made more simple (No. III, Fig. 5).

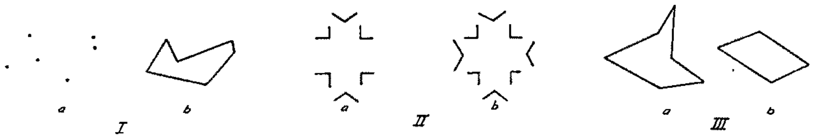

FIG. 5. EXAMPLES OF CHANGES TOWARD SIMPLIFICATION

If, in addition to such changes, there was a noticeable increase in differentiation, this was not counted as simplification, since such lines might possibly be considered to detract from the simplicity (although in many cases they did not seem to). That an increase in the number of lines may produce an appearance of greater simplicity has already been stressed. This explains why the percentage of cases of simplification, shown in Table V, is much larger than the percentage of cases in which parts were subtracted (Table IV). Table V shows that 37% of all changes simplified the figures in accordance with our criterion. This does not imply that the other 63% of changes were in the direction of *decreased* simplicity. On the other hand, they were changes having to do with differentiation, symmetry, closure, etc., often not affecting the property of simplicity. Furthermore, many of the figures not simplified were so simple to begin with that almost nothing could be done to simplify them.

The open asymmetrical figures showed the largest proportion (44%) of such changes, probably because their original irregularities gave more opportunities for creating unity and coherence. The closed symmetrical figures, possibly because they give less possibility for simplification, showed the smallest proportion of changes toward simplification (9%) and the largest percentage of differentiation. The difference of 18% between open and closed figures is very reliable (C.R., 7.2), showing that the former become simplified more frequently than the latter. But the difference between symmetrical and asymmetrical figures in this respect, 7%, is neither as large nor as reliable (C.R., 2.9) as that between closed and unclosed figures.

Our results now make possible a new interpretation of the two tendencies toward simplification and complication. On the one hand, increased differentiation, or articulation, is found to govern 68% of all changes. On the other hand, simplification occurs in 37% of all changes, not including many of the figures left unchanged (such as the square, circle, and triangle) which can be considered simple. These apparently opposed trends have also been reported by other investigators who find both simplification and complication at work in our perceptual and memorial processes. Granit[21] for example found that figures appeared simpler than they actually were when exposed for a fraction of a second. L. Hartmann[22] found that the critical fusion frequency for simple figures was lower than for less simple ones. Allport[23] found a tendency toward simplification in the memory trace. On the other hand, Wohlfahrt's[24] study showed that very small figures sometimes appeared more complicated than the stimuli. Fehrer[25] found that progressive complication of figures occurred in 32%, and progressive simplification in 27% of the reproductions of figures under short exposures.

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[21] A. R. Granit, A study on the perception of form, *Brit. J. Psychol.*, 12, 1921, 223-247.

But need we assume that these two tendencies are directly opposed? Unquestionably, complication, in the sense of added confusion, is the opposite of simplification. But our results show that complication may enhance, rather than destroy the whole-properties of the figure. If the added parts emphasize and elaborate the properties of the figure, as they do in so many, if not all, of our cases of differentiation (See Fig. 4), then complication may increase the unity and coherence of the figure, and so may work toward the same ends as simplification. Since both tendencies may be in the direction of better perceptual units, they are not as directly opposed to each other as might at first seem.

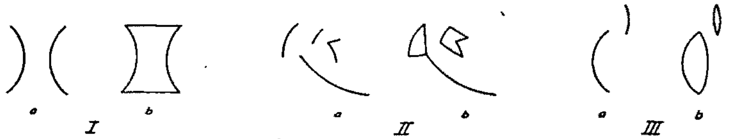

FIG. 6. EXAMPLES OF CHANGES TOWARD INCREASED CLOSURE
I, complete closure of the figure; II, closure of one area of the figure; III, production of two closed figures.

At the same time, the hitherto unemphasized factor of differentiation should be given a place of greater importance as a determinant of goodness of form. The more differentiated figures have the advantage of greater structuration or articulation; too great a simplification of structure, resulting in the lack of discernible parts, is equivalent to no structure at all, and is too uninteresting to be considered good.

(2) Closure. We have already observed (*supra,* Table II) that the open figures are changed much more frequently than the closed. This evidence in favor of closure as a good quality is fully supported by the nature of the changes themselves. It was possible to set up objective criteria for the changes involving closure.

(a) Increased closure. The closure of a figure was increased when (1) a closed or continuous outline was formed from an unclosed figure as in No. I, Fig. 6; (2) a closed section was created in an originally open figure, as in No. II, Fig. 6;

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[22] L. Hartmann, Neue Verschmelzungsprobleme, *Psychol. Forsch.,* 3, 1923, 319-397.

[23] *Op. cit.*

[24] E. Wohlfahrt, Der Auffasungsvorgang an kleinen Gestalten; Ein Beitrag zur Psychologie des Vorgestaltserlebnisses, *Neue Psychol. Studien,* 4, 1932, 347-414.

[25] *Op. cit.*

(3) the given figure was made into two or more separate closed figures, as in No. III, Fig. 6; (4) if dots were connected by lines, even though a continuous outline were not produced. The concept of increased closure could apply only to figures originally open, not of course, to the closed figures.

(b) Emphasized closure. Changes in which the borders of a figure were emphasized, demarcating the figure more sharply from the background,. as in Fig. 7 b.

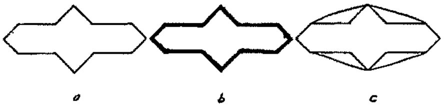

FIG. 7. EXAMPLES OF CHANGES TOWARD EMPHASIZED CLOSURE

seemed clearly related to closure, but since they did not actually increase this property, they were regarded as changes in the direction of emphasized closure. Enclosing the figure in a simpler outline, as in Fig. 7 c, was also considered to emphasize the closure.

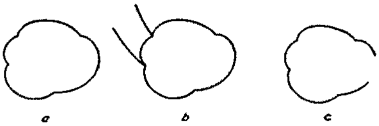

FIG. 8. EXAMPLES OF CHANGES TOWARD DECREASED (b) AND DESTROYED (c) CLOSURE

(c) Decreased closure. If the change brought about a lesser degree of closure than in the original figure, yet left some part of the outline closed, it was called a change in the direction of decreased closure. Most of these changes consisted in the addition of unclosed lines to an originally closed figure as in Fig. 8 b.

(d) Destroyed closure. When a closed outline was opened, leaving a completely open figure, as in Fig. 8 c, such a change was regarded as in the direction of destroyed closure. In open figures, decreased but not destroyed closure could occur, a fact important for Table VI.

(e) Changes not affecting closure. There remained some cases in which the change did not affect the closure of the figure in any way.

The proportions of these changes in the various classes of figure appear in Table VI. The last row indicates that the tendency toward increasing and emphasizing closure is overwhelmingly greater than that toward decreasing or destroying it. Considering the two general trends toward and away from closure, we find that 61% of all changes enhance closure, while only 2% detract from it. Still more striking is the case of the open figures, where 87% of the open symmetrical, and 76% of the open asymmetrical group were changed in the direction of closure. The average of 81% is in striking contrast with the mere 5% of decreased and destroyed closure in closed figures. For theoretically the *S* was just as free to open a closed figure as he was to close an open one, yet he did the latter *sixteen* times more frequently than the former.

TABLE VI

PERCENTAGES OF CHANGES INVOLVING CLOSURE IN THE VARIOUS TYPES OF FIGURE

An asterisk (*) means that occurrence is impossible by definition, whereas 00 indicates that the *S*s might have made a change which was never made.)

Type of figure	Closure				
	Increased	Emphasized	Decreased	Destroyed	Not affected
Closed symmetrical	*	12	4	1	83
Closed asymmetrical	*	8	4	1	87
Open symmetrical	87	*	00	*	13
Open asymmetrical	76	*	00	*	24
All open	81		00		19
All closed	9		5		86
All	58	3	1	1	37
	61		2		

Thus the fact that open figures are most frequently changed, and the extent to which these figures are closed by the *S*s both indicate the importance of closure as a factor in the good configuration. Rosenbloom found that with tactual stimulation, using metal outline figures, open figures were perceived as closed.[26] Similarly hemianopsic patients, reported by Fuchs saw a complete figure when only half or three-quarters of it was in their normal field of vision.[27] Gibson found a similar tendency in his work on memory,[28] *i.e.* fewer and smaller gaps appeared in the memory trace of forms than in the original figures. Closure thus operates both at the lower, automatic level of memory trace and sensory processes, and at the level of conscious activity in the creation of good configurations.

(3) Symmetry. Symmetry is another property frequently mentioned as characteristic of a good configuration. Our results already give evidence that symmetry is an operative factor in that symmetrical figures were left unchanged more frequently than asymmetrical (Table II). In order to find out the extent to which the factor of symmetry governed the changes, the following classifications were used.

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[26] B. Rosenbloom, Configurational perception of tactual stimuli, this JOURNAL, 41, 1929, 87-90.

[27] Koffka, *op. cit.,* 146.

[28] J. J. Gibson, The reproduction of visually perceived forms, *J. Exper. Psychol.,* 12, 1929, 1-39.

(a) Increased symmetry. If one or more axes of symmetry were created in an originally asymmetrical figure, this was considered to be in the direction of increased symmetry. No. I b, Fig. 9 gives an example of a change where mirror-image symmetry is created, No. I c, Fig. 9 of a change where rotational symmetry about a point is produced.[29] Figures originally symmetrical could be changed to have increased symmetry by the creation of additional axes of symmetry, as in No. II b, Fig. 9, where a horizontal axis of symmetry was added, or by the addition of an axis of symmetry in the third dimension, as in No. II c, Fig. 9. No. III b, Fig. 9 shows still another type of increased symmetry, where greater correspondence between parts is made by the addition of a diagonal axis of symmetry.

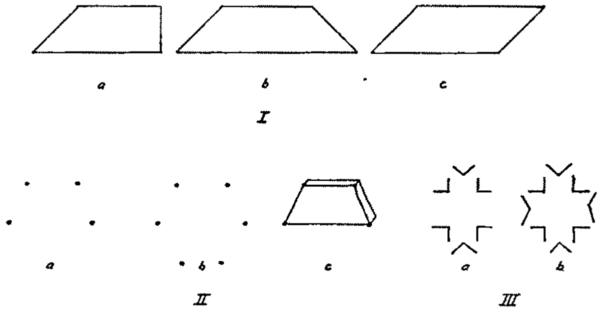

FIG. 9. EXAMPLES OF CHANGES TOWARD INCREASED SYMMETRY
I b, mirror-image symmetry; I c, rotational symmetry; II b, horizontal axis of symmetry; II c, symmetry in the third dimension; III b, diagonal axis of symmetry.

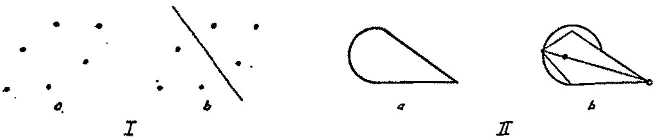

FIG. 10. EXAMPLES OF CHANGES TOWARD EMPHASIZED SYMMETRY
I b, in a symmetrical figure; II b, in an asymmetrical figure.

(b) Emphasized symmetry. Changes which consisted in the addition of lines to emphasize the existing axes of symmetry, as in No. I b, Fig. 10, or in the addition of corresponding symmetrical parts were considered to be in the direction of emphasized symmetry. In some cases symmetry was emphasized in a figure not originally symmetrical, if the change emphasized an aspect in which the figure seemed somewhat although not entirely symmetrical, as in No. II b, Fig. 10.

(c) Decreased symmetry. Changes were regarded as exemplifying decreased symmetry if one or more axes of symmetry were destroyed, while at least one axis

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[29] Rotational symmetry is a type of symmetry described by Birkhoff, *op. cit.,* 17. A polygon possesses this type of symmetry if it may be rotated about a central point through a certain angle and returned as a whole to its initial position.

was left, for example Fig. 11 b, in which the horizontal axis is lost but the vertical remains. Cases in which additional asymmetrical parts were added to asymmetrical figures were also classified as in the direction of decreased symmetry.

(d) Destroyed symmetry. If all the axes of a symmetrical figure were destroyed, this was a change in the direction of destroyed symmetry, an example of which is Fig. 11 c.

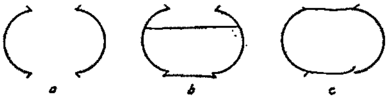

FIG. 11. EXAMPLES OF CHANGES TOWARD DECREASED (B) AND DESTROYED (C) SYMMETRY

(e) Changes not affecting symmetry. There remain cases in which the changes had no effect on the symmetry of the given figure.

The percentages for each type of change in the four main classes of figures appear in Table VII. An outstanding result is that more than twice as many of the changes are in the direction of increased or emphasized symmetry (38%) as in the direction of decreased or destroyed symmetry (17%). It is notable also that only 1% of all changes completely destroy the symmetry. The closed asymmetrical figures were changed toward increased symmetry more frequently than any other type —50% of all cases in this group, with the open asymmetrical group second—39% of all changes. One would expect the asymmetrical figures to have symmetry increased more frequently than symmetrical ones, and

TABLE VII

PERCENTAGES OF CHANGES INVOLVING SYMMETRY IN THE VARIOUS CLASSES OF FIGURE

An asterisk (*) means that occurrence is impossible by definition, whereas "oo" indicates that the Ss might have made a change which was never made.

Type of figure	Symmetry				
	Increased	Emphasized	Decreased	Destroyed	Not affected
Closed symmetrical	9	38	38	1	14
Closed asymmetrical	50	12	10	*	28
Open symmetrical	17	5	21	2	55
Open asymmetrical	39	00	8	*	53
All symmetrical	15	13	25	1	46
	28		26		
All asymmetrical	43	4	9	*	45
	47		9		
All	30	8	16	1	45
	38		17		

the table shows that this is true. But the difference between the open and the closed figures may probably be explained by the fact that *S*s tend to close the open figures. Within the frame of reference of the given figure, closure represents a degree of improvement for an originally open figure, and further improvement in the form of symmetry does not seem to be demanded. With a closed asymmetrical figure, however, the possibility for increased closure is not present, and a change toward increased symmetry is therefore more frequently made. Each improvement is carried out with relation to the frame of reference of the original figure, rather than of the finished product. That the closed symmetrical group shows the highest percentage of emphasized symmetry corroborates the finding that this group had the highest percentage of differentiations, for the drawing of lines to emphasize symmetry constituted one form of differentiation. Taking all asymmetrical figures, we find 47% of all changes increase or emphasize this property. This result, together with the small percentages of decreased and destroyed symmetry, shows that this property, while not absolutely necessary for a good configuration, is nevertheless definitely preferred in a figure. Since the percentages in Table VII do not include cases of occult balance,[30] which might be regarded as a type of symmetry, the case for this property is made even stronger.

Considering briefly the results of other experimenters, we find that irregular shapes, momentarily exposed, appear more symmetrical, according to Lindemann.[31] Wulf reported changes toward symmetry in the memory trace for forms, and called them changes toward a good *Gestalt*.[32] But the fact that symmetry did not occur as frequently in our results as did some other good qualities should cast doubt upon the overemphasis given to symmetry by such writers as Perkins who considered changes toward symmetry the most important in the memory trace.[33]

One reason why symmetry does not play a more important rôle is that it is not always an end in itself, but a means of achieving coherence by creating correspondence between the parts of the whole. It is true that in many forms of decoration, tiles, etc., symmetry prevails. But when other ways of achieving unity are possible, as in the pictorial arts, where exact symmetry is rare, symmetry may be abandoned in favor of more dynamic properties to be discussed presently.

(4) "Good continuation" and dynamic properties. Among the dynamic

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[30] Occult balance means that the parts of a design are not arranged identically, as in symmetry, yet they still show a feeling of equilibrium. See A. and B. Jackson, *The Study of Interior Decoration*, 1928, 10-11.

[31] *Op. cit.*

[32] *Op. cit.*

[33] *Op. cit.*

tendencies operating in the productions of the *Ss*, that known as "good continuation" frequently appeared.[34] Good continuation in a configuration means that one part of the pattern follows out the direction of another part, fulfilling the implications inherent in the latter. Thus figures in which the added lines continue the direction of given lines in the original figure were classified as cases of good continuation. For example, in No. I b, Fig. 12, a continuation of the curve is shown, which also resulted in closure. In No. II b, Fig. 12, continuation of a line (the horizontal) in the direction in which it is pointing is shown, and here symmetry is achieved as well. Good continuation in which a motif is repeated is exemplified by No. III b, Fig. 12. Such cases show how the various principles act together in the determination of a single change.

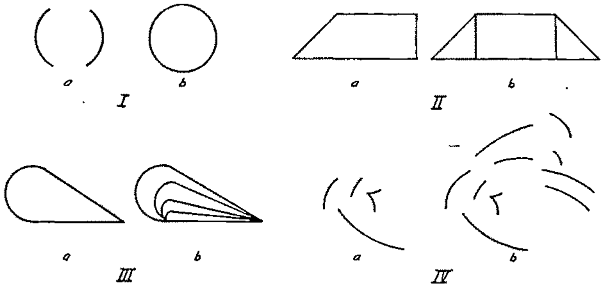

FIG. 12. EXAMPLES OF CHANGES IN THE DIRECTION OF GOOD CONTINUATION

TABLE VIII

PERCENTAGE OF CHANGES IN THE DIRECTION OF GOOD CONTINUATION IN EACH TYPE OF FIGURE

Type of figure	Good continuation
Closed figures	23
Open figures (except dot figures)	27
Symmetrical figures	20
Asymmetrical figures	30
All figures	25

The percentage of changes showing the factor of good continuation for each main type of figure appears in Table VIII.[35] 25% of all changes are in the direction of good continuation. But this should by no means be interpreted as indicating that the remaining 75% of changes exemplified "poor continuation." It was impossible to count the cases of poor continuation, since no objective criterion for it could be set up, but very few cases which would strike the observer as definitely poor were noted. Rather, the other cases were changes in the direction of symmetry, differentiation, etc. In short, the absence of good continuation does not necessarily imply the presence of its opposite. Had a less strict interpretation of good continuation been used, the percentage of cases falling in this category would be much higher than 25%.

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[34] Wertheimer, *op. cit.*

[35] Dot figures were not included in the count, since it was difficult to consider the direction of lines in figures which did not originally possess lines.

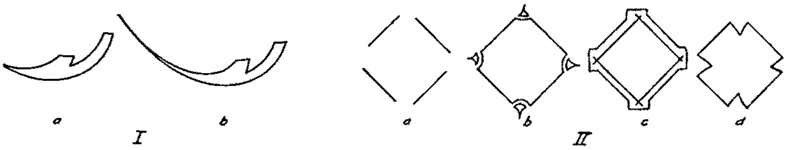

FIG. 13. EXAMPLES OF ACCENTUATION

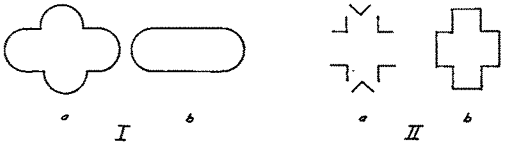

FIG. 14. EXAMPLES OF LEVELLING

The difference between the classes of figures is worth noting, especially that between symmetrical and asymmetrical figures, which is statistically significant (C.R., 5.0). Since asymmetrical figures already lack balance, it is not surprising that the added lines sometimes accentuate this lack, often giving a feeling of motion to the figure. In No. IV b, Fig. 12, which is typical of many cases, one has a definite impression of motion in the pattern. Since figures originally symmetrical do not suggest motion, good continuation of their lines is not called for so strongly.

Often an unbalanced, dynamic pattern was more pleasing than one exactly symmetrical. Openness, as well as asymmetry, often adds to the effect of movement and life, as No. IV b, Fig. 12 well shows. Even more striking effects attributed to the properties of figures have been reported by Lundholm[36] and Poffenberger and Barrows,[37] who found that lines could suggest various emotions.

(5) Accentuation and levelling. In his study of memory for forms, Wulf stressed the fact that all changes in the memory trace were in ac-

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[36] H. Lundholm, Affective tone of lines; experimental researches, *Psychol. Rev.*, 28, 1921, 43-60.

[37] A. Poffenberger and B. E. Barrows, The feeling value of lines, *J. Appl. Psychol.*, 8, 1924, 187-205.

cordance with the law of *Prägnanz*,[38] in that they brought about either an accentuation or a levelling of the characteristics of the figure. These two tendencies were found to govern some of the changes toward good configuration in our experiment. Since it was difficult to set up an objective criterion for these changes which did not yield many doubtful cases, no numerical count was made. However, some striking cases in which the two trends described by Wulf clearly operate appear in Figs. 13 and 14. In No. I b, Fig. 13, prolongation of parts of the figure accentuates them. In No. II, Fig. 13, three types of accentuation of the corners of a single figure are shown. The opposite trend toward levelling is shown in Fig. 14, where the outline of the figure is 'toned down' in both No. I and No. II. Such cases show that the law of *Prägnanz,* in Wulf's sense, governed some of the changes toward better form.

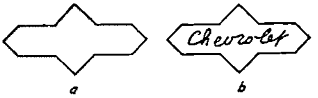

FIG. 15. A CHANGE IN THE DIRECTION OF FAMILIARITY, WITH SYMMETRY DESTROYED

(6) *Familiar forms*: In any experiment involving forms and their changes, the question most frequently raised is: Are these changes in the direction of more familiar figures? The answer to this question in this experiment is that 31% of all changes created familiar objects (*e.g.* houses, people, animals), familiar designs (such as flower patterns) or familiar geometrical figures. The latter include the circle, square, triangle, rectangle, ellipse and parallelogram. But to find the relative importance of structural properties versus the factor of familiarity in determining the changes, several possibilities must be considered.

First, there are cases in which a figure with properties found to be good, such as symmetry and closure, is changed into a more familiar form at the expense of good structural properties. An example is Fig. 15 b in which the symmetry of the figure is destroyed while a familiar figure is produced. It is significant that no good example could be found in the data of a case in which the production of a familiar figure decreased *both* the symmetry and the closure of the given figure. We assume that in such cases familiarity, rather than goodness of form is the chief determinant of the change. At the opposite extreme are found cases in which a figure originally familiar in form is changed to have good characteristics at the expense of its familiarity. In Fig. 16, for example, the familiar simple triangle is changed to have greater symmetry and differentiation, but becomes a less familiar form. In these two types of change, either the factor of familiarity or that of good form is clearly predominant. But in addition, there is a great group of cases in which changes toward good properties also make the figure more familiar. For instance, in No. I, Fig. 12, the change not only brings about greater symmetry and closure, but makes a frequently experienced figure, the circle. Such equivocal cases cannot be taken as evidence either for or against the factor of familiarity as the cause of the change. Some of the figures left unchanged are likewise ambiguous, for example, the circle and the square, which are both familiar and good according to the results of the experiment.

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[38] *Op. cit.*

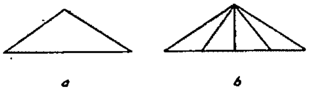

FIG. 16. A CHANGE IN THE DIRECTION OF INCREASED SYMMETRY, WITH FAMILIARITY DECREASED

TABLE IX

CHANGES DETERMINED BY SYMMETRY OR CLOSURE ALONE, AND BY FAMILIARITY ALONE

Type of change	Number of changes
Symmetry or closure but *not* familiar forms	867
Familiar forms but *not* symmetry or closure	167
Ratio	5:1

In comparing the extent to which good configurational properties and the factor of familiarity governed the changes, only closure and symmetry were regarded as good properties, since they are more objectively determined than most of the others, and since the inclusion of differentiation and simplification would comprise the great majority of figures. Limiting goodness of form to symmetry and closure makes the odds all the more strongly in favor of the factor of familiarity by cutting down the number of figures which possess good characteristics. In Table IX the changes which increase the familiarity but do not in any way increase the symmetry or the closure of the figures, 167 in all, have been compared with the changes which increase or emphasize symmetry or closure, but do not increase the familiarity. Of the latter there are 867, making the ratio of changes determined entirely by these two good properties to those determined by familiarity (to the exclusion of these two factors) 5 to 1. This indicates that these structural factors account for five times as many of the changes as are accounted for on the basis of past experience alone.

The numbers given above do not include the figures left unchanged which must have been considered good, so we may now consider the results for *all* figures. The categories into which the figures were classified may be briefly summarized as follows: (1) Cases of familiar forms in which neither symmetry nor closure was present. (2) Cases of symmetry or closure which were not familiar forms. (3) Cases of forms both familiar and closed or symmetrical. (4) Cases of forms neither familiar nor closed nor symmetrical.

The numbers and percentages of figures falling in each category appear in Table X. Closure and symmetry, in the absence of familiar forms, appear in 57% of all cases, while familiar forms lacking symmetry and closure appear in only 8% of all cases, showing that these two structural properties govern the *Ss'* preferences seven times as frequently as does the factor of familiarity of the figure. The remaining cases comprising 27% of all figures, in which both familiarity and good properties were present, and 7% in which neither occurred, cannot be used as evidence for the importance of either factor.

Thus the evidence from all figures, even more strongly than that from changed figures alone, is in favor of the structural properties as against past experience with the figures as determiners of good configurations. Our results are in line with those of Gottschaldt whose well-known experiment demonstrated the importance of structural properties over past experience in determining our perception.[39]

TABLE X

OCCURRENCE OF FAMILIARITY VS. SYMMETRY AND CLOSURE

Classification	No.	Percentage of all cases
(1) Familiar forms, no symmetry or closure	167	8
(2) Symmetry or closure, no familiar forms	1137	57
(3) Both familiar forms and symmetry or closure	548	27
(4) Neither familiar forms nor symmetry nor closure	148	8
Total	2000	100

A second question often raised in connection with the factor of familiarity is: Are such properties as closure, symmetry, etc., produced because they are good properties or merely because they are familiar qualities? Since they do occur more frequently in daily life than do their opposites (asymmetry, openness, etc.), were the *Ss* simply trying to produce patterns more familiar to them than the given forms? This question of the familiarity of *general properties* has not been attacked in this study, and other experiments must be designed to answer it. However, the overwhelming predominance of the structural properties over past experience with the figures, as shown by the ratios given above, points to the view that symmetry, closure, and such factors appear in the world of man-made objects because they are good and not because they are frequently experienced. It may be that we make things in these forms because they are physically stable and result in economy of behavior and perception, in short, because they are good: later, because we have made them, they become familiar.

(D) Simultaneous operation of several principles. While the various types of changes were treated separately, one is not to suppose that in each figure only one principle underlays the change made by each *S*. Rather it was the rule that several of the principles operated together in determining a change. For example in Fig. 3 c, we see a change governed

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[39] K. Gottschaldt, Über den Einfluss der Erfahrung auf die Wahrnehmung von Figuren I., *Psychol. Forsch.* 8, 1926, 261-317.

both by the tendency toward closure and that toward symmetry. In No. II b, Fig. 4, the change is in the direction of symmetry, closure and added differentiation. Symmetry and simplicity operate together in No. III b, Fig. 5. These are but examples of many such cases.

TABLE XI

Reasons Given for Changes Compared with Changes

Reasons	Percentage	
	of all reasons	of all changes
Symmetry and balance	22	38
Closure, connection, and completion	20	60
Familiar forms:	26	31
objects	13	21
geometrical designs and figures	13	10
Simplicity, regularity and uniformity	9	37
Elaboration (differentiation)	6	68
Motion (good continuation)	4	25
Miscellaneous individual reasons	13	

(E) Reasons for changes. We may next consider the reasons given by the *S*s for their changes in relation to the changes themselves. The reasons given show, on the whole, that the *S*s were often conscious of the principles they were employing in making their changes. The various types of reason are compared with the changes in Table XI. Balance and symmetry were classified together, as were all terms conveying closure, such as connection and completion. The various reasons, though mirroring many of the tendencies found in the results, do not correspond numerically to the changes actually produced. The latter usually occur more frequently than the former. The two reasons most frequently mentioned, symmetry and closure, occur in about equal numbers, whereas changes toward closure far outnumber those toward symmetry. Familiar objects, and simplicity were also created more frequently than they were mentioned by the *S*s. Elaboration (the reason most comparable to differentiation) is not mentioned nearly as frequently as it occurred. This discrepancy may indicate that the differentiation was not made for the sake of mere elaboration, but to accomplish some of the other aims of which the *S*s were conscious, *e.g.* symmetry, connection, uniformity, and motion. This corroborates the finding already discussed, that the added lines were almost always in accord with the whole-properties of the figure. The fact that simplification is mentioned as a reason more frequently than elaboration is significant in showing that the *S*s preferred to regularize the figures rather than to complicate them. But even when they had no clearly formulated rule in mind, their productions reveal the operation of the principles we have discussed.

SUMMARY AND CONCLUSIONS

In this study we have attempted to find the principles governing the productions of naïve *Ss* in attempts to produce good visual configurations by making changes in a wide variety of figures. A consideration of the types of figures changed shows that there is a steady increase in the chances that a change of some sort will take place as the figures go from the most closed and symmetrical to the most open and asymmetrical (Table II). We may conclude that closure and symmetry are considered good properties of a figure.

While lines or dots were added to the figures in 88% of all changes, this result is meaningless without a consideration of the ways in which they were added. The change occurring most frequently, in 68% of all changes, is that toward greater differentiation, a factor which must henceforth be reckoned as significant for goodness of form. The differentiation was found to exemplify configurational principles by enhancing the unity, simplicity or symmetry of the original figure and by emphasizing its natural subdivisions and regularities.

That the good configuration tends to be simple (unified and coherent) is shown by 38% of changes which were in this direction. That this number is not larger indicates that a good figure must not be too simple, or lacking in structuration.

Closure occurs in 61% of all changes, while the opposite tendency toward opening occurs in only 2%, showing again that the need for closure is strong in the good configuration.

Symmetry is increased in 47% of asymmetrical figures, and decreased in only 27% of symmetrical figures. Of all the changes, 38% are toward increased symmetry, showing that while not as important as closure, symmetry is nevertheless a preferred property.

That symmetry occurred less frequently than in other studies is partially explained by the tendency of the *Ss* to produce an effect of movement, incompatible with exact symmetry. This was frequently accomplished (25% of all changes) by continuing lines already present in the given figure.

Sharpening and levelling of the characteristics of the given figure also occurred to some extent.

Although 31% of the changes brought about familiar forms, good configurational properties often were operating in the same cases. Considering all figures, both changed and unchanged, we find that the two factors of symmetry and closure alone (without familiar forms) govern almost seven times as many cases as does the factor of familiarity in the absence of symmetry and closure. Thus configurational properties, rather than familiarity with the figures, determined the *S*s' judgments.

Working with an entirely different method from other investigators, we find that many of the factors commonly interpreted as good are considered good by naïve *S*s. Our *S*s, although free to do as they chose, and with plenty of time at their disposal, still were guided by the principles found to govern the good configuration in more primitive psychological functions. These factors operate as well in the fields of art and aesthetics. Since the continuity with respect to these principles between the various levels of mental activity is so clear, we may minimize culture as the determinant of the changes in the experiment. The tendencies toward closure, symmetry, good continuation, simplicity and differentiation operate in the short exposure of forms, vision in the blind spot, and tactual perception of forms, where the factor of culture could scarcely enter into the almost automatic reaction of the *S*. The prevalence of these properties in such natural events as crystal formation, plant structures, etc., attests their fundamental nature.

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AN ANALYSIS OF HUMOR AND LAUGHTER

By JOHN M. WILLMANN, University of Illinois

The chief obstacle to an understanding of laughter has been the variety in laughter itself. This peculiarly human expression arises from situations which seem to have so little in common that apparently there is no fundamental principle. There is the laughter of mere joy and happiness found particularly in children; in fact small children seem to indulge almost without cause. Then we find the same expression in adults produced by the subtle intellectual processes involved in wit and humor. For some strange reason tickling various parts of the body will cause the same reaction. Laughter usually denotes friendliness and congeniality, but even such a broad generality as this finds exception, in the laughter of ridicule. In spite of such variety there must, however, be some basic principle. The following formula may be postulated. *Laughter occurs when a total situation causes surprise, shock, or alarm, and at the same time induces an antagonistic attitude of playfulness or indifference.* Let us now examine the whole wide field of laughter to test whether our formula is applicable throughout. If it is, we may then examine the formula itself and discuss its psychological significance.

The genetic approach is best, for in children we find laughter in its most natural and unlearned forms. First there is tickling, which obviously involves an attack upon the body, but the total tickling situation also includes the playful spirit in which the attack is made. Allport repeats the common observation that "the ticklish zones overlie some of the most vital organs of the body. Hence there is something terrible in a thrust at these parts."[1] But it is all in fun. Tickling does sometimes fail to produce laughter, producing fright or anger instead. Particularly does this occur when it is attempted by a stranger. The child must know and trust the tickler in order that the situation will arouse a playful attitude along with the natural alarm produced. When these conditions are fulfilled, the terms of the above formula are satisfied, for the total situation causes alarm and at the same time induces an attitude of playfulness.

Along with tickling we may also consider a few of the typical laughter-producing situations that have been studied experimentally. When Kimmins secured laughter from small children by means of a jack-in-the-box, he explained it as due to the

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[1] F. H. Allport, *Social Psychology*, 1924, 252.

surprise or slight fear which the toy produced when followed by an immediate dissipation of that fear on finding the toy quite harmless.[2] In other words, the situation had the same ambiguity of fear over something harmless that occurs in tickling.

Washburn reported that laughter was produced in infants at 24 weeks by suddenly appearing before them from under a table and saying "boo."[3] Here again we have sudden surprise plus playfulness. Laughter was also produced in the same infants by "elevator play," in which "the child was raised quickly into the air above the experimenter's head, held there and jiggled gently back and forth before being lowered to its original position on the experimenter's knee. While the child was held above her head the experimenter smiled and talked a little to the subject. Although this was universally reported as a game which elicited laughter at home it was one of the least successful in the laboratory." This also is analogous to tickling. The child is alarmed at being held so high, but he is able to take it in fun because his fear is removed when the experimenter smiles and talks to him. It succeeds better in the home than in the laboratory, because the child can more easily enter into a playful spirit with his parents than with a stranger.

Eastman has summarized this general type of laughter in the following way: "To children every untoward, unprepared for, unmanageable, inauspicious, ugly, disgusting, puzzling, deafening, banging, bumping, or otherwise shocking and disturbing thing, unless it be calamitous enough to force them out of the mood of play, is enjoyed as funny."[4] The reason that children laugh so much in general as compared with adults is that they are perpetually in more or less of a play-attitude. Thus half of our laughter formula is already fulfilled, and the rest is fulfilled whenever something shocking or surprising appears.

Exactly the same kind of laughter can occasionally be observed in adults, as at the amusement-park. Half of the attractions at such a place are devices for frightening persons under conditions of actual safety. A roller-coaster ride, for instance, is really a terrifying experience, with its sudden descents and quick curves, and all the speed and noise. Timid persons will not ride on them and those who do ride will be heard to do about as much screaming as laughing. But while the situation produces plenty of surprise, shock, and alarm, it produces at the same time an attitude that is playful and carefree. One goes to the amusement park for fun, and everybody else is there for the same purpose. So in spite of the frights of the roller-coaster one keeps an attitude of play throughout, knowing that real danger is absent. Again we have an analogy to tickling, in which the playful attitude and the complete trust in the aggressive agent eliminates fear.

Similar to, but not identical with, this type of laughter is the laughter of relief which follows a narrow escape. It occurs, *e.g.* when something breakable is knocked off a table and is caught just before it strikes the floor. It occurs after any such averted accident at the dinner table as almost upsetting a glass of water. Such incidents are usually followed with a 'whew' and a nervous laugh. It is the laughter of relief. The imminent accident momentarily fills one with alarm, as the jack-in-the-box alarms the child, but in like manner when everything turns out right the fear becomes groundless. The harmless conclusion does not call for any such vigorous reaction as was present the moment before. The situation as a whole, therefore, arouses the two antagonistic attitudes named in our formula: shock plus indifference. This particular kind of laughter coincides with Kant's definition of laughter as "an affection arising from the sudden transformation of a strained expectation into nothing."

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[2] C. W. Kimmins, An investigation of the sense of humor in school children, *Rep. Brit. Ass. Adv. Sci.* for 1921.

[3] R. W. Washburn, A study of the smiling and laughing of infants in the first year of life, *Genet. Psychol. Monog.*, 6, 1926, 397.

[4] M. Eastman, *Enjoyment of Laughter*, 1936, 3.

We may now turn our attention to humor, the most prolific source of laughter, and observe in it the same principles. In order to do so we must first examine its *logical* structure in detail; and here we shall find that humor always results from *the union of two ideas which involve some sort of contradiction or incongruity.* The importance of incongruity in humor has long been recognized, but the fact that the two incongruous parts must be united in some specific way has generally been neglected. This union is accomplished in three different ways; (1) *the two ideas may be united by possessing important common elements,* (2) *the one may simply be an inference drawn from the other, or* (3) *they may be seen actually to occur together in objective reality.* In one or another of these three ways we are compelled to consider the two incongruous ideas as a logical unit. This principle can be clearly seen in the following jokes.

Stranger (to seven-year-old Boston boy): "Say, bub, where's Boylston Street?"
Boston Boy: "While your mode of address, sir, seems to me to savor of undue, not to say unwarranted, familiarity, you shall have the information you seek. You will, perchance, descry some distance up the street, an imposing structure of commingled Renaissance and early Venetian architecture. The street for which you inquire is immediately contiguous."

Friend: "Isn't there anything you would like to say, Sam, before they pull the rope?"
Sam (with head in noose): "Jes' tell the judge maybe he done a good thing after all. This is gonna be a mighty good lesson to me."

In the latter of these examples the incongruity is obvious: if the man is about to die how can he mend his ways in the future? The execution will end his life, while the idea of 'a good lesson' to him requires that his life continue. We may express this diagrammatically in the following way:

Being hanged	That it will be a good lesson to him
cessation of life	*continuation of life*

In roman type we have the two 'ideas' (humor is the union of two ideas which involve an incongruity) and in italics beneath them there is isolated the incongruity itself which these ideas involve. The incongruity is made up of a pair of direct opposites. But what is the element in common that unites the two parts printed in roman? In what respect do they fit in spite of the incongruity they involve? They fit in the matter of *punishment.* The idea that something is going to be a good lesson to a person always implies that he is about to be punished in some way, and the execution also includes this punitive element. Therefore it is common to both the ideas; and we may complete our diagram as follows, inserting a dotted line to represent the uniting of the ideas.

Being hanged	(Element in common) punishment	That it will be a good lesson to him
cessation of life		*continuation of life*

The first of the above jokes yields readily to a similar analysis. The two complete ideas are expressed in roman in the diagram below; and they contradict each other because the informality and lack of learning that we associate with 7-yr.-old boys is incongruous with the formality and amount of learning displayed by this boy's reply, even though he is of Boston. But these two incongruous ideas are connected by certain elements that they have in common: the man asks where Boylston Street is and the boy does direct him to it; also the boy reproves the man for his familiarity, and undoubtedly he *was* addressed in a very familiar way: and finally, we are informed that the lad is a Boston boy, and Bostonians claim a high level of culture. In these three respects the inappropriate reply is appropriate.

A seven-year-old Boston boy being asked where Boylston Street is	(Elements in common) 1. The element of directing to Boylston Street. 2. The element of familiarity of address. 3. The element of superior culture in Boston.	"While your mode of address, sir, seems to me to savor of undue, not to say unwarranted, familiarity, you shall have the information you seek, etc."
general attitude and intellectual level associated with seven-year-olds		*extreme formality and culture*

A second way in which the two ideas may be logically united is by a process of inference. In this method what is actually presented in the joke is a third factor plus just one of the 'ideas.' Then from these two elements the second idea is *inferred.* For example:

"Did you make the debating team?"
"N-n-n-no. They s-s-said I wasn't t-t-tall enough."

Anyone asked to name the point of this joke, will of course reply that what really kept the man off the team was his stuttering. But that fact is nowhere stated in the joke: it is only inferred.

"N-n-n-no. They s-s-said I wasn't t-t-tall enough."	+	"Did you make the debating team?"	=	That the real reason was his stuttering

In other words the stutterer's reply, appearing at the left, when taken in conjunction with the question that preceded it, which is shown in the center, compels the deduction which follows at the right; compels us to deduce that the real reason he was rejected was his stuttering. Obviously then the two outer portions are 'united.' For if A + B can actually compel us to deduce C, then C is certainly logically connected with A and B; just as it might be said that the conclusion of a syllogism is intimately connected with the two premises from which it is drawn.

We may complete the diagram by including the precise contradiction which makes the point of the joke.

"N-n-n-no. They s-s-said I wasn't t-t-tall enough."	· · · · ·	"Did you make the debating team?"	· · · · ·	That the real reason was his stuttering
that he is not tall enough				*that the real reason is his stuttering*

The 'contemptible pun' is constructed in exactly the same way as the above joke. Always some ambiguous word is presented with one of its two meanings dominating and then an additional statement compels us to *infer* the other meaning of the word. For example:

"What made the Tower of Pisa lean?"
"It was built during a famine."

"A little bit goes a long way," said the zoo-keeper, feeding a handful of hay to the giraffe.

The question, "What made the Tower of Pisa lean?" has one obvious meaning when considered by itself. But the addition of the answer, "It was built during a famine," compels a new interpretation, as the diagram illustrates.

"What made the tower of Pisa lean?"	+ · · · · ·	"It was built during a famine."	= · · · · ·	What made the tower of Pisa thin?

No one would ever think of the second interpretation, the tower being thin, until he was forced to do so by the statement that it was built during a famine. Therefore that statement *unites* the two interpretations—compels us to consider them together—and the new interpretation contradicts the other interpretation: it displaces it.

The second example is one of those puns in which the play upon words consists in bringing out the literal meaning of an idiom and contrasting it with the figurative meaning. The resulting contradiction is shown in italics.

"A little bit goes a long way."	+ · · · · ·	said the zoo-keeper, feeding a handful of hay to the giraffe	= · · · · ·	A small amount travels a long distance
figurative meaning: a little is a plenty				*literal meaning: a small amount travels a long distance*

The third and most natural way in which the two ideas may be united is by their actually occurring together in objective reality. For we certainly must consider them together if they occur that way before our very eyes. This general class of humor includes most of those incidents that 'must be seen to be appreciated'; no verbal description can really make such things seem funny, at least to the point of laughter.

A good example of this is the humor arising from small children making very sophisticated remarks. In the case already considered in which the small Boston boy directed a man to Boylston Street, we found certain common elements uniting the two ideas, *i.e.* connecting the small-boy idea with the formal speech which was so inappropriate to his age. But such common elements are not necessary if, instead, we actually overhear a child speak inappropriately. Should a little girl of four or five years—to take a timely instance—frankly accuse her chum of having no sex appeal (using the term without knowing its meaning, as children will often do), it may sound funny indeed to a bystander, even though her remark has none of those logical justifications such as we found in the case of the Boston boy's speech. It is just for want of such justifications that her remark is not really funny when stated here; nor would it be eligible for print on a 'joke page.' Because as quoted here, or as seen among stock jests, the two ideas are not united in any way, *though the incongruity itself is obvious enough.* The bystander, on the other hand, is compelled to consider together the small child and her sophisticated remark because he observes them actually occurring together. In this way they are united.

A small child ...... United by actually occurring together ...... Accusing her chum of having no sex appeal

innocence *sophistication*

Another common source of humor may be described as *a radical change in something familiar.* A new style in dress, for example, if it is at all extreme, is apt to appear comical at first. 'New-fangled' things are usually greeted with laughter, because the new aspect of the thing is incongruous with what we are already familiar with. Or take an example from pure comedy;—the reflections seen in comic mirrors. Here we may see a friend suddenly very thin or fat, and the humor of the sight is due to the incongruity between the size and shape we associate with his person and the alteration which we now see in the mirror. There is no logical justification for it but we do *actually* behold it.

The person himself .... Actuality .... The present fatness or thinness

his normal figure, to which we are accustomed *the present fatness or thinness*

What is actually seen is the upper outside portions of the diagram. The incongruity involved is in italics.

The humor of inferiority is a special case of "a radical change in something familiar." When the word inferior is applied to an object it merely indicates that it falls below the average quality for that general class of objects. For example, a watch might be inferior to the point of humor through its unusual size, its very loud ticking, or some other property. But it is still a watch. Because watches in general have a better quality, this specimen is incongruous with its general class.

A watch ....Actuality.................. Enormous size, audibility, etc.

the average quality of watches — *the inferior quality denoted by these properties*

It is true that other factors enter into 'inferiority' laughter, particularly the desire to establish one's own superiority by ridiculing defects in other people, or ridiculing the things they do or things they possess. Fundamentally this brand of humor is not, however, different from the rest. Like other humor it is made up of united incongruities; in this case *quality* incongruities.

We have now examined several instances of humor and found upon analyzing them that they consist of united incongruities. Let us now consider a few examples of such incongruities lying outside the field of humor, which occur in our daily life, for the purpose of finding out what they normally signify and what is our normal reaction to them. We shall then be in a position to understand their significance in humor, why they may sometimes be funny.

The fact is that united incongruities are responsible for all those interesting portions of our experience that may be described as strange, mysterious, queer, unusual, puzzling, peculiar, or out-of-the-ordinary. For any strange occurrence is such by virtue of the fact that it conflicts with what was to have been expected under the circumstances.

If a cat barks it is a united incongruity. Or we may take for example any trick performed by a magician. The thing he achieves which makes the trick is presumably impossible under the conditions which he sets up; this provides the incongruity. While the fact that he does achieve it, that it 'actually occurs,' is what unites the incongruity. After shuffling a deck of cards and allowing one to be withdrawn face-downwards, presumably he cannot know what card has been withdrawn, but under these very conditions he does name the card. And our reaction is *surprise* and *curiosity.*

Shuffling the deck and allowing a card to be withdrawn at random, face-downwards .....Actual occurrence.......... That he correctly names the card

that he cannot know the card withdrawn — *that he correctly names the card*

The advent of aviation afforded an illustration. The original airplanes constituted in everybody's mind a united incongruity. Because the fact that they *actually* flew was incongruous with the universal observation that any object heavier than air cannot remain in the air without visible support. The reaction here, as in the above example, was of curiosity and wonder. *Surprise, curiosity, or wonder is the normal reaction to united incongruities.*

The popular use of the word 'funny' is of interest at this point. This word is commonly used to describe anything that fits into our category of strange, mysterious, queer, puzzling, or unusual. We hear such remarks as, "Isn't it funny that he should act like that?" or "There's something funny about this that I can't quite understand." This familiar use of the word to describe things that are not really funny at all simply reflects the dual rôle of united incongruities; the fact that they produce surprise and wonder ordinarily and the fact that they can also produce humor. Funny things and strange things are identical in their logical structure.

But when are they strange and when are they funny? The fundamental difference is that the non-humorous incongruities seem to be more real; they are puzzling and hard to understand. Or they may deal with serious matters. The humorous incongruities on the other hand are nonsense, are easy to 'see through.' We quickly appreciate that there is no real incongruity involved.

This distinction can sometimes be observed clearly in the case of parlor magic. On the completion of a trick the group may be very curious about what it has seen, but if the magician explains the trick and then repeats it, the spectators will usually break into a laugh when they see how simple it really is. At first they are puzzled over what seems to be a very real contradiction. But once they understand the trick the contradiction becomes a mere illusion and is no longer worthy of serious consideration.

Or if one meets a friend whom one has not seen for a year or two and the friend is now very slender or very stout, whereas he was formerly of normal size, one is *surprised* at the change, but does not laugh. If the change is in the direction of a loss of weight it is actually a serious matter. But, as we have noted, such changes in size are very ludicrous when seen in comic mirrors. For here the sudden change, whatever it is, cannot be a serious matter: and we realize all the while that the incongruity is not real but only apparent.

It is interesting that, in explaining the origin of words, etymology presents many a play upon words but without punning. We learn, *e.g.* that the Pilgrims drew lots for the division of ground, and that this is where we get our word *lot* as it is used today by realtors; each Pilgrim's piece of land was the *lot* that he drew. To learn the roots of some of our familiar words such as this may be quite surprising but not humorous, though, as in this case, there may be an actual play upon a word. The difference between such a bit of information and an ordinary pun is that this play represents an actual historical fact, while a common pun represents sheer nonsense in which the ambiguity has no more foundation than that afforded by the world itself (*e.g.* 'What made the Tower of Pisa *lean?*')

We may now fit humor of the type we have been studying into our general formula for laughter. The original formula, it will be recalled, was as follows: laughter occurs when a total situation causes surprise, shock, or alarm, and at the same time induces an antagonistic attitude of playfulness or indifference. The humor we have considered up to this point fulfills these conditions. For we have found that the normal response to a united incongruity is surprise and curiosity. We have also learned that united incongruities are the substance of jokes and humor, the difference being that the humorous incongruities are relatively unreal; they represent nonsense which cannot be taken seriously. Therefore a joke is a situation which tends to produce surprise or curiosity and at the same time calls for an attitude of playfulness. The nonsense is playful; the incongruity is surprising; and the total situation therefore is quite analogous to tickling, with its playful but alarming thrusts, which easily make a child laugh.

There remains a second general class of humor, as a rule superior to the kind we have been considering. Freud uses the term *tendency humor* to describe a type of jest which roughly corresponds to this class. The fact that humor results whenever a situation contains both surprise and playfulness makes it possible for united incongruities to produce humor in a much more direct way than in the examples thus far considered. Our exact definition for united incongruities is "the union of two ideas which involve some sort of contradiction or incongruity." How simple then merely to have one of the ideas playful while the other is in some way surprising. The fact that the ideas are united makes them into a single 'total situation.'

There are just four major types of such surprising or shocking ideas employed in humor. These are *tragedies, insults, wickedness,* and *obscenity.* Humor is produced whenever such a shocking idea is united with some other idea which, by way of contrast, is playful or mild or commonplace.

An immigrant was making his way across the Wild West in search of a man to whom he had a letter of introduction. He came across a cowboy sitting by the side of a track and asked him if he could tell him where to find the man for whom he was looking.

"Does Big Joe live around here?" asked the immigrant.

"Nope," said the cowboy.

"Well where can I find his neighbor, Long Sam?"

"I'm Long Sam," replied the cowboy.

"But they told me," said the immigrant, "that Big Joe lived within gunshot of you."

"That's right," said the cowboy, "he did."

"No, my brother can't play with us today."

"Why not?"

"We were playing a game yesterday seeing who could lean out the window farthest."

"Well?"

"Well; he won."

Tragedies and misfortunes are always surprising or startling to some extent. We are shocked if a man is killed or loses a limb. It is even shocking to see a person cut himself or to learn that he has lost an object of value.

Serious tragedies are to be found in both of the above jests, but it must be observed that they are expressed in a mild way. In the first, the mere admission on the part of the cowboy that Big Joe lived within gunshot of him, informs us of Big Joe's tragic end. In the second, the mild statement "he won" tells us that a boy fell from a window and was injured. Obviously *there is an incongruity involved when such tragic things are so mildly expressed.* But how is it that "he won" informs us that the boy fell from the window? Without making a detailed analysis we may simply say that it is on the basis of the preceding dialogue that the inference is made. That is "he won," when taken in conjunction with all the preceding dialogue, forces us to the conclusion that he fell from the window.

"He won"	+	"My brother can't play with us today . . . we were playing a game seeing who could lean out the window farthest."	=	That he fell from the window and was injured
the mildness of this statement when taken alone				*the shockingness of tragedies*

The following example illustrates how poetic factors may contribute to the fundamental requirements of humor, though they are never sufficient of themselves to produce humor.

There was a young lady of Rigre
Who smiled as she rode on a tiger.
  They returned from the ride
  With the lady inside,
And the smile on the face of the tiger.

Here we have the tragedy of a woman eaten alive by a tiger. But this calamity is preceded by the mild statement that the young lady, with a smile on her face, went out for a ride on the tiger and returned later on. Not only is it preceded by this statement but it is intimately connected with it, by common elements. For glancing at the two sections of the poem in the diagram below we see that the 'smile' occurs in both, though, to be sure, it is transferred from the lady to the tiger. More important still is the fact that these two portions together form a poetic unit; they share the pattern of rhythm and rhyme found in all limericks. The first three lines of the poem definitely predict two more lines which will have a certain meter and whose terminal words will rhyme with 'ride' and 'Rigre,' respectively, and the last two lines completely satisfy this prediction. Thus we have two united ideas; and they involve an incongruity insofar as the one is a mild statement while the other describes a tragedy.

There was a young lady of Rigre Who smiled as she rode on a tiger. They returned from the ride . . .	(Elements in common) 1. The smile. 2. The identical rhythm and the rhyming elements.	With the lady inside, And the smile on the face Of the tiger.
mild, emotionally neutral		*surprising, shocking*

The commonest type of humor occurring in everyday life is the humor of tragedy in which the two ideas are united by actually occurring together. A case of this type is found whenever some mild commonplace act is seen to result in a misfortune; for example, a person making a wide gesture and thereby ripping his coat, or attempting merely to pull down a window-shade and thereby pulling roller and all down upon himself. Similarly we have the standard 'tragedy' of pounding one's finger accidentally while hammering a nail (the two things are actually seen to occur together): first we observe the person striking at the nail in all sincerity and then we see him actually hit his finger instead.

Hammering a nail	Actuality	.... Hitting a finger instead
mild, commonplace		*surprising, shocking*

All the standard practical jokes are built on this same plan. For example, in the old prank of placing a tack on a chair or of removing the chair altogether just before the person seats himself, the commonplace act of sitting down is seen to result in a sudden shock. Or in the case of the exploding cigar, the mild pastime of smoking is seen to develop into sudden amazement on the part of the victim.

> By undertaking a series of risky schemes, two not very scrupulous business men had succeeded in amassing an enormous fortune and were now intent on forcing their way into good society. Among other things they thought it advisable to have their portraits painted by the most prominent and expensive painters in the city, men whose works were considered masterpieces.
>
> The costly pictures were exhibited for the first time at a great evening gathering, and the hosts themselves led the most prominent connoisseur and art critic to the wall of the salon on which both portraits were hanging side by side, in order to elicit from him a favorable criticism. He examined the portraits for a long time, then shook his head as if he were missing something. At length he pointed to the bare space between the pictures and said "And where is the Savior?"

This is an example of the humor of insult, a very common type of impromptu joke in ordinary light conversation. An open insult is an offence; but an insinuated insult is a joke, because then the offensive thing is being said mildly. Had the art connoisseur frankly called his hosts a couple of thieves he would have 'surprised' and 'shocked' every one; but there would have been nothing comical about it. If, however, he can make that very accusation by merely asking the question, "Where is the Savior?" then he is being witty. The mild question and the shocking insult are united by the method of inference. The insult is indirectly inferred from his question.

The following is a sample of the many jokes produced by alluding to some form of wickedness, sin, vice, or other transgression of the prevailing moral code.

"Willie," said the mother, "come and give your aunty a nice big kiss."
"No, I won't."
"For shame Willie, aunty wants you to kiss her."
"But daddy got slapped yesterday when he tried it."

The disclosure made by this small boy was no doubt surprising, but he makes this disclosure simply as an explanation of his own reluctance at kissing his aunt, which is a mild matter in itself. The element in common that serves to link the two ideas may be designated as something unpleasant that the boy associates with kissing her. For such an unpleasant association is *implied* in his reluctance and is then *fulfilled* in the reason that he gives and is therefore common to both the ideas.

	(Element in common)	
His reluctance at kissing his aunt	…… That there is something unpleasant about kissing her ……	That his father got slapped for trying it
commonplace		*shocking*

Finally, we have the humor of obscenity, which is illustrated by the following excerpt from a schoolboy's history paper.

"Queen Elizabeth was the 'Virgin Queen.' As a queen she was a success."

Obscenity is really a special case of the humor of wickedness and transgression which we have just considered. It is the extreme case. Because of the taboo that is placed on the mere mention of sexual matters we are shocked when such mention is made, particularly in a mixed group. So all that is required for obscene humor is that the shocking idea shall be united with some other idea that is not shocking, giving rise to an incongruity. The schoolboy informs us of Elizabeth's loose morals by the bland statement that she was a successful queen, which in itself is quite inoffensive. Obscene humor then is not in a class by itself, in some way intrinsically different from all other humor. Structurally it is constituted like all the other types.

The necessity of having a mild idea to accompany the shocking idea in the kind of jokes we have been considering cannot be overemphasized. Without it obscenity becomes mere vulgarity, tragedies are pure misfortune and nothing else, and insults are simply vicious verbal attacks. Only when something incongruously gentle is linked with such things do they become humorous in the least.

The relation of such humor to our laughter formula is obvious. "Laughter occurs when a total situation causes surprise, shock or alarm, and at the same time induces an antagonistic attitude of playfulness or indifference." It is the obscenity or the tragedy or the insult that produces the shock, while the mild commonplace idea linked with it is what calls for the antagonistic attitude of indifference. *The two being "united" operate together as one "total situation."*

Let us now examine the formula itself. Why is it that laughter results when a single situation tends to induce the antagonistic attitudes of surprise and playfulness? A simple but adequate hypothesis can easily be formulated, in fact has been formulated, though in fairly vague terms. The laughter of tickling is taken as the starting point, and without explanation; the response is considered as innate. Humor is then described as a sort of 'mental tickle,' and the figure of speech is a good one in view of the exact similarity which we have found does exist between humor and the tickle situation, and the theory is entirely acceptable.

We may take the position of Darwin and certain others and consider laughter in its most elemental form to be merely *the overt expression of the emotion joy: an expression denoting pleasure and happiness, and indicative of a carefree, playful state of mind.* An expression, what is more, which very early in life becomes socially conditioned, since most of the infant's pleasures and satisfactions, as well as the adult's, depend upon the presence of other persons.

The tickle situation then has factors which definitely tend to stimulate the laughter-response. First, it is a social situation for, there are at least two persons involved (a person cannot tickle himself and produce laughter) ; and secondly, both parties are being playful. But as we have found, in the person being tickled fear and alarm tend to be aroused as well as playfulness. The two responses are antagonistic, incompatible; only one can occur. The significant fact is that apparently when a single situation thus stimulates two antagonistic responses, whichever response is made will actually be reënforced by the presence of the tendency towards the other. This suggests the principle of *positive induction,* discovered by Pavlov, of which the following illustration may be cited.[5]

> A conditioned alimentary reflex was established to tactile stimulation of the front paw. A similar tactile stimulation of the hind paw was differentiated, the inhibition being complete, so that not a single drop of saliva appeared in response to stimulation of the inhibitory place. The secretory effect was increased almost 50% when the positive conditioned stimulus was applied immediately after the termination of the inhibitory stimulus, and the latent period of the reflex was

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[5] I. P. Pavlov, *Conditioned Reflexes,* 1927, 189.

definitely shortened. Moreover, the intensity of the motor alimentary reaction of the animal was also considerably increased.

Contrary to what might be expected, the co-stimulation of the two opposite responses does not merely produce a resultant or cancellation of them. The response that does occur is actually strengthened by the presence of the tendency towards the opposite reaction.

In order to effect this reënforcement, the stimuli for the two antagonistic responses must be closely associated ('united'). In Pavlov's experiment this condition was fulfilled by way of the fact that the excitatory stimulus had to follow the inhibitory stimulus immediately, creating in effect a single situation the two parts of which could not be reacted to separately.

Thus in tickling not only does the laughter represent the winning out of the playful response over the tendency to become angry or fearful, but the laughter is actually reënforced by this incipient fear. This accounts for the violence of such laughter. As Allport says, "the tickler does not miss the opportunity of making the feint as sudden and terrifying as possible in order to get the heartiest peal of laughter from the child." If it were not for this reënforcement, if the fear itself were not actually finding expression in the laughter, then the laughter of the person being tickled would be as mild as that of the person doing the tickling, and the two are hardly to be compared in strength or quantity.

The same principle of course may be applied to the laughter of humor, for humorous situations are exactly analogous to tickling. In both something surprising or alarming is presented along with an appeal to playfulness or indifference. Nor is this enough in itself. The appeal for a laughter-response and a playful attitude is further strengthened socially. It was mentioned above that the laughter of joy and happiness is very much socially conditioned, and that this is one source of the appeal for laughter that is made by the tickle situation, since tickling is a social pastime. Similarly humor too will produce little or no laughter except in the presence of other people. But when there is added to the playful appeal in the joke itself this factor which facilitates laughter, then a humorous situation presents a clear choice between the playful laughing-response and the surprise- or shock-response. If the playful response wins out, it is actually reënforced by the attendant surprise or shock, *with the result that actual laughter occurs, perhaps violent laughter, instead of a mere playful attitude or passive indifference.*

This reënforcement is particularly obvious in the case of obscenity and in the humor of tragedy, because here the shock involved may be considerable. The practical joke is a good illustration of the latter. There is always some sort of painful surprise suffered by the victim of the joke; this is the "surprise, shock, or alarm" part of the situation, while other factors in the total situation cause the whole thing to be taken in the 'playful' way, at least by the onlookers. The significant fact is that the more surprising or tortuous the trick can be made the funnier it appears just so long as it can still be considered a joke. Because in this way a response that is really rather violent is being aroused along with the playful attitude, and the laughter therefore is proportionally violent. In other words, the more we tend to gasp at what happens to the victim the harder we laugh instead, if we laugh at all. On the other hand, if the joke goes amiss and the victim is really injured, then the shock-response dominates, and it is actually accentuated by way of contrast, by the playful spirit in which it was all intended. Here the reënforcement-effect is going in the other direction; and nothing is sadder than the practical joke which ends in a real tragedy.

Just as the funniest practical joke is the one as painful as possible that can still be taken humorously, so the funniest obscene joke for any individual is the one that falls just within the limits of his toleration, so that he receives the maximal shock that he can take with a playful attitude. Obscene jokes in general produce greater laughter than other jokes, and for this same reason; they are *shocking* beyond the limits of most of the other kinds of humor.

It is interesting that similar conclusions were reached by Lund in an investigation of crying and weeping.[6] He makes the followng statements:

> Observations made at funerals again show the value of a pleasure stimulus and the extent to which weeping in such situations represents a mingled emotional state. Reference by the minister to some desirable trait in the character of the deceased or to some heroic incident in his life produced marked emotional effects and increased lacrimation. . . . Even in connection with privation and loss, then, a pleasure element or a redeeming feature introduced in the midst of the generally depressing circumstances serves to release the flow of tears.[7]
>
> Neither sorrow, dejection, joy, nor elation, when occurring in pure form, is very effective, if at all, in producing the lacrimal discharge. This discharge appears typically when a depressing or otherwise unpleasant situation gains a redeeming feature, or when tension and unpleasant stimulation are followed by pleasant or alleviating stimulation.[8]

While weeping is primarily associated with sorrow apparently the situation most effective in producing actual tears is a double situation which creates sorrow and at the same time induces an opposite kind of response. It is in like manner that primary laughter is associated with joy and playfulness, as Darwin has pointed out, but that a playful appeal alone is not adequate to produce laughter (except in children). With adults the typical funny situation is one providing a playful appeal *plus* an antagonistic response to reënforce it.

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[6] F. H. Lund, Why do we weep? *Soc. Psychol.*, 1, 1930, 136-151.

[7] *Op. cit.*, 146.

[8] *Op. cit.*, 149.

AN EXPERIMENTAL AND THEORETICAL ANALYSIS OF FINE MOTOR SKILLS

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A survey of current experiments on individual differences in motor skills reveals the inadequacy of any single theoretical interpretation and the need for an eclectic examination of all theories, both qualitative and quantitative. The present paper reviews these theories, together with an evaluation by means of data from a number of experiments previously unpublished. It also provides a background for the more refined statistical analyses by modern factorial methods for two new and more extensive experiments by Buxton[1] and Seashore, Buxton, and McCollom.[2]

The major aspects of the problem are as follows: I. The principal variables: speed, strength, and precision. II. The secondary variables: gross *vs.* fine coördinations in each of the primary variables. III. The size or magnitude and stability of individual differences. IV. The *kinds* of factors underlying or determining individual differences. V. The quantitative organization of abilities into (A) general, (B) group, and (C) specific factors.

I. PRINCIPAL VARIABLES

Every motor skill involves three variables: speed, strength, and precision or steadiness. Any given skill ordinarily emphasizes only one or two of the variables and is classified on the basis of this emphasis, *e.g.* speed of reaction, high speed precision. Most experimental results emphasize speed. What little is known regarding strength and precision indicates that individual differences in these two fields may not accord with those now shown for speed.

II. SECONDARY VARIABLES

All skills probably involve the action of entire body, but some emphasize the *gross coördinations* of large muscle activities, *e.g.* athletics, while others emphasize the finer coördinations, *e.g.* manual skills. Here again the results suggest that the organization of individual differences may not be the same for both fine and gross coördinations. Our results

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[1] C. E. Buxton, The application of multiple factorial methods to the study of motor abilities, *Psychometrika*, 3, 1938, 85-95.

[2] R. H. Seashore, C. E. Buxton, and I. N. McCollom, Multiple factorial analysis of fine motor skills, (in press, this JOURNAL).

concern primarily the *fine* coördinations, including those emphasizing either speed alone or precision at high speed. Strength and steadiness tests are brought in only incidentally in the studies which have appeared to date.

III. MAGNITUDE AND STABILITY OF INDIVIDUAL DIFFERENCES

The study of individual differences may be thought of as a secondary factor in the prediction of results according to the general laws or principles which describe the primary or central tendencies of psychological activities. The importance of individual differences depends primarily upon two factors, viz. their *size* and *stability* in relation to ordinary amounts of training. The practical question of differential psychology is always "can we save time and energy in the development of a skill by *selecting* personnel on the basis of short samples of behavior, and training only these persons rather than by attempting to train all candidates for a given type of work?" Having determined the importance of individual differences from a study of their size and stability, we next have to consider the relative importance of selection, of work-methods and working conditions, and of training. These variables shift according to the nature and interrelations of the underlying factors such as biological capacities, adoption of behavior patterns, and refinement of these patterns by training.

IV. NATURE OF THE UNDERLYING FACTORS

(A) Biological capacities. For a given set of stimuli (including instruction) designed to produce a given response (skilled act)[3] we find that different individuals will produce different results. These individuals are known to vary in body-build, sensory acuity, and in other basic anatomical and physiological aspects which might explain the differences in psychological performances. In the field of physics we have a useful analogy by which the structural characteristics of a machine, *e.g.* the size of a pump cylinder, determine its maximum output or capacity. In physics the formulae are reversible, that is we can either predict the performance from the structural characteristics, or vice versa.

In carrying over this structure-capacity analogy to psychological performances two complicating factors appear. (1) We have so far been able to measure only a few of the gross structural variables, and these which have been measured are usually found to be relatively unimportant in relation to individual differences in performance.[4] We are therefore forced back to the hypothesis that performance is determined by some other biological factors as yet unmeasured, or even unknown. Capacity is thus usually an *inference* in psychology, not directly demonstrated. (2) Human organisms differ from the ordinary non-living machine in the fact that humans can function in *many* different patterns of movement, whereas the machine usually operates in only one pattern—or at most a few patterns and even then in quite definite ways. These modes of performance (behavior patterns, work-methods) are of varying appropriateness and efficiency, hence the human performance depends upon the *way of acting* as well as the individual's structural limitations. The importance of these variations in work-methods has so far been little recognized in differential psychology.[5] For the present, therefore, we must simply emphasize the fact that capacity cannot be accurately inferred from psychological performances until we know the importance of the third variable, work-methods. Differences in biological capacities have not then been demonstrated to be the major determinants of differences in performance, either by direct measurement or forced inference. Nevertheless it is advisable at least to consider some of the possible biological bases in order to know what to look for in further analyses.

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[3] The term *skill* is here used to represent any *degree* of efficiency in an act.

[4] The evidence for this lack of correspondence between structural and functional characteristics is reviewed by D. G. Paterson, *Physique and Intellect*, 1932, 304.

Effectiveness in performance may be correlated with individual differences in anatomical and physiological capacities of single cells, organs, and the body as a whole. Thus in skills emphasizing precision of movement the sensitive cells of the receptors may vary as to their physiologically determined thresholds for qualitative, intensive, and temporal aspects of stimuli. The sense organs, *e.g.* the eye, may vary as to the number and areal distribution of the sensitive cells, and in the structure as a whole as it produces freedom from distortion. The cells of the nervous system may also vary in thresholds due to their physico-chemical constitution, and the number, size, and specialized structure of fibers might also be important. The muscle fibers likewise vary in threshold of excitation, and the musculature as a whole may exhibit a relation between its delicacy in coördinated movements and its shape, the number, and proportions (cross section and length) of fibers. For the organism as a whole there might be certain reflex circuit mechanisms such as that giving postural balance, reciprocal innervation in antagonistic muscle balance, and basal metabolic rate.

In skills emphasizing speed, the same factors might be operating, plus a low refractory period and possibly larger numbers of cells to permit stronger and, therefore, faster stimulation of the effector movements. Skeletal leverage and the ratio of muscle-fibers to load moved may also be important here. Secondary factors might be the effectiveness of the nutritional, respiratory, and vascular systems in supplying energy and removing waste products. Glandular sufficiency is probably another important variable as an activating agent for the other tissues. In skills emphasizing strength, no additional factors would seem to be essential in the receptor and neural structures but the number and size of muscle-cells would probably be of greater importance.

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[5] A paper by the writer summarizes the widespread evidence for this hypothesis in all fields of individual differences, including the sensory, affective, and intellectual as well as motor processes. R. H. Seashore, Work methods: A neglected factor underlying individual differences, *Psychol. Rev.*, 46, 1939, 123-141.

It will be recognized here that nearly all of these correlations are speculative inferences from physiological experiments on excised fibers, or rough observations on the importance of gross body structures. They represent, at most, suggestions for numerous research problems rather than any well established biological foundation for differential psychology.

(*B*) *Specific training.* One of the earliest and most frequently confirmed observations on individual differences is that extended amounts of specific training on the skills themselves do not obliterate the differences, but may in fact increase their magnitude. Furthermore, the intercorrelations between initial and final standings on the learning curves of motor skills show a reasonably high degree of stability or reliability of the measures.

Examples are: the studies of simple and serial reaction reported by Farnsworth, Seashore, and Tinker,[6] the learning study in motor rhythm by Seashore;[7] studies on steadiness by Belton, Blair, and Humphreys,[8] and on tapping, spool packing and pursuit tests by Buxton and Humphreys.[9]

Even where the absolute improvement in a skill is relatively great, the relative improvement within the ranks of the group tested is frequently minor in nature. To summarize, differences in *amount* of training on a given skill have been shown to account for a part of the measured differences in attainment, but still leave a large proportion of the variance to be explained on other grounds.

(*C*) *Transfer.* As the next most logical explanation it has been argued that some of the variance not attributable to differences in amount of specific training may be due to transfer of training from qualitatively similar activities. Thus it might be expected that differences in speed of tapping and serial discriminative finger reactions might be traceable to training in typing, piano playing, and similar skills. In a prevous study the writer did not find this to be the case.[10] In motor skills, as elsewhere, transfer of training is usually found to be rather ineffective, especially for relatively short experiments on adult *Ss*.

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[6] P. R. Farnsworth, R. H. Seashore, and M. A. Tinker, Speed in simple and serial action as related to performance in certain "intelligence" tests, *Ped. Sem.*, 34, 1927, 537-551.

[7] R. H. Seashore, Studies in motor rhythm, *Psychol. Monog.*, 36, 1926, (no. 167), 142-189.

[8] W. Belton, J. R. Blair, and L. G. Humphreys, Effects of practice upon measures of steadiness, *Psychol. Bull.*, 31, 1934, 591.

[9] C. E. Buxton and L. G. Humphreys, The effect of practice upon intercorrelations in motor skills, *Science*, 81, 1935, (no. 2105), 441-442.

[10] R. H. Seashore, Individual differences in motor skills, *J. Gen. Psychol.*, 3, 1930, 38-65.

Several other considerations should be mentioned here. (1) On logical grounds, transfer might be more effective at earlier ages when each of the skills is just being introduced, and when the practice period is longer in proportion to the total of previous experiences in habituation to such activities. (2) Even in the case of adult *Ss*, a relatively low transfer effect may be cumulative from a number of sources and thus become important over a period of time. (3) As Cox has shown,[11] a given amount of time devoted to the acquisition of skill in a factory assembly line produced little or no transfer, whereas the same amount of time devoted to a *study of efficient work-methods* on such skills produced a very significant transfer to other industrial skills. Furthermore the transferred advantage continued to become even greater as further practice on the new skills continued. To summarize: transfer from ordinary types of rote training is rarely found to be important except when cumulative from a number of sources and over a considerable period of time. A more intelligent type of training on *work-methods* does, however, produce a significant transfer in even a short time, and may facilitate still further gains on the new activity with further practice. This, however, is a very specialized type of explanation which is important in itself, and is treated in the next heading.

(D) Work-methods. The importance of work-methods has recently been reviewed,[12] hence it may suffice here to summarize briefly the principles involved. A given organism having various biological capacities for work may utilize these capacities in a variety of work-methods, each method emphasizing the use of certain body structures and varying greatly from other methods in its degree of effectiveness. Thus a biological capacity which presents a physiological limit *for one work-method* may be of little or no importance for another work-method, as was pointed out long ago in the early learning studies of Bryan and Harter.[13] Following the selection of an initial work-method or set of work-methods, further progress may take place by the substitution of still more efficient work-methods or by the improvement of details through cue reduction and overlapping of component activities in the initial work-method. These methods are frequently not understood by the skilled performers who use them, but the applied sciences of industrial motion study and athletic coaching are well known examples of the efficacy of analyzing and training for the most efficient work-methods in their respective fields.

V. INTERRELATIONS OF ABILITIES

The term ability is here operationally defined to mean the present measured performance of a person under standard conditions of instruction, demonstration, and practice. It is simply what a person is *able to do* at a given time, usually after a practice-period long enough to approach maximum effectiveness. A good deal of information is available on such measurements and their intercorrelation. The results from various studies are reasonably consistent, but the interpretations are often at variance with the main body of data and in accord with certain preconceived hypotheses, *e.g.* belief in a single general (motor) ability, and ideas usually derived from or similar to those now current in intelligence testing. These theories as to the organization of human motor abilities are subject to statistical evaluation, and constitute the major interest of the present paper and of a second to follow.[14] We may now present a brief outline of the principal hypotheses and the most important types of evidence for and against each one. For every hypothesis considered we shall give (*1*) a general explanatory statement, (*2*) the type of evidence required for its substantiation, and (*3*) the actual evidence available.

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[11] J. W. Cox, *Manual Skill: Its Organization and Development,* 1934, 294.

[12] R. H. Seashore, *op. cit., Psychol. Rev.,* 46, 1939, 123-141.

[13] W. L. Bryan and Noble Harter, Studies on the telegraphic language: The acquisition of a hierarchy of habits, *Psychol. Rev.* 6, 1899, 374-375.

(A) General motor factor. (*1*) According to the hypothesis of a general motor factor there is a factor or set of factors common to all motor skills. Individuals who rank high in one skill tend to rank proportionally high in others. The nature of the factor is usually assumed to be a variation in some anatomical structure or physiological function or both.

(*2*) If this is true, there would be significant positive intercorrelations between scores on various tests of motor skill, above and beyond that which could be attributed to other factors such as intelligence, motivation, or other not strictly motor variables. A general factor should not be analyzable into group factors which partially overlap one another.

(*3*) The usual findings with tests of motor skills are that for a given age-range and with tests having reliability coefficients or $r\,\frac{1}{2} - \frac{1}{2} = 0.75$ to 0.95 (not stepped up), the intercorrelations cluster about the central tendency of +0.15 to +0.25. This degree of commonality is so low as to be insignificant, and in fact is not even as high as might be expected from motivational factors alone without any general motor factor. Correlations with intelligence test scores are usually insignificant,[15] whether negative or positive. Likewise the minute amount of commonality might equally well be attributed to slightly overlapping narrow group factors, such as are found by Thurstone's method of factor analysis.

As a variation of this hypothesis it might be assumed that children start out with a general motor ability which becomes gradually *differentiated* with further anatomical development and training, both formal and informal. This highly reasonable hypothesis is negated by the fact that in successive experiments on observers at the ages of 20, 18, 16, 10, and 6 yr., the writer and his co-workers[16] found practically the same low intercorrelations of the standard motor skills unit tests at all ages, while the data of Williams[17] on 2-yr.-old pre-school children show the same general result—specificity (low correlation between tests) at all ages from birth onward. Bayley's study of the first three years of life reports a slightly greater commonality during the first few months, with specificity later on.[18] Differentiation of skilled movements undoubtedly does occur, as can be shown in many learning experiments, but at least the differentiation apparently did not follow the same paths of differentiation in all people.

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[14] R. H. Seashore, C. E. Buxton, and I. N. McCollom, *op. cit.* (in press, this JOURNAL).

[15] R. H. Seashore, *op. cit.*, *J. Gen. Psychol.*, 3, 1930, 38-65.

(B) Group factors. (*1*) The hypothesis of group factors, sometimes called basic capacities, is that motor skills may be classified into subgroups (*e.g.* speed of simple reaction, speed of repetitive action, steadiness), each group having certain underlying factors in common.

(*2*) If such groupings exist we should expect to find from moderate to high intercorrelations between tests within a group, and lower intercorrelations between groups. The kinds of skills which are observed to fall within a group should give some indication of the basis for grouping (*e.g.* similarities in portions of body involved, or similarities in pattern of action). The size of the grouping might range from a factor so broad as to be almost a general factor, to one which is so narrow as to be hardly more than a specific factor, and groups might vary widely as to their size.

(*3*) The findings in support of this hypothesis may be classified according to the nature of the relationship being measured. Thus we may determine the importance of similarities in types of movement by varying that while holding constant the principal sense field involved as well as the musculature to be employed.

(a) Similar movements. (i) A group factor of simple reaction-times. Perrin,[19] studying 51 college students, reports a correlation of +0.59 ±.06 between simple reaction to a color stimulus and discriminative reaction to color stimuli, indicating the presence of a group factor covering these two tests. Farmer and Chambers,[20] studying small numbers of young factory women, found that simple reaction to light,

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[16] S. H. Seashore, The aptitude hypothesis in motor skills, *J. Exper. Psychol.,* 14, 1932, 555-561; R. Y. Walker and R. D. Adams, Motor skills: The validity of serial motor tests for predicting typewriting proficiency, *J. Gen. Psychol.,* 11, 1934, 173-186; Nancy Bayley, The development of motor abilities during the first three years, *Monog. Soc. Res. Child Develop.,* 1, 1935, 1-26.

[17] Williams, unpublished study at the University of Iowa.

[18] Bayley, *op. cit.,* 26.

[19] F. A. C. Perrin, An experimental study of motor ability, *J. Exper. Psychol.,* 4, 1921, 24-57.

[20] E. Farmer and E. G. Chambers, A psychological study of individual differences in accident rates, *Report No. 38, Brit. Ind. Fatig. Res. Bd.,* 1926, 1-446.

involving a short jump movement to the key correlated from +0.20 ±.16 to +0.83 ±.05 with discriminative jump movements to one of a number of keys. The simple pressing movement, however, correlated only +0.17 ±.16 to +0.47 ±.11 with the simple jump movement indicating that there may be two groups of simple reaction time factors. Slightly higher relationships were obtained by Seashore, Buxton, and McCollom.[21] As further evidence of the limited range of these groupings, it may be noted that simple reactions do not correlate with speed of tapping or with serial discriminative reactions involving the same sense field (vision) and the same musculatures, (right hand). This factor could represent then only a rather narrow group, far narrower than most psychologists have previously supposed.

(ii) Serial discriminative action. In a series of studies, Campbell found a correlation of +0.62 to +0.65 ±.06 between a test calling for arm movements of a baton to the corners of a diamond shaped opening, and a four finger-key test which also involved serial discriminative responses to visual stimuli.[22] The narrowness of this group factor is indicated by the finding of Hansen[23] and Robinson[24] that a four-finger serial discrimination-test employing typewritten letters on a regular typewriter did not correlate with speed of typing in classes. This is confirmed by the more extensive investigation of Walker and Adams[25] which showed that neither serial discrimination nor tapping, rotary wrist movement, Koerth pursuit, motor rhythm or spool packing were predictive of typing speed in high school typing classes (male commerce students) at the end of a year of training. Farnsworth, Seashore, and Tinker[26] also showed that the same serial discrimination test of finger action as above was not significantly related to scores on adult intelligence tests, while Beck found no relation with reading tests.[27]

(iii) Speed of tapping. To anticipate slightly the results of a later study, McCollom found evidence for two group factors in tapping tests.[28] The first included simple one-plane repetitive movements such as tapping with a stylus or on a telegraph key or on a free moving instrument called an oscillometer. The second factor included two more complex tapping tests in which a stylus touches two plates (5 in. apart) alternately, or three such plates in a triangular pattern. These movements were much larger, and involved two planes of movement and at least a minimum of accuracy in hitting the target plates. There was a distinctly lower correlation between the two groups of tests.

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[21] Unpublished.

[22] Malcolm Campbell, Instrument for measuring the serial reaction-times of large muscle groups, this JOURNAL, 46, 1934, 481-483; The "personal equation" in serial pursuit performances, *J. Appl. Psychol.*, 18, 1934, 785-792; The cognitive aspects of motor performances and their bearing upon general motor ability, *J. Exper. Psychol.*, 19, 1936, 323-333; An experimental evaluation of the significance of three factors involved in motor performances for general motor ability, *ibid.*, 19, 1936, 612-620. The term "general factor" as used by Campbell seems to be equivalent in meaning to our usage of the term "group factor."

[23] C. F. Hansen, Serial action as a basic measure of motor capacity, *Psychol. Monog.*, 31, 1922, (no. 140), 320-382.

[24] Unpublished study—cited by Hansen.

[25] R. Y. Walker and R. D. Adams, *op. cit.*

[26] P. R. Farnsworth, R. H. Seashore, and M. A. Tinker, *op. cit.*

[27] L. F. Beck, Relation of speed of reaction to intelligence, this JOURNAL, 44, 1932, 793-795.

[28] I. N. McCollom, An analysis of factors determining individual differences in speed of simple repetitive motions of the right arm, unpublished M.A. thesis, University of Oregon, 1932.

Both groups were related to a spool peg test in which large-holed, easily grasped spools were moved from one row of pegs to another. This test was intentionally designed to include a thumb and forefinger grasping movement similar to the oscillometer test and a horizontal movement of about the same distance and difficulty as in the two and three plate tapping tests.

If this result were representative of the predictability of a complex skill from simpler component activities it would afford great delight to students of differential psychology, and especially to personnel workers who could then apply the method to industrial and other situations and guidance problems.

Unfortunately, however, the result seems to be extremely limited in its significance. The various tapping tests were selected or devised to provide a nearly continuous series of slight variations in pattern of movements, so that it is not particularly surprising that they did show some relationship. The finding of two group factors instead of one should be in fact more surprising to us.

Further evidence of the limited significance of the group factors in tapping is shown by the following facts. (1) Simple tapping speed was only slightly related to tremor rate as measured in photographic recording of the movements of the extended forearm and finger. (2) Simple tapping is not related to simple reaction-time (numerous experiments). (3) The spool *peg* test was not related to a two handed spool packing test, or to any other of a large variety of high speed manual skills. (4) The negative results of the tapping test in S. H. Seashore's study of knitting-machine operation,[29] and Walker and Adams' study[30] on typing give further direct denial of the predictive significance of a tapping factor in industrial skills. (5) The lack of correlation with speed of turning a Miles hand drill indicates that tapping should not be given a name such as "motility test" if that implies any significant group factor covering all measurements of speed of movements.[31]

(iv) Pursuit coördinations. The writer found the following correlations of 50 college men for three eye-hand coördinations involving the pursuit of a moving point or target.[32]

Koerth pursuit-rotor *vs.* Miles pursuit-pendulum .... r = + 0.56
Koerth pursuit-rotor *vs.* Miles pursuit-meter .... r = + 0.29
Miles pursuit-pendulum *vs.* Miles pursuit-meter .... r = + 0.14

The one significant correlation here warrants further investigation to determine whether it represents a stable relationship, and if so, the limits of the tests which would fall within such a group factor. The Koerth pursuit-test has not been found to be significantly related to any other motor skills except in one possible instance reported by Buxton.[33] It was not significant in the previously mentioned knitting machine and typing studies.

(v) Motor rhythm. In a previously unpublished study, the writer found that scores on a motor rhythm-test did not correlate significantly with scores on the same apparatus when two other rhythm-patterns were used as stimuli. Seashore's original study on motor rhythm cited significant correlation with a test for discrimination of sensory rhythm-patterns.[34] This seems to conflict logically with the above finding

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[29] S. H. Seashore, *op. cit.*, 555-561.
[30] R. Y. Walker and R. D. Adams, *op. cit.*
[31] S. H. Seashore, *op. cit.*, 561.
[32] R. H. Seashore, *op. cit.*, *J. Gen. Psychol.*, 3, 1930, 38-65.
[33] C. E. Buxton, *op. cit.*
[34] R. H. Seashore, *op. cit.*, *Psychol. Monog.*, 35, 1926, (no. 167), 142-189.

of low correlations between patterns on the same type of motor coördination-test.

Two minor studies on the relation of the Seashore motor rhythm-test to dancing ability may be mentioned as indicative of the limits of the hypothetical group factor of motor rhythm. The first, performed by the writer's students at the University of Oregon, involved the selection of a group of fraternity students who were considered best able to judge the social dancing ability of campus girls. The 50 girls who were thus designated as distinctly superior were then given C. E. Seashore's phonographic test of sensory rhythm and R. H. Seashore's test of motor rhythm. In comparison with the norms for these tests the average of the superior dancing group was equivalent to the 65th percentile in the sensory rhythm-test, but there is some question as to whether the norms used do not give results which are consistently too high. The average score on the motor rhythm-test was equivalent to the 35th percentile. While this result is clearly contrary to expectations on the hypothesis of a group factor in motor rhythm, there may be several sources of error. In the first place, the men's criteria of selection may have been unduly influenced by such factors as beauty, conversational ability, and dress, and, as the girls pointed out, a girl follows her partner, not the music (!), in social dancing.

The girl selected as the most outstanding dancer (both social and interpretive types) also made the best score ever reported on the motor rhythm-test.

The second study, by Childers,[35] attempted to avoid the errors of selection involved in judgment of social dancing by studying individual tap dancing. The children studied, members of a private dancing class, were tested on a scale of tap routines of gradually increasing difficulty, giving a fairly accurate estimate of their attained proficiency. No significant relation appeared between the sensory or motor rhythm-tests and the dancing proficiency ratings.

(vi) Steadiness. According to the hypothesis of a group factor for steadiness, those coördinations which emphasize accuracy (precision or steadiness) while minimizing speed and strength should cluster together. Such steadiness-tests, in wide variety, should intercorrelate moderately or highly, and show correlations with speed and strength tests. Actually, Spaeth and Dunham[36] report that $r = +0.61 \pm .11$ between Dunlap's test for precision in thrusting a stylus at holes and rifle target shooting for 73 army men ranging from poor to expert marksmen. Extending this study, Seashore and Adams[37] found that on a multiple standard score of five steadiness-tests (postural sway with eyes closed, rifle muzzle sway when sighting, hand-tremor, stylus thrusting at holes, and stylus held stationary in holes) a university rifle team of 6 men was superior to all but one of 50 unselected fellow students enrolled in R.O.T.C. work. They also found that in spite of several rather low test reliabilities all test intercorrelations were +0.45 or higher. The same trends were verified in a subsequent study of Humphreys, Buxton, and Taylor in the same laboratory.[38]

The writer's earlier finding of an r of $+0.30 \pm .09$ between precision of thrust and steadiness in tracing a narrow V-slot may or may not be an exception to the above trend, depending upon the representativeness of the coefficient.[39] In view of the encouraging findings on steadiness, which are also corroborated by a moderate correlation between various types of postural sway in later studies,[40] the writer has been extending and improving a battery of such tests in order to determine more accurately the range in kinds of tests, the closeness of relationship, and the significance of this factor.

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[35] H. Childers, The possibility of improvement in motor rhythm through training in tap dancing, unpublished Master's thesis, Univ. So. Calif., 1937.

[36] R. A. Spaeth and G. C. Dunham, The correlation between motor control and rifle shooting, *Amer. J. Physiol.,* 56, 1921, 249-256.

[37] R. H. Seashore and R. D. Adams, The measurement of steadiness: A new apparatus and results on marksmanship, *Science,* 78, 1933, (no. 2022), 285.

[38] L. G. Humphreys, C. E. Buxton, and H. R. Taylor, Steadiness and rifle marksmanship, *J. Appl. Psychol.,* 20, 1936, 680-688.

Katz,[41] in a minor experiment employing one of the writer's new photo-electric devices for measuring steadiness of arm and hand, studied a group of 25 boys in the Whittier, California State School for Boys who were picked for "emotional instability," as evidenced by temper tantrums, and other emotional maladjustments. In comparison with 25 more institutional boys paired for age and IQ there was no significant difference on the steadiness test. Both emotional and muscular stability have been referred to a *neural* basis of unstability but whatever the merits of this theory, these results indicate that they obviously do not depend on the same factors.[41]

(vii) Gross muscular coördinations. Since the present series of studies deals primarily with only the finer skills, and especially those emphasizing speed and precision, we can only refer here to the work of Cozens,[42] McCloy,[43] and Wendler[44] on gross athletic skills and strength tests, to indicate the possible existence of one or more group factors in these areas. Our own data show no significant relationship between gross and fine motor speed skills.

We have thus found that in certain areas such as tapping, a relatively slight variation in the type of movement called for will produce very low correlations between reliable tests. Types of movement are thus indicated to be one of the variables by which group factors may be classified. The detailed study of such patterns constitutes the field of motion study, in which psychologists have barely begun to work and therefore cannot yet give more exact information on any of our tests.

(b) Variation of musculature. The experimental study of individual differences on tests where only the musculature was varied indicates quite clearly that persons who rank high on a given test with, say the right arm, will tend to rank proportionately high on the same test with the left arm, right foot, left foot, and even other musculatures whose structure is such as to make possible a similar action. There are of course *absolute* differences in the speed of musculatures, so that hands

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[39] R. H. Seashore, *op. cit., Psychol. Monog.*, 35, 1926, (no. 167), 142-189.

[40] H. G. Seashore and G. Koch, Postural steadiness under conditions of muscular tension and fatigue, *Psychol. Record,* 2, 1938, 319-332; R. H. Seashore, C. E. Buxton, and I. N. McCollom, *op. cit.* (in press, this JOURNAL).

[41] B. Katz, An experimental and clinical evaluation of the concept "nervous instability," *Psychol. Bull.*, 32, 1935, 538-539. It is not even certain that "emotional instability" represents anything more than a qualitative collective term. The study of Seashore and Katz on analyzing emotional adjustments into specific mechanisms indicates the possibility that such adjustments are interrelated very differently among different individuals.

[42] F. W. Cozens, The measurement of general athletic ability in college men, *Univ. Oreg. Publ., Phys. Educ. Ser.*, 1, 1929, (no. 3), 1-92.

[43] E. McCloy, Factor analysis methods in the measurement of physical abilities, *Res. Quart. Amer. Phys. Educ. Ass.*, 6, 1935, 81-85.

[44] A. J. Wendler, A critical analysis of test elements used in physical education, *ibid.*, 9, 1938, 64-76.

are faster than the feet, but the relative differences (in terms of ranks) are small. The correlations between musculatures are often on the order of $r = +0.60$ and higher as compared to test reliabilities of $r \frac{1}{2} - \frac{1}{2} = 0.80$ or higher with the same musculature.

This interesting fact, of which the first clues were given by Reymert,[45] cannot be further elaborated here except to mention the experiments in which it has been confirmed; namely, S. H. Seashore's study of simple reaction to an auditory stimulus,[46] Campbell's studies of serial discrimination to visual stimuli,[47] and the writer's studies of simple tapping and motor rhythm.[48]

C. E. and R. H. Seashore have also pointed out a possible explanation of these high inter-muscular correlations in the parallel finding of higher transfer of training between different musculatures (cross education) than between different patterns of movement with the same musculatures.[48] This of course raises the point as to why the differences in transfer occur, and our present hypothesis is that the similarities in *work methods* may be the common elements in both cases.

Strangely enough, these consistently high intercorrelations between musculatures afford but little cheer to practical-minded advocates of the hypothesis of biological capacities. It will easily be seen that if the aim is to predict the skill of the right hand in a given act, there is nothing to be gained by measuring the left hand, even when the correlation between them is high. It would be just as easy to measure the right hand in the first place, and unfortunately the significance of the measurement is usually limited to the specific skill or a narrow group of skills, as previously indicated.

(c) Variation of sensory field. Roughly speaking, the change from one sense field to another in producing the stimuli for a skilled response will affect individual rankings moderately. Thus Seashore, Buxton and McCollom find the following r's between various types of simple reaction-tests.[49] Simple 'press' reaction, vis. stim., *vs.* same with aud. stim. $= +0.55$ ($N = 50$). Simple 'jump' reaction (6 in.), vis. stim. *vs.* same with aud. stim. $= +0.78$.

The studies of Hansen[50] on serial discriminative finger movements on typewriter keys to four different stimuli showed that serial discrimination to spaced sound stimuli correlated only $+0.48 \pm .11$ with the same type of movement in response to visual stimuli (four different typewriter letters). Campbell reports correlations of $r = +0.37$ to $+0.51 \pm .07$ between visual and auditory stimuli on his previously mentioned baton movement test of serial discriminative responses.[51] The writer found insignificant correlations between visual and auditory stimuli on all three different patterns of his motor rhythm tests, but since the visual tests turned out to be too difficult for half of the fifty observers the results may not be statistically reliable.

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[45] M. L. Reymert, The personal equation in motor capacities, *Scand. Scient. Rev.*, 2, 1923, 177-194.

[46] S. H. Seashore, The personal equation in simple reaction-times (unpublished study with the writer).

[47] Malcolm Campbell, *opp. citt.*

[48] C. E. Seashore and R. H. Seashore, *Elementary Experiments in Psychology*, 1935, 1-219.

[49] R. H. Seashore, C. E. Buxton, and I. N. McCollom, *op. cit.* (in press, this JOURNAL).

[50] C. F. Hansen, *op. cit.*

[51] In an unpublished study.

(C) Specific factors. This view simply postulates the existence of certain factors which are specific to a single type of performance. No one questions the widespread existence of such factors in all types of tests, but their actual importance can only be determined by statistical evalua- of any general and group factors. The residual of whatever cannot be accounted for in these wider factors is presumably to be explained as due to the specific factors (plus errors of measurement as such, but not errors due to actual shifting of specific factors from one measurement to another). The detailed analysis of specific factors is the field of motion study experts, such as Gilbreth's classic example of the improvement of brick laying from 16 to 4 movements required per brick.

SUMMARY

Correlational analysis of individual differences in sensori-motor coordinations has indicated the existence of certain group-factors or areas within which tests are at least moderately related. Examination of the nature of these groupings of tests shows that the boundaries of the groups usually cut across those of *(1)* specific musculature and *(2)* specific sense-fields.

(1) Specific musculatures. The results with specific musculatures reveal (a) that various skilled actions of a given musculature are *not* significantly correlated; and (b) that similar skilled actions of different musculatures (*e.g.* hands and feet) *are* closely correlated, that is, it makes little difference relatively which musculature is used.

(2) Specific sense-fields. The results with specific sense-fields reveal (a) that various skilled actions involving a given sense-field are not ordinarily correlated (b) that similar skilled muscular reactions involving different sense-fields are usually at least moderately correlated, *i.e.* the sense-fields make some difference in relative standings but do not obliterate them; and (c) that studies of threshold activities of a single sense-field have shown in the case of audition that there is no single sense of hearing, but rather a series of nearly specific acuities (*e.g.* pitch, intensity, time).

Further examination of the groupings of such tests shows that the group-boundaries usually mark off a set of very similar *patterns* of movement (regardless of musculature), a *functional* rather than an anatomical grouping.

Additional evidence as to the nature and extent of these group-factors will be afforded by the intercorrelations and more elaborate methods of factor-analysis of two newer and more extensive experiments to be reported later.

SUGGESTIBILITY IN DIFFERENT PERSONALITY TYPES

By Bengt J. Lindberg, University of Lund, Sweden

The problem of suggestibility in different types of human beings and under varying experimental conditions is chiefly of interest to theoretical psychology. Questions relating to this subject are, however, of direct practical importance to the psychiatrist and in fact to every physician for, as is well known, a certain amount of suggestion enters into the treatment of most every disease. Indeed, in psychotherapy, suggestion is the chief aid of the physician. The results of the study reported here are, therefore, of practical as well as of theoretical significance.

In 1935, Ingvarsson and Lindberg published a report of an experimental investigation of suggestibility in neurotic subjects.[1] The material was divided into groups in accordance with Sjöbring's diagnostic principles,[2] which are: that the asthenic, the hysteric, and the manic-depressive (syntonic) personality, together with genuine intellectual inferiority, are minus variants of four fundamental constitutional factors; namely, *validity, stability, solidity,* and *capacity.* Thus, subvalidity (V—) means a constitutional asthenia; substability (St—), a syntonic; subsolidity (So—), a hysteric; and subcapacity (K—), a constitutional intellectual inferiority. Sjöbring's theory also admits of plus variants, thus numerous combinations are possible. All of them are thought of as being phenomena within different functional areas which vary independently in the biological disposition.

Method and Procedure

Test I. Ingvarsson and Lindberg's investigation revealed that some of the different diagnostic groups showed great dissimilarities in the result obtained. In that respect it was found that the simple method they employed was practicable. Their test, called henceforth Test I, was arranged so that the patient (*S*) was handed a tray with 6 glass-stoppered bottles together with a slip of paper on which were typed the words "essence of roses, cinnamon, vanilla, lavender, tar, cloves." *S* was also given written instructions to try to find out which of the odorous substances each bottle contained. He was urged to follow the directions very carefully and was

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* Accepted for publication February 1, 1939.

[1] C. G. Ingvarsson and B. J. Lindberg, Experimental studies of suggestibility in mental disturbances, *Acta Psychiat., Kbh.,* 10, 1935, 77-85.

[2] For an account of Sjöbring's *Individual Psychology,* see B. J. Lindberg, Experimental studies of colour and non-colour attitude in school children and adults, *Acta Psychiat., Kbh., Suppl.,* 16, 1938, 89-101; also Erik Goldkuhl, Persönlichkeitsanalyse und positive Neurosendiagnostik, *Acta Psychiat., Kbh.,* 11, 1937, 739-748; Psychische Insufficienszustände bei Oligophrenien leichteren Grades, *Acta Psychiat., Kbh., Suppl.,* 15, 1938, 1-481.

then left alone in the room. No time limit was fixed for the test. When *S* had written the result on the slip of paper provided, the test was finished. Only 2 of the 6 bottles contained one of the odorous substances mentioned. The *S*s reporting odor from an empty bottle were regarded as suggestible.

Although the test, arranged in this way, possesses certain advantages, *e.g.* freedom from the influence of the examiner, it has nevertheless its limitations. It does not give a record of the *S*s *immediate* reaction since it permits him to check the results as much as he likes. It may, therefore, be suspected that some of the *S*s, who at the first attempt thought they detected a smell but were unable to identify it, cautiously refrained from recording their first impression. In that way the test gave values of suggestibility that were too low.

Test II. In the present study, performed during 1936-1937, the method was modified in certain respects; the most important being that the test was carried out by *S* in the presence of the examiner (*E*) and under his direct supervision. The procedure under the new method, called Test II, was as follows. *E* placed 6 corked bottles of the same size in front of *S*, these were numbered from 1 to 6, and every one contained a pellet of cotton wool, but only bottles 2 and 5 contained an odorous substance. *E* began the test by saying: "You see here six bottles, and I want you to find out what odorous substances they contain. The odors are very faint and it will not be easy to recognize them. Here is a list of the substances concerned. Now read what is printed on the paper." (On the paper in front of *S* are typed the words "essence of roses, cinnamon, vanilla, onions, tar and cloves.") When *S* read the paper, *E* removed the cork from bottle 1 and handed it to him saying, "Please begin, but don't guess. What do you smell in this bottle?" As soon as *S* responded, *E* put the bottle to one side and continued with the other bottles in numerical order. If *S* asked any questions in the course of the test as to whether he has answered correctly, etc., he is told that any hints would spoil the value of the test.

It was originally intended to employ the test in an attempt to classify the degree of suggestibility, so that, for instance, an *S* who gave 4 incorrect answers would be characterized as more suggestible than one making only 1 error. The matter, however, was not quite so simple. Several considerations must be taken into account, as for instance the two odorous substances actually present in their respective bottles. It is conceivable that these odors may have an intensifying effect on the suggestibility of some *S*s, whereas in the case of others they tended to bring the *S*s back into the world of reality, sharpen their critical faculty and increase their resistance to suggestion. These problems seem to be worth studying more closely, but such an investigation would require more material than is at present available if we are to obtain a fairly reliable statistical treatment of all the groups involved. In the present work I shall, therefore, content myself, as in the previous investigation, with including in the suggestible group all those who stated that they detected a smell in any of the inodorous bottles, irrespective of the sequence of the erroneous answer. The two bottles in the series actually containing odorous substances play, nevertheless, an important rôle, as they prevent a doubter from telling his fellow *S*s that there is nothing at all in the bottles.

I am aware that in this study the group of 'suggestibles' is by no means homogenous. There are certainly several factors influencing the responses of the *S*s. Among other things, we have to reckon with the possibility that some of them simply guess and in that way state that they have sensations of smell which they do not really experience. The *Ss*, however, gained no advantage by simulating; they believed the test was designed to determine their sense of smell, and they were, of course, quite accustomed to undergo all kinds of test in the clinic. We may, therefore, venture to assert that those classified here as 'suggestibles' did really experience a sensation of smell from the objectively inodorous bottles.

Subjects. The *Ss* were obtained from the Psychiatric Department of Malmö General Hospital and from the Psychiatric Clinic of the University of Lund. They were chiefly cases of slight psychoses and nervous disturbances and all may be characterized as neurotic cases. In diagnosing at these clinics an endeavor is made to arrive at a characterological diagnosis in accordance with Sjöbring's principles. Thus, in addition to the customary clinical diagnosis, we tried to determine the type of personality to which every *S* belonged. Practically speaking, it was only a question of determining the minus variants included in Sjöbring's system. Besides, mixed types there also occurred, as subsolid-subvalid types, and, from a theoretical point of view, plus variants, *e.g.* the superstable type, although they are not yet sufficiently defined symptomatologically.

Mixed types are more difficult to diagnose correctly than the pure types. Moreover, the various individual characteristics of the former frequently produce very unexpected effects, which may render the interpretation of the experimental results more difficult. On that account I have selected from my original material, comprising 365 cases, those types of personality which are as pure as possible, that is, *Ss* in whom *one* of the 4 minus variants predominated. The number of such cases, which form the basis of the following analysis of suggestibility, was 159:

Of the Malmö material I have included only those *Ss* having only one of the signs for subsolidity (So—), subvalidity (V—), substability (St—) or subcapacity (K—) in their diagnosis. As I have previously shown,[3] the diagnostic principles at the Lund Clinic are much more subtle; importance being attached even to very insignificant characteristics, especially with respect to subvalidity. Therefore, in the Lund material I have also included *Ss* with combined diagnoses, in which a *slight* subvalidity is indicated in addition to the dominating character.

In order to make the work as accessible as possible I shall use below the familiar designations *hysteric, asthenic, syntonic,* and *oligophrenic* personality. This is done with the express understanding that I attach to these variously defined concepts only the meaning ascribed to them in Sjöbring's system of individual psychology. Thus, hysteric will here be considered as equivalent to subsolid, asthenic to subvalid, syntonic to substable, and oligophrenic to subcapable.

RESULTS

(a) Comparison of the different groups of Ss. Table I gives a survey of the suggestibility of the different groups. It will be clearly seen that there is a very good agreement between the corresponding groups. I shall, therefore, combine the Lund and Malmö *Ss* and regard them as a single unit. Thus it will be possible to obtain figures large enough to permit of a statistical analysis.

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[3] *Op. cit., Acta Psychiat., Kbh., Suppl.,* 16, 1938, 109.

In the first place the number of oligophrenics is very small, not more than 12 in all. All of them gave incorrect answers. This 100% 'suggestibility' agrees very well with the observations recorded in the literature that the degree of suggestibility is greater in a lower degree of intelligence. As in the previous investigation, the oligophrenics in the present material also exhibit the highest value of all groups. One must be extremely cautious, however, in the interpretation of this result. Oligophrenics have of course more difficulty than other groups in understanding the instructions given in the test and are unable to master the situation as well. Their incorrect replies may, therefore, be explained in part at least by their having misunderstood what was required of them. The oligophrenics occurring in the material are, it is true, of a mild form but their results should not be compared with those of other groups. In the following investigation, therefore, the intellectually defective have been entirely excluded.

TABLE I

Distribution of Suggestible Cases in Different Types of Personality

Personality type	Sex	Lund Ss			Malmö Ss		
		No. Ss	No. sug.	%	No. Ss	No. sug.	%
Hysteric (subsolid)	male	2	2	100	10	10	100
	female	4	3	75	14	11	79
Asthenic (subvalid)	male	29	17	59	16	11	69
	female	27	13	48	11	6	55
Syntonic (substable)	male	7	3	43	11	5	45
	female	8	2	25	8	2	25
Oligophrenic (subcapable)	male	3	3	100	3	3	100
	female	0	—	—	6	6	100

From Table II it will be seen that there is a difference between the results of the men and women; the men constantly showing a somewhat higher percentage of suggestibility. This difference has also a certain statistical significance, as the mean difference in the three groups is 17.8 ± 7.4%, *i.e.* 2.3 times its standard error. In the case, however, of the *S*s (84 men and 80 women) examined at the Surgical Department of Malmö General Hospital, which were employed as comparative mate-

TABLE II

Percentage of Suggestible Men and Women Among the Different Personality Types

Personality type	Men	Women	Diff.
Hysteric	100	77.8	22.2±12.8
Asthenic	62.2	50	12.2±11
Syntonic	44.4	25	19.4±15.9
		$M_{diff.} = 17.9 \pm 7.4$	

rial, conditions were reverse. In that material 59.5% of the men and 71.3% of the women were found to be suggestible. The difference in this case is −11.8 ± 7.4%. As no psychiatric examination of this material, with a grouping according to the types of personality, was made, the values are of no great significance in discussing the problem of the possible difference in the degree of suggestibility in men and women. The material must be further increased before this problem can be statistically evaluated. An interesting point in this connection is that the values obtained in the 1934 investigation (Test I) were approximately the same for both sexes. We must content ourselves, therefore, with recording that no great difference could be observed between the mode of reaction of men and women.

From Table II it will be further seen that the number of suggestibles among both sexes is highest in the hysteric group and lowest in the syntonic group. The difference between these two extreme values is statistically significant for both men and women separately. For the men the difference is 55.6 ± 14.4% and for the women, 52.8 ± 14.6%. The mean difference between the percentage of suggestibility in all hysterics and syntonics is not less than 54.2 ± 10.2%, thus more than 5 times the standard error.

The percentage of suggestibility in the asthenic group is intermediate between the values of the two extreme groups. There is also a statistically significant difference between the values for the asthenics and those for the subsolids, the mean difference being 32.8 ± 8.4%. On the other hand there is also a probable statistically significant difference as compared with the syntonic group, the difference, in spite of the low figures, being 21.4 ± 9.7%. We are thus able to establish that in this test the three groups (hysteric, asthenic, and syntonic personality) were found to react in a characteristic manner: the hysterics showing the highest percentage of suggestibles, the syntonics the lowest percentage, and the value for the asthenics being intermediate between these two extremes.

(b) Comparison of Tests I and II. As pointed out above, the new method should theoretically bring about an increase in the number of suggestibles. Table III gives the value obtained with the two tests (Test I and Test II).

The two groups of *Ss*, it should be pointed out, are not exactly comparable. In the 1934 investigation (Test I) the *Ss* were diagnosed with respect to the *type of disease* (hysteria, asthenia, manic-depressive insanity, etc.), whereas in the Lund investigation (Test II) the diagnosis referred to the *type of personality.* Generally speaking, however, the diagnoses coincide. A further dissimilarity is that the 'normal' *Ss* in Test I originated from both the surgical and the medical departments while Test II was made only on *Ss* from the surgical department. That Test I was made in other clinics than Test II is of little significance, as the *Ss* at both clinics are homogenous.

It will be seen that all the values obtained in the earlier investigation are lower. In both tests the oligophrenic group shows the highest percentages, the next being the hysteric group. Then the order is reversed in that the asthenics, which showed the lowest value in 1934, have in this investigation a higher percentage than the syntonics.

TABLE III

PERCENTAGE OF THE VARIOUS TYPES OF DISEASE SUGGESTIBLE IN TEST I (1934) AND TEST II (1936–1937)

Disease	Test I	Test II
Hysteria	48	86.7
Asthenia	7	56.6
Syntonia	29	35.3
Oligophrenia	86	100
Surg. & med.	27	65.9

The cause of this constant difference is most likely due to the modification in the method. In the later investigation the patients were tested on their immediate spontaneous reaction, while in Test I they were able to correct their results. That the relatively greatest increase in the number of suggestibles is shown by the asthenic group is easily explained. The asthenics are characterized, among other things, by their pedantic turn of mind, their punctiliousness and frequently scrupulous fear of making a mistake. In Test I the asthenics were able quietly to test the different bottles and compare them with each other and it is, therefore, quite natural that the more they tried to control the results the more uncertain they became. That is undoubtedly the reason why the majority of them, after careful consideration, finally fastened only on the bottles which gave an evident, unmistakable odor. In Test II, this criticism cannot tell to full advantage. Being unable to make any comparison and to revise their first reaction, the asthenics did their utmost to detect the 'weak odor' and finally, when they experienced a slight odor, gave their answers. It is remarkable how the ambition of the asthenics showed itself in this test. They took the task more seriously than any of the other groups, they sniffed and cleared their noses before the test began, turned and twisted the bottle, held it in different positions in front of their noses, and did everything to give the correct response.

The hysterics, on the other hand, were quicker in their responses, many of them smelt the bottle only for a moment and then gave their replies loudly and clearly. In this group, too, the opportunity of revising their results, allowed in Test I, evidently put a check on their spontaneity.

Finally, as regards the syntonics the difference in the method seems to have played the least rôle. In both tests about a third of the *S*s reacted to suggestion. Perhaps some difference would also bave been discernible in these groups if the material had been larger, but it is also conceivable that the syntonic mode of reaction is not influenced by minor differences in the method. As far as can be judged, there seems to be justification for the view that syntonics are more realistic than any other type of personality. They experience the world concretely and realistically. They are evidently not so easily suggestible. If they are brought under the influence of suggestion, the suggested experience has such a realistic value to them that they have no need of reconsidering their decision; if they detect the smell of onions in a bottle, then of course there is a smell of onions, and there is nothing more to think about. This attitude seems to offer a probable explanation of the reaction of the syntonics in this test.

We are thus able to discover a difference in the mode of reaction made in Tests I and II. All groups in Test II show a higher percentage of suggestible *S*s. This difference is probably due to the change in method. In the case of the asthenics in particular, a considerable difference can be recorded in the two methods while the syntonics show the least change. This different sensitivity to the variation in the method may be associated with certain characteristic features—of these two types of personality.

(c) Relation to bodily types. As compared with the other groups, the syntonics showed a much greater resistance to suggestion. Now, if this result is due not only to external factors but also to constitutional characteristics, we are justified in expecting a certain correlation between suggestibility and bodily type. As is well known, it has been demonstrated in various ways that syntonic characters are intimately connected with the bodily type that Kretschmer calls the pyknic type.

Anthropometric determinations, were, accordingly made of most of the *S*s from the Psychiatric Department of Malmö Hospital. Strömgren's index system, recently introduced, was used.[4] A plus-index $\geqq +0.3$ is considered here to denote positive pyknic type; a minus-index ≦ —0.3 denotes leptosomic type; and an intermediate value of about ±0 furnishes no sure indication of the bodily type. On the basis of these three indices, the *S*s were divided (Table IV) into 'pyknics,' 'leptosomes,' and 'indetermined' bodily types. The pyknics have the lowest percentage of suggestibles, 30.4%, while the other two groups have almost exactly the same high values, 77.4% for the leptosomes and 77.8% for the indeterminate group. We thus see that there is no difference between the leptosomes and the group that is anthropometrically uncertain in this respect. Consequently it appears as if the pyknic bodily type is the only one here to represent a definite psychological mode of reaction, viz. a strong resistance to suggestion. Although the leptosomes have a high degree of suggestibility they do not represent a purer type of suggestibility than the indeterminate group. The differences between the value for the pyknics and that for each of the other two groups are significant statistically. The difference between the percentages of suggestibility in the pyknic and leptosomic groups is 47 ± 10.7% and between the pyknic and the indeterminate group 47.4 ± 11.9%. These results indicate that suggestibility is a constitutionally established factor in personality, and they confirm our inference that the syntonic type of personality is the most resistent to suggestion.

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[4] Erik Strömgren (Über anthropometrische Indices zur Unterscheidung von Körperbautypen, *Zsch. f. d. ges. Neurol. u. Psychiat.*, 159, 1937, 75-81) has constructed an ingenious table by means of which it is possible to read directly whether a person belongs to the pyknic or the leptosomic bodily type on the basis of his bodily height and the width and depth of his chest. In a test (*op. cit., Acta Psychiat., Kbh., Suppl.*, 16, 1938, 129), I found a close agreement between my somatoscopic diagnosis of bodily types and Strömgren's index, thus furnishing further confirmation of the view that his indices are really expressions of those two bodily types of Kretschmer.

TABLE IV

PERCENTAGE OF SUGGESTIBLE Ss IN DIFFERENT BODILY TYPES

	Pyknics	Leptosomes	Indeterminate
Index	≧+0.3	≦−0.3	+0.25 to −0.25
No. Ss	23	84	36
% sug.	30.4	77.4	77.8

In an earlier study in which I investigated the suggestibility of *S*s with and without a 'color attitude,'[5] I found that the 'color reactors,' *i.e.* those possessing a 'color attitude,' were the least suggestible of the two groups and that they were most numerously represented among the syntonics. Those results corroborate our present findings and support the view that there is a relationship between syntonic personality (substability) and a relatively strong resistance to suggestivity.

(d) Relation between olfactory and visual suggestibility. In the study on 'color attitude,' the color sensitivity of the *S*s was tested with Boström's "tabulae pseudo-isochromaticae." This test[6] is similar to Ishihara's and to Stilling's. It consists of a series of charts, the first twelve of which

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[5] *Op. cit.*, 137.

[6] C. G. Boström, *Tabulae pseudo-isochromaticae*, 1935, 1-20.

have figures in other colors than those of the background that may easily be read by persons with normal color vision. In addition to these 12 charts there are others without figures but with similar mosaic patterns and backgrounds. These cards are used in color-tests for the purpose of detecting dissimulators. They may also, as my experiments have shown, be used in tests of suggestibility. My procedure was as follows. After presenting the 12 charts with figures to *S*, he was presented with Chart 13—which was without figures. In most of the trials the *S*s responded correctly, *i.e.* they replied that the chart did not contain a figure, but in some of them the *S*s reported figures. It is possible, of course, that some of the *S*s were simply guessing, but I received, in giving the test, a definite impression that the *S*s giving figures really thought that they saw the figures they reported. I am of the opinion that their 'optical illusions' were due to suggestion and that the suggestive factor in their cases was the constant occurrence of figures in the first twelve charts.

TABLE V

Distribution of Olfactory Suggestibility Among the Visual Suggestible and the Visual Non-Suggestible Cases

Cases	Visual non-suggestible			Visual suggestible		
	No. *S*s	olf. sug.		No. *S*s	olf. sug.	
		No.	%		No.	%
Surgical	92	59	64.1	14	13	92.9
Psychiatric	44	34	77.3	12	12	100
Total	136	93	68.4	26	25	96.2

Diff. = 27.8 ± 5.5%

Of 162 *S*s examined with both the smelling- and sight-tests, only 26 (16%) yielded to visual suggestion while 118 (75%) were suggestible in the smelling-test. Now, if the report of figures in Chart 13 was really an expression of a suggestible tendency, then we should expect those *S*s to be susceptible to suggestion in the smelling-test. And such was found to be the case. Of the 118 *S*s suggestible in the smelling-test, 25 (22.2%) were also suggestible in the color-test. Of the 44 *S*s not suggestible in the smelling-test, only 1 (2.3%) reported a figure in Chart 13. The difference between the two values, 19.9 ± 4.4%, is significant statistically as it is more than 4 times its standard error.

Unfortunately, the number of *S*s with personality diagnoses examined with the visual test is much too small to permit of a division into groups on psychological principles. Several of the cases originate from surgical departments and have, therefore, not been diagnosed psychiatrically. All cases are summarized in Table V, which shows that of the *S*s visually suggestible 92.9% of the surgical and 100% of the psychiatric were also olfactorily suggestible, and that of the *Ss* not visually suggestible only 64.1% of the surgical and 77.3% of the psychiatric were olfactorily suggestible. The difference between the percentages of olfactorily suggestible in both groups, 27.8 ± 5.5%, is statistically reliable.

There appears to be a connection between the tendency to give incorrect responses in the smelling-test and the tendency to discern a figure in the unfigured chart of the color-test. The conclusion seems warranted, therefore, that the olfactory and visual tests reveal deep-seated aspects of personality which seems to be associated with a common factor which I have called "suggestibility."

SUMMARY

The results of this study may be summarized as follows.

(1) Out of 365 cases of slight mental disturbances examined, in which the type of personality was determined by Sjöbring's method, an account is given of the results obtained in 159 cases in which only one of the 4 minus variants predominates. In these 4 'pure' types of personality the greatest number of suggestibles was found among the oligophrenics (subcapables) 100%, then come the hysterics (subsolids) with 86.7%, the asthenics (subvalids) with 56.6%, and finally the syntonics (substables) with 35.3%.

(2) In the psychiatric material the males show a slightly higher percentage of suggestibles than the females, but in the surgical material (164 cases) the results were just the reverse; hence the only conclusion to be drawn is that there is no difference in these tests between the mode of reaction of men and women.

(3) The new method of olfactory testing (Test II) gives throughout higher values of suggestibility than the old method (Test I). In both of my investigations, the highest and next highest percentage were found among the oligophrenic and hysteric groups respectively. On the other hand, in the 1934 investigation (Test I), the syntonics showed higher values than the asthenics. These differences in the results were expected and may be explained as a consequence of the differences in the methods.

(4) Suggestibility is associated with bodily type in such a manner that the pyknics (determined in accordance with Strömgren's anthropometric index) have the lowest percentage of suggestibles.

(5) The incorrect answers in the smelling-tests appear to be mutually related to certain incorrect responses in color sense examinations with Boström's pseudo-isochromatic charts. These erroneous replies are assumed to be expressions of visual suggestibility.

(6) The correlation between the olfactory and visual tests indicate that here we have to do with a quality established in the structure of personality, which we have called suggestibility. The connection between bodily type and suggestibility also indicates that suggestibility (or resistance to suggestibility) is, partly at least, constitutionally conditioned.

THE MOON ILLUSION AND THE ANGLE OF REGARD

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To the erect observer the moon appears large at the horizon and small in culmination. This discrepancy in the appearance of a constant stimulus-object has interested philosophers from antiquity, and the history of its consideration has been given elsewhere.[1] That the phenomenon is subjective is demonstrated at once by viewing the horizon moon with the head and eyes in the position which they assume when a standing observer with head inverted regards the horizon between his legs: the moon then seems to be small. The ratio of the diameter of the moon in culmination to the diameter of the moon at the horizon has been found in some experiments to be about 1:2.5, a measurement which, however, includes considerable variation.[2] Zoth's theory has been that the illusion depends upon the position of the eyes,[3] and this view has recently received explication and support by Schur.[4] She showed with artificial moons (projected circles of light) that the amount of the illusion depends directly upon the distance of the stimulus-object. For her the amount of the illusion was measured by the zenith-horizon ratio as 1:1.2 at a distance of 4 m., and it shifted to the ratio 1:2.0 at 33 m., changing by a continuous function that was approximately asymptotic to the ratio 1:2.0. It thus becomes obvious why the illusion is normally astronomical: there are no objects other than the sun, moon and stars which are usually seen by the observer at large constant distances and at greatly varying elevations.

The present paper is a first report of observations which substantiate the generalization that the illusion, occurring for distant objects, is primarily dependent upon the direction of regard—upon the posture of the head and eyes. We have devoted this paper principally to establishing the fact that the illusion depends upon posture, and thus, following the example of our predecessors, we have not separately controlled head and eye movements; but we hope to present such an analysis in a later paper.

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[1] E. Reimann, Die scheinbare Vergrösserung der Sonne und des Mondes am Horizont, *Zsch. f. Psychol.*, 30, 1902, 1-38, 161-195.

[2] F. Pozděna, Eine Methode zur experimentellen und konstructiven Bestimmung der Form des Firmamentes, *ibid.*, 51, 1909, 200-246.

[3] O. Zoth, Ueber den Einfluss der Blickrichtung auf die scheinbare Grösse der Gestirne und die scheinbare Form des Himmelsgewölbes, *Arch. f. d. ges. Physiol.*, 78, 1899, 363-401.

[4] E. Schur, Mondtäuschung und Sehgrosskonstanz, *Psychol. Forsch.*, 7, 1925, 44-80.

ASCENDING MOON: ERECT POSTURE

Our observations of the full moon were made from the roof of the Harvard Biological Laboratories where there is an unobstructed view of an eastern horizon.

O sat erect in a chair, facing the moon (with the median plane of the head coinciding with the vertical plane through the moon). *O* controlled a small projection lantern which projected a circular disk of light on a vertical white screen (6 x 6 ft.), placed at one side 3.5 m. away, with its center about at the level of *O*'s eyes. This lantern held a film with circular apertures developed upon it, ranging in 50 successive frames from a size that would give an image of 3-cm. diameter on the screen to

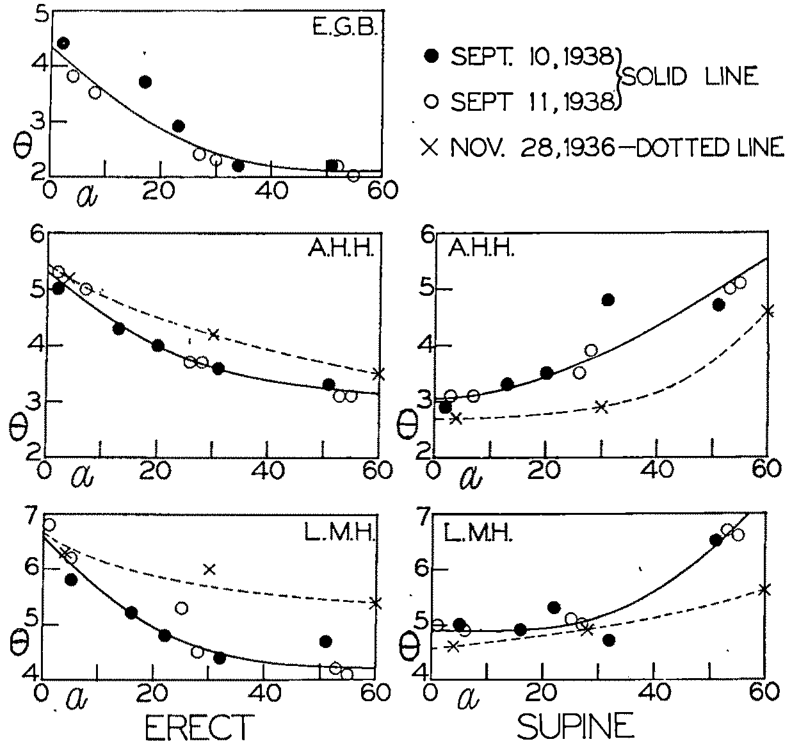

FIG. 1. APPARENT SIZE OF THE MOON AS A FUNCTION OF ITS ELEVATION FOR ERECT AND SUPINE OBSERVERS

α = elevation of moon above horizon in degrees. Θ = diameter of comparison stimulus (circle of light projected on screen 3.5 m. from *O*) in degrees of visual angle. Observers: E.G.B., A.H.H., L.M.H. Erect posture: *O* is seated, body erect, facing the moon, head and eyes elevated as necessary. Supine posture: *O* lies on back, head toward moon, head and eyes thrown back so as to view the moon. The supine position approximately reverses the posture for the erect position, and thus reverses the illusion.

an image of 30-cm. diameter. The diameters for the 50 frames varied approximately in a geometrical series, and the intervals between successive images were small. *O* equated the apparent size of the image on the screen to the apparent size of the moon, using binocular regard, looking back and forth from image to moon, and moving the film back and forth so as first to bracket the correct equivalent size and then to make the 'equation' exact. This final adjustment was a little difficult since it involved the comparison of two sizes at different distances, but the bracketing procedure enabled *O* constantly to reëstablish his certainty by viewing at will sizes that were obviously too small and obviously too large. Usually *O* was able quickly to narrow his choice to two, or at most three, successive frames.

The moon itself subtends a visual angle of about 31′. It varies almost 2′ in either direction from this mean as its distance from the earth changes. We measured the elevation of the moon from the horizon by a *porte lumière,* checking the angles by reference to tables, and we reoriented *O* and the apparatus as the moon southed during its ascension.

The two authors (A.H.H. and E.G.B.) and Dr. L. M. Hurvich (L.M.H.) acted as *Os*.

The determinations of change in apparent lunar diameter are shown in the left half of Table I and the left half of Fig. 1. For the erect *O,* as the altitude, α, of the moon increases, its apparent size, Θ, the angle subtended by the diameter of the equivalent comparison stimulus, decreases. The ratio of the diameter of the comparison stimulus for the moon at the horizon to that for the moon near culmination ("Max. ratio" in Table I) ranges from 1.23 to 2.00. Fig. 1, with its smoothed curves, shows the trend. Each reported value of Θ is an average of 5 observations, and the critical ratios between the first and last point of each function are large(only one is less than 18).

These results are for observations of the full moon on the evenings of September 10 and 11, 1938. In Fig. 1 other data for A.H.H. and L.M.H. on November 28, 1936 (dotted lines and crosses) are added. When all the data are considered together, consistency is seen to be high. The results of 1938 are consistent with the results of 1936, even in respect of the individual differences between the two *Os*, both for the apparent size of the moon and for the 'maximal ratio.' For all three *Os* the functions of September 11, 1938, approximately repeat the functions for the preceding night, and observations are consistent for data taken a few minutes apart (cf. for E.G.B., Sept. 11, 3 with 8°, 27 with 30°, 52 with 55°, and similarly for the other *Os*).

It is seen that these data are consistent with the view that the angle of regard is the chief determinant of the apparent size of the moon, although the elevation of eyes and head were not separately controlled. The eyes can be elevated from the primary position only a little more than 40°, but the moon itself, at latitudes north of 40°, is not often seen at more than 60° above the horizon. Thus the "horizon-zenith" illusion, as it is sometimes called, concerns a difference between no elevation and an elevation which is ordinarily so much less than 90° as to be not much greater than the amount the eyes can be raised without movement of the head. In the present experiment the regard of the moon at its 60°-culmination must have involved at least 15° bending of the neck and quite possibly more.

TABLE I

Apparent Size of the Moon as a Function of Its Elevation for Erect and Supine Os

α=angle of elevation of moon from horizon in degrees. θ=visual angle in degrees subtended by comparison stimulus at 3.5 m., equated in size to moon. σ=standard deviation of the distribution of θ. Erect posture: *O* seated erect, facing moon. Supine posture: *O* lying on back, head toward moon. Max. ratio=ratio of θ's for maximal and minimal α's. Every value of θ is an average of 5 observations.

S	Erect						Supine					
	Sept. 10, 1938			Sept. 11, 1938			Sept. 10, 1938			Sept. 11, 1938		
	α	θ	σ	α	θ	σ	α	θ	σ	α	θ	σ
	2	4.4	.09	3	3.8	.15	(E.G.B. did not make the observations in the supine position)					
	17	3.7	.11	8	3.5	.07						
E.G.B.	23	2.9	.22	27	2.4	.12						
	34	2.2	.07	30	2.3	.09						
	51	2.2	.13	52	2.2	.13						
				55	2.0	.08						
	Max. ratio=2.00			Max. ratio=1.90								
	2	5.0	.20	2	5.3	.08	2	2.9	.19	3	3.1	.09
	13	4.3	.17	7	5.0	.20	13	3.3	.17	7	3.1	.08
A.H.H.	20	4.0	.12	26	3.7	.11	20	3.5	.11	26	3.5	.14
	31	3.6	.18	28	3.7	.15	31	4.8	.14	28	3.9	.20
	51	3.3	.07	53	3.1	.06	51	4.7	.33	53	5.0	.10
				55	3.1	.14				55	5.1	.15
	Max. ratio=1.52			Max. ratio=1.71			Max. ratio=1.62			Max. ratio=1.64		
	5	5.8	.26	1	6.8	.19	5	5.0	.25	1	5.0	.15
	16	5.2	.26	5	6.2	.19	16	4.9	.34	6	4.9	.09
L.M.H.	22	4.8	.26	25	5.3	.08	22	5.3	.32	25	5.1	.26
	32	4.4	.31	28	4.5	.23	32	4.7	.09	27	5.0	.25
	51	4.7	.06	53	4.2	.34	51	6.5	.11	53	6.7	.27
				55	4.1	.25				55	6.6	.18
	Max. ratio=1.23			Max. ratio=1.66			Max. ratio=1.30			Max. ratio=1.32		

These judgments involve what has been called the "size-constancy" phenomenon. The moon, about 240,000 miles from *O*, subtends an angle of about half a degree. The equivalent stimuli, 3.5 m. from *O*, subtend angles that vary from 2.0° to 6.8°, that is to say, their retinal images are linearly from four to twelve times as large as the images for the moon.

All the images for the near object are larger; none is smaller. When a perceived object changes from far to near, its apparent size increases less rapidly than the retinal image. Conversely, if the apparent size remains the same, then the retinal image of the near object must be larger than the retinal image of the far object.

In this connection it is interesting to note that the artificial moon in use at the Hayden Planetarium in the American Museum of Natural History is made to subtend an angle of 1°, twice as much as the real moon, and it still looks too small to many people.

Ascending Moon: Supine Position

If the elevation of regard from the primary position is the essential condition of the moon illusion, then the phenomenon ought to be reversible. For this reason we placed *O* supine, lying on his back with his head toward the moon.

To view the horizon moon from this position is difficult. Since the eyes can be raised only about 40°, the *O* must assume the awkward position of bending back his neck and shoulders about 50°, so as partly to invert his head. The task was facilitated on the second night (Sept. 11) by the use of a cot over the end of which the head hung down. At neither time was E.G.B. able to assume this position without nausea, so that observations from him in the supine position had to be abandoned.

The data for A.H.H. and L.M.H. are given in the right half of Table I and the right half of Fig. 1. It is apparent at once that the illusion is reversed under these conditions. The comparison stimulus is small for the moon at the horizon and large for the moon in culmination. It is just about as small for the horizon with supine posture as it is for culmination with the same *O* in the erect posture; and, conversely, the moon in culmination for the supine position is equated by this measurement to the horizon moon for the erect position. The 'maximal ratios' for the supine posture are of the same size as the ratios for the erect posture.

All the indicators of consistency in the data for the erect posture exist with equal reliability in the data for the supine posture. There can no longer be any doubt that the illusion depends primarily upon the orientation of *O* to the moon, and is essentially independent of physical variables that lie outside of the observing organism.

The only exception to this generalization lies in the fact that the supine *O* is not so impressed by the large size of the moon at culmination as is the erect *O* by the large size of the horizon moon. Measurement shows that the supine *O* requires just as large a comparison stimulus for the equivalent of the moon in culmination as the erect *O* requires for the horizon moon. Unless the supine *O* also sees the comparison stimulus as smaller than the erect *O* sees it, in spite of the fact that his positional relation to it is about the same, then the two moons must be the same apparent size. Perhaps here the much discussed effect of adjacent objects plays a rôle, and the horizon moon and its comparison equivalent are contrasted with relatively small objects, whereas the moon in culmination and its comparison equivalent are compared only with the huge spaces of the heavenly vault. Such a contrast effect could enter into an 'absolute judgment' of size, without altering the comparative judgment.

MIRRORED MOON

Gauss suggested that the moon illusion be tested by projection of the mirrored image of the moon to different parts of the sky, and many persons, including Helmholtz, have tried this experiment with varying success. If the large horizon moon be viewed in an ordinary mirror held close at hand, it loses its size and appears quite small. That is the common experience. It has been noted, however, that the illusion may be observed if the mirror is distant;[5] and it is obvious that the reflection ought to provide the illusion if the mirror can be made irrelevant to the perceptual situation, and that the mirror ought to be irrelevant if it is invisible.

We employed as a mirror a glass plate, 6 x 6 x ½ in., worked to an optically flat surface and aluminized on the front surface from edge to edge. It was mounted on a metal plate with narrow blackened clips at the edge, and it was supported by a long blackened rod. This mirror was used in the bright light of the full moon, on the evening of October 9, 1938, on the roof of the Biological Laboratories. At a distance of a half a meter the rod and frame of the mirror were quite obvious, and the mirror surface itself, although, being front-aluminized, it provided no visible glass, nevertheless seemed to be projected to the plane of its frame. At 5 m., however, when reflecting the image of the moon, the mirror and its supports disappeared in the darkness of the night, which became especially dark by contrast to the brilliant image of the moon. Under these circumstances the illusion, measured by the apparent size of the same comparisón stimulus as before, was as striking as for the moon seen directly.

The upper half of Table II shows what happened with the mirror only about 0.5 m. away from *O* for elevations of 0° and 90°. This observation was difficult because the mirror was visible. *O* found that he tended to adjust his vision to the distance of the mirror, an adjustment which did not cause diplopia for the image of the moon. With effort he could manage to gaze through the mirror to a distant moon, and, when regard thus became more distant, the apparent size of the moon increased. The observations in the table are all for such distant regard. Although the *O*s were not satisfied with these difficult observations, the ratios show that the illusion occurred almost in its maximal amount.

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[5] Zoth, *op. cit.*, 363-366; Schur, *op. cit.*, 69.

TABLE II

APPARENT SIZE OF THE MIRRORED MOON AS A FUNCTION OF THE ELEVATION OF ITS IMAGE

d = distance of mirror from O in meters. α = angle of elevation of moon's image from horizontal in degrees. θ = visual angle in degrees subtended by comparison stimulus at 3.5 m., equated in size to moon's image. σ = standard deviation of the distribution of θ. Erect posture. Ratio = ratio of θ's. Every value of θ is an average of 5 observations. October 9, 1938.

		E.G.B.		A.H.H.		L.M.H.	
d	α	θ	σ	θ	σ	θ	σ
.61	0	3.1	.18	4.7	.26	6.4	.13
.53	90	1.7	.08	3.0	.37	4.7	0
		Ratio = 1.8		Ratio = 1.6		Ratio = 1.5	
5.6	0	4.5	.26	5.3	.18	7.3	.29
6.7	38	2.1	.16	2.3	.13	5.0	.33
		Ratio = 2.1		Ratio = 2.3		Ratio = 1.5	

The lower half of Table II gives the data when the mirror was somewhat more than 5 m. away from *O*. We did not find it practicable to raise the mirror 5 m. above the roof on which we were working, but we could clamp the mirror to a stool on top of a chair on top of a chimney, and thus we obtained a maximal elevation of 38°. These observations, with the supports of the mirror invisible in the dark, simulated closely the direct observation of the moon, even to exclamations of surprise called forth by the projection of the lunar image to the horizon. Actually the ratios tend to be slightly larger than those obtained with the real moon (Table I)—as perhaps they ought to be, since in these observations the *O* was instructed to elevate his eyes without raising his head.

We conclude, therefore, that this illusion can be studied independently of the natural elevation of the moon by the use of a front-surfaced, optically flat mirror, if the support is invisible and if the mirror is placed at a distance of more than 5 m. The moon's image should in this way be projected against the sky. If it is protected against a near visible object, the image is seen more readily at the distance of the object than in the sky through a hole, as it were, in the object.

CONCLUSION

The moon illusion is a function of the angle of regard as referred to the body as a frame of reference. When *O* shifts from an erect to a supine position, the illusion remains constant with respect to *O's* body and is reversed with respect to the earth. Thus the moon appears large with regard in the primary position, and the supine *O* sees the moon smaller at the horizon than in culmination.[6]

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[6] We are continuing the observations, controlling separately the movements of the eyes and head. We find that we can work with the sun by using a dense filter close to the eyes, much too close to be fixated. The filter excludes the sight of all other objects, and on a clear day the size illusion for the sun is striking.

A METHOD OF INDUCING FRUSTRATION IN HUMAN SUBJECTS AND ITS INFLUENCE UPON PALMAR SKIN RESISTANCE

By G. L. FREEMAN, Northwestern University

The growing interest in 'frustration' as an experimental problem has resulted in the development of various methods for inducing the condition. As mentioned in a recent symposium on the topic,[1] these range all the way from depriving some drive or inner 'craving' of its usual means of consumation to move acute interference with satisfaction by sudden thwarting of goal directed activity. Practically none of these methods have utilized human subjects, and the most typical one—the positive—negative conditioning of Pavlov,[2] has been limited exclusively to animal experimentation. This paper describes an adaptation of the Pavlovian methodology to human subjects and indicates that as the sensory discrimination becomes more difficult, breakdown of differentiated performance occurs and bodily reactivity rises.

METHOD AND PROCEDURE

In the Pavlovian experiment, a dog was conditioned to discriminate between different visual patterns; the presentation of a circle of light was always followed by food, while the presentation of an elipse of light was not. Thus the circle became a positive conditioned stimulus to salivation and the elipse became a negative conditioned stimulus, inhibiting salivation. As the elipse was made more and more nearly circular differentiation finally broke down, and, after a period of negative reaction, behavior passed over into continual excitement—even in connection with stimuli previously discriminated.

To adapt this general method of frustration to human subjects, the motivating condition has been desire to escape shock rather than desire for food. The subject is trained to make finger reactions with right or left hand, signaling whether the second of a pair of near-threshold visual stimuli is lighter or darker than the first. Failure to react within 10 sec. following presentation of the second stimulus is punished by shock and so are wrong responses. As with Pavlov's dog, the experimental series begins with stimulus-pairs widely separated in degree (of brightness) and continues to narrow down the separation until both members of the pair are alike. There are 30 stimulus-pairs in the series and these are presented by means of a newly developed visual thresholdmeter.[3] Since the frustration here developed is

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[1] S. Rosenzweig, The significance of frustration as a problem of research, *Char. & Person.*, 7, 1938, 126-128.

[2] I. P. Pavlov, *Lectures on Conditioned Reflexes*, 1928, 328.

[3] R. M. Simpson and G. L. Freeman, An instrument for determining visual thresholds (in press).

of a mild variety compared with that in the dog, the experiment is arranged to test for its effects in the following manner. As soon as *S* makes 3 consecutive failure-reactions, without announcement he is shifted back to the beginning of the discrimination series, and whenever three consecutive failures are made again the shift to easier problems is repeated.

RESULTS

Practically all of the *S*s (30 undergraduate men) who served in these experiments 'failed' before the end of the 30 trials, and as they were returned to the easy discriminations in the first part of the series, differentiation had become so disturbed that failure-reactions tended to appear earlier than on the first presentation. This trend toward disorientation as the problem becomes too difficult was borne out by work with several *S*s in an analagous problem requiring the discrimination of tachistoscopically exposed nonsense figures. In both situations, as shown by the detailed treatment of results, there was breakdown of specific differentiations, and a detailed analysis of results obtained with sensory discriminations shows the concurrent rise in general bodily excitement, indicated by decreased palmar skin resistance.

We present below, in Table I, the number of discrimination problems completed by every *S* before making 3 failure-reactions in each series, together with the average reaction-time and resistance changes of each.

It will be seen from the table that more problems are completed in the first series than in later ones: *i.e.* that *S* 'breaks' more quickly on easy discriminations following failure with difficult ones. This disorientation is accompanied by an increase in general bodily excitement (decreased skin resistance), especially significant because increased practice in the later series should favor decrease in excitement. While average reaction-time is not notably affected, there is some indication of delayed reactions, increased variability, and more erratic performance on later tests. Subjective reports confirm the effectiveness of forced reaction to nearly equivalent stimuli in developing 'frustration,' and results on 10 *S*s repeating the tests with higher order 'concept' discriminations substituting for sensory differentiation showed a similar trend.

Just as in other frustration studies, our results show individual differences in ability to withstand the experimental condition. As an expression of this ability we have taken the ratio between number of problems attempted before reaching the failure criteria in Series 1 and number attempted in Series 2. According to this measure, the person with the highest percentage retention of discriminatory ability, *i.e.* who attempted as many problems in Series 2 and in Series 1 before reaching the failure criteria, would be the most stable individual. The neurotic, unstable person on the other hand would be so disturbed by the more difficult problems near the end of Series 1, that even when put back upon easy problems (in Series 2) he would fail almost at once to react correctly.

TABLE I

NUMBER OF DISCRIMINATION-PROBLEMS COMPLETED BY EVERY *S* BEFORE FAILING, TOGETHER WITH THE AVERAGE REACTION-TIME (R-T) AND PALMAR SKIN RESISTANCE (PSR)

(Reaction-times in sec.; resistance in 1000 ohms)

	Series A			Series B			Series C		
S	Prob.	R-T	PSR	Prob.	R-T	PSR	Prob.	R-T	PSR
1	17	1.72	39	15	.97	38	17	.64	39
2	12	1.23	9	9	.57	8.	10	1.50	12
3	11	2.09	18	6	1.81	15	8	.75	14.5
4	17	1.77	—	5	1.17	—	11	1.12	—
5	9	1.78	39	15	1.59	36	19	1.98	33
6	6	.65	—	5	.44	—	6	.46	—
7	14	1.18	75	10	1.34	77	16	1.17	72
8	5	7.64	—	10	6.35	—	14	3.71	
9	16	2.68	73	11	1.16	73	13	2.05	73
10	20	1.77	49	9	1.29	45	10	1.61	44
11	20	1.08	34	19	1.44	31	10	1.47	25
12	5	2.55	17	7	1.79	17	2	2.07	17
13	20	1.38	15	6	1.42	15	10	1.61	15
14	21	2.52	120	6	2.82	120	9	2.22	113
15	9	4.30	55	6	5.33	55	9	2.13	55
16	5	1.60	—	10	2.45	—	9	2.81	—
17	10	1.50	76	14	4.10	80	9	2.91	74
18	19	1.30	20	11	.98	19	7	.76	20
19	9	4.04	19	10	3.54	17	7	3.18	17
20	17	2.63	43	10	3.63	38	7	3.38	30
21	15	2.00	88	6	1.40	85	6	1.20	85
22	12	1.86	66	6	1.23	62	6	1.25	61
23	20	.42	15	9	.24	11	6	.16	11
24	14	1.46	123	11	1.20	130	6	1.27	124
25	9	2.39	20	3	3.01	20	4	2.88	20
26	9	3.57	109	5	4.96	118	4	4.34	108
27	9	2.59	27	8	2.69	20	4	3.31	20
28	14	2.51	127	6	2.12	125	7	2.24	115
29	15	1.67	87	12	1.84	78	10	1.91	84
30	20	1.94	28	11	3.46	24	6	2.50	26
Av.	18.5	3.28	85	14.5	3.32	84	13.2	2.88	77

Significant correlations are found between the percentage of discriminatory retention and physiological indices of frustration. While the relation between disturbance in performance and mean resistance level is very slight, the palmar recovery quotient is very revealing.[4] This measurement is obtained by the percentage of return to the preëxperimental resistance level following conclusion of the discrimination trials. Previous work has shown that this measure correlates highly with such indices of neurotic behavior as inability to inhibit micturition, electric shock, and low ratings on neurotic inventories.[5]

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[4] G. L. Freeman, Toward a psychiatric plimsoll mark; physiological recovery quotients in experimentally induced frustration, *J. Psychol.*, 8, 1939, 247-252.

Since problems of increased difficulty were used to induce frustration and since there was an increase in palmar secretory activity with increased difficulty,[6] it might be argued that it is unnecessary to add frustration as a factor influencing greater bodily activity. This conclusion would be in agreement with the report of Davis,[7] who found that the level of muscular activity for failures on a given problem was not significantly higher than that for successes. Our results are similar to Davis' on success and failure; however, the way in which our problem was set up favored increased bodily activity on problems not intrinsically difficult if they follow difficult ones. While no sharp break in reactivity records which might be taken to indicate the occurrence of frustration was found, it is possible that the ratio of the level of reactivity to the level of difficulty might provide a criteria of frustration distinct from difficulty as such.

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[5] *Loc. cit.*

[6] W. Giese and G. L. Freeman, The relationship between task difficulty and palmar skin-resistance (in press).

[7] R. C. Davis, The relation of muscle action potentials to difficulty and frustration, *J. Exper. Psychol.*, 23, 1938, 141-158.

NOTES AND DISCUSSIONS

DUALISM IN PSYCHOLOGY

"What psychology strives to do, all that it could ever want to do, is to explain the self *in terms of* physiological conditions, among which those responsible for perception and memory would play a significant rôle."[1] With this statement in a recent book on the logic of modern psychology, few psychologists today would have any serious quarrel. Except that those who believe that the primary concern of the science is with such distinctively human activities as observation, memory, thought and voluntary action would make one important reservation. This is that these so-called higher activities of man (the traditional subject-matter of psychology) should first be thoroughly studied for themselves, introspectively and behaviorally, as an essential preliminary to any attempt to discover their organic conditions and causes. They would hold that the sound order of procedure is to begin with an examination of the products of these mental activities and (having increased our understanding of these), then to investigate the processes involved in their production, and, finally, to discover if possible their causes and conditions in the human organism. The methodological warrant for this procedure, and for regarding objects as products of psychological activity, has been clearly stated by Madison Bentley, who writes, in discussing the approach to psychology, "Where the physiological functions constantly refashion and reconstruct the living body, the psychological functions fashion and reconstruct knowledge, conduct, society, and the universal theatre of objects, events, desires and values."[2]

The justification, methodologically speaking, for following this order from final product back to original sources appears obvious; yet it is scarcely an exaggeration to say that just the opposite order has been followed by many investigators in psychology, who have assumed from the beginning what the organic conditions, the true causes, of human behavior must be. They may in fact be said to have begun with a preconception as to the nature of these physiological processes and how they operate, *i.e.* as chains or series of events in necessary (causal) sequence. Because it agreed with, and confirmed, such preconceptions, the reflex theory—which represents all neural activity as taking the form of straight-line, reflex-arc conduction—made a wide appeal and gained ready acceptance in psychological circles. Starting with such a conception of the physiological, and particularly the neural, conditions of human behavior, these psychologists give serious attention and detailed study only to those aspects of man's mental activity which can be correlated with these preconceived organic causes. To one committed to this mode of attack on the problems of psychology, it might well seem that "for scientific psychology, the descriptions of man's higher activities are relatively unimportant." Indeed, if we agree that explanation in psychology must be in terms of physiological processes, and if, in the second place, we admit that the line between the physiological and the physical" retains a verbal existence but in every other respect has vanished,"[3] it may be hard to see how such a conclusion can be avoided. The detailed and apparently distinctive characteristics of the higher mental activities are not only unimportant psychologically but irrelevant.

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[1] C. C. Pratt, *The Logic of Modern Psychology,* 1939, 39.

[2] M. Bentley, *The New Field of Psychology,* I, 1934, 17.

[3] Pratt, *op. cit.,* 29, 167.

The penalty for thus reversing the logical order of investigation would naturally be incompleteness and inadequacy of explanation. That this was the penalty actually paid seems to be the lesson of much of the progress made in psychological explanation during recent years. Explanation of perception was essayed in terms of sensory-motor integrations resultant upon conditioning, habit-formation, and the preparatory motor sets they induced under the influence of factors governing organic drives. The 'gestalt' psychologists, however, have shown the equal importance of a radically different group of factors; configurational tendencies, functional schemata, and socially standardized perceptual patterns—'frames of reference,' as they have been called. A plausible explanation of retention and recall was arrived at in terms of the associative linking of particular items experienced contiguously, dependent upon the establishment of brain-paths or connections which are susceptible of re-activation under appropriate stimulation; but the experimental studies of Bartlett and others have shown that remembering is not merely the reproduction or revival but rather the reconstruction of past experience, a reconstruction in which the organizational tendencies and the practical, emotional and esthetic interests of the individual play a large part. Likewise the attempt was made to explain learning in terms of frequency, recency, and intensity of serial responses, held to be factors in modifying synaptic connections, and opening new pathways of receptor-effector conduction; but in this case also further research has demonstrated the effect upon learning of influences of another kind, such as the organization of the perceptual field relative to the goal, the readiness of the organism to respond to cues leading goal-wards and to acquire a goal-directed orientation in its field of action.

Now the point to be noticed here is not that those explanations based upon a narrowly restricted view as to how the nervous system works were altogether wrong but that they were certainly incomplete. What the situation demanded was not that they be replaced by an entirely different explanation but that they should be supplemented and completed through recognition of the part played by a different and complementary type of factor. This point I wish to emphasize because it calls attention to a feature of mental activity whose significance is not, as I think, fully appreciated. The point is that when products of the 'higher' human activities are examined, they all disclose a duality of aspect if not of essential character.

The perceived object has certainly the character of present existence; it is that to which the organism adjusts itself here and now. Its qualities also signify the results of the percipient organism's past dealings with the object and the habits they have produced. These same characteristics also pre-figure the outcome of future behavior toward the object; in fact such control of future adjustment has been held to be their essential behavioral function. From this point of view, characteristics of the perceived object all relate themselves to the interaction between an individual organism and its environment in the past course of its adjustments. But equally and as well, the perceived object is a generic whole, exhibiting in its qualities the distinctive characteristics of a type or class. As such, it is not limited to this place and this time, but has its permanent position in the world-order and may be encountered again and again. If exception be taken to these terms as smacking of epistemology and not properly psychological at all, then it can as easily be said that from this second point of view the perceived object is a 'transposable gestalt,' a 'recurring constellation of qualities and relations.'

The products of memory relate themselves unmistakably to the life-history of the organism. The circumstances of time and place in which items are experienced or responded to, of spatial and temporal contiguity, undoubtedly establish bonds which partly determine the content of memory. The backward reference of memory is, as has been aptly said, the extension of the spatial and temporal co-efficients and ordering, already registered in perceiving. Without doubt the principle of association goes a long way to explain what we recall and when we recall it. But it is likewise true that the content of memory shows the effect of a process of reorganization in which items of past experience are sorted out, correlated and classified under heads supplied partly by individual interests, attitudes and life-goals, and partly by the relational patterns inherent in the experienced material itself. We know that in later life much of what has been experienced in early years is more frequently and fully recalled through its 'meaning,' *i.e.* through the relational construction which has assimilated it, then through those associative ties which link it with past time and place.

In the case of voluntary action, we deal with a sequence of events which begins within the organism and, in most obvious and familiar form, runs its course in a series of observably bodily movements. While some of the intra-organic events from which action starts (of course, there is no absolute beginning) remain obscure, some are quite well understood. But overt action itself, although it involves the complexities of skilled performance, nevertheless presents the aspect of straightforward causal determination resolvable into terms of stimulus-response connection. Yet here too, if we insist upon a just and adequate description of the activity in question, we must admit the presence of another aspect and of another type of relation. For in any course of goal-directed activity, each successive act is related functionally to the result toward which the whole series tends and which is required to round out and complete it. This functional relation is no artifact of impressionistic interpretation; it helps to determine the character of each such act as it is veridically experienced and observably exists—as the character of each succeeding note is determined by the melody in which it takes a part. It is more than a mere metaphor to speak of the course of voluntary action as a movement melody.

The products of thinking show the same dual nature. On the one hand, thinking is an 'internal' activity which may consist of inner speech, occurring as an event in the life-course of the organism. Thus viewed, its content is composed of 'symbolic' objects which follow the linear sequences predetermined by speech-habits conforming to the syntax of differentiated language. But, on the other hand, these symbolic objects—the words and sentences of consecutive discourse—are associated with relational complexes which exhibit the permanent structure and coherent organization of the real world. As thus regarded, the concepts and principles of thought summarize and express those modes of relationship which, at all times and for all individuals, weave the diverse and distinctive characteristics presented by existing objects into stable and objectively verifiable patterns.

A final illustration may be drawn from the vicissitudes undergone by 'the law of association' at the hands of psychologists. As every student of introductory psychology is taught, association by similarity is reducible to terms of contiguity by a well-known device of analysis and re-combination. But no sooner is the ghost of duality thus laid than it serenely rises again. For experiment proves, as psychologists all agree, that ideas or objects do not become associated merely through occurring or appearing together in the experience of an individual. To become associated, they must be actively observed together. As this fact has been stated in terms of the response theory of consciousness: "A brief explanation of the process of association would be that exploratory responses execute a pattern over the objects which are to figure in the association and this organic pattern is the total response to the situation."[4] Intention, if only the intention to see, the interest to observe and find out, introduces organization, patterning, into the behavior situation, and such organization is necessarily relative to a goal which stands in functional relation to other aims and interests in the life and experience of the individual.

The products of the higher human activities point, on the one hand, to the sensory-motor interaction between the individual organism and its external environment, determined on every occasion by processes of present stimulation and the continuing influence of residues of previous behavioral adjustments. But they connect, on the other hand, with the orderly structure of the objective world, and the possibilities of reorganization this structure holds by virtue of its dependable relation-patterns. These different and opposing characteristics apparent throughout the whole range of man's mental activities have furnished the most substantial support for the dualistic theory of the mind-body relation. In this theory mind has been identified primarily with the second-named set of characteristics; these are believed to reveal its essential nature. Mind is held to be, in and of itself, an organizing activity capable of reflecting, through its own characteristic modes of synthesis, the systematic inter-relation of objects and forces comprised in the existing world. But mind, while not itself in space, does nevertheless, according to the dualistic theory, inhabit and act upon a bodily organism which is in space and a part of the material world. Thus mind, though essentially a unity, self-determined and indivisible, is brought by the bodily organism it controls into indirect but effective relation to the realm of matter and physical causation. And the first-mentioned characteristics of mental products are simply, on the dualistic view, the results of this commerce between mind and the material realm through the agencies and instrumentalities of the living organism.

This dualistic conception of mind has been generally abandoned by psychologists for the quite sufficient reason that it gives us no help in explaining human activities to refer them to conscious mind in the dualistic sense, besides rendering incomprehensible the part played by these activities in organic behavior and their influence in the natural world. Certainly it does not help to solve the problem we have been considering of the indisputable duality of feature in perception, memory, thinking, etc., to find source for it in an ontological dualism between matter and mind. For the dualism in question remains or reappears as a matter of experience within the contents or field of 'conscious mind.' More than that, such attempted solution is positively misleading, since it encourages the dualistic psychologist to emphasize those features of human activity which are allied with the distinctive nature of mind as he understands it, and to neglect or minimize the spatial properties and external determination which certainly pertain to all or at least many mental activities.

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[4] H. S. Langfeld, A response interpretation of consciousness, *Psychol. Rev.*, 38, 1931, 104.

Suppose that we do not fall back on conscious mind dualistically conceived to explain the enduring order and stable cosmic structure that finds expression in the products of perceiving, remembering, thinking, and purposive action. We have then no alternative but to seek the conditions of this, as well as that other aspect of those mental products in question, which reflect the sensory-motor interaction of organism and environment, in the physiological activities of the organism. There would seem to be no insuperable difficulty about this. To be sure, no explanation in terms of organic activities could be found if neural activity is limited in manner of functioning to the traversing of straight-line pathways of receptor-effector discharge. But an enlarged understanding of the structure and working of the human nervous system has proved the older neurology to be one-sided if not untrue. The operation of dynamic patterns of brain-activity postulated by the gestalt psychologists has been confirmed by the experiments of Lashley and by recent investigation in neurology. In summarizing that view of the activity of neural systems and central levels of organization which he finds to be supported by evidence from all available sources, Papez says: "Straight-line or reflex-arc conduction is the essence of the chain-reflex theories. In the various levels of neural activity described here, conduction is not left out, nor are reflex organizations omitted or denied. What is added is a number of other levels of integration where, owing to the architecture and mass-action of specially organized nerve-tissue, a qualitatively different function is contributed to the total neural process. . . . And there seems to be little doubt that configurational patterns of all kinds emerge at the level of the cerebral cortex; and also that recurrent configurations of various kinds arise quite unlike the afferent patterns. Yet the afferent and configurational patterns are linked to each other and to the motor patterns in a labile way so that prediction in terms of stimulus and of resulting organic activity, physiological and psychological, cannot be made with certainty."[5] To this may be added statements from Judson Herrick: "The reality of reflexes cannot be challenged and their usefulness in physiology and in clinical diagnosis has not been impaired; but it is clear that simple reflexes are not the elementary units of behavior, and the actual conduct of animal and human bodies is not fabricated by monumentally piling up simple reflexes. . . . This internal rivalry or conflict of dynamic patterns is inherent in the vital process everywhere, but it comes to especially acute expression in the higher adjustors and integrators of the brain, and especially in the human cortex."[6]

On another score, however, difficulty is bound to be felt. It may be argued that we can understand how the content of perception, so far as it concerns the present object of sensory-motor adjustment, the content of memory so far as it deals with events linked in the individual's life-history, and of thought so far as it is comprised of symbolic objects of inner speech, can be correlated with physiological processes in the human organism. For, after all, both fields of activity, the mental and the physiological, belong to the same universe of discourse, the sequence of natural events, outside and inside the organism, within which relations of causal dependence exist and can be discovered. But it is altogether different with that other characteristic of perceptions and memories, of thought and voluntary action, that context of stable and orderly world-structure in which these activities place the succeeding objects of organic experience and response. How are we to conceive or to render intelligible the relation between such generalized, objective meaning and any physiological activities whatsoever, whether merely serial or elaborately patterned, which are themselves but events in the life-history of an organism? Many would agree that this objection is, from the standpoint of coherent scientific systematization, fatal, and that such a relation, if affirmed, would richly deserve that hard-worked epithet of opprobrium 'mystical.'

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[5] J. W. Papez, The brain considered as an organ; neural systems and central levels of organization, this JOURNAL, 49, 1937, 231.

[6] J. Herrick, Neural integration and neural disorder, in *Problems of Mental Disorder*, 1934, 198, 205.

It does not seem to me impossible to answer this objection or to conceive of the nature of such relation, although any answer we give must in the present state of our knowledge be largely speculative. But speculation in connection with a problem like this has its uses, if only to show that it is not impossible to render the relation in question rationally intelligible in consistency with the principles and conclusions of modern science. Assuming that the human organism is the highest product of organic and cosmic evolution, is it inconceivable that there should have emerged in the most advanced, highly organized and distinctively human nervous centers, energetic patterns which reproduce within the organism not merely sundry sequences of events in bio-physical nature (which prepare the way for adaptive response), but also the relational structure of the universe as an organized, encompassing whole? This supposition does not necessarily overstep the limits of 'mimetic duplication' or 'functional mimesis,' a principle which affirms "the kinship of the living organism and what is beyond the boundaries of the body." "If certain characteristics and properties of 'objects,'" says Bentley, "could not repeat themselves in energetic patterns of bodily function, it is difficult to see how perceiving could go on; go on, that is to say, without appeal to a magical eye, ear, or other 'organ and avenue of sense'."[7] The view suggested is also not incompatible with the "more or less plausible conjecture" of Raymond Dodge,[8] upon the mind-body relation, though he carries conjecture a little further than I am proposing to do. Dodge holds it to be the essential characteristic of consciousness that we never observe its phenomena except in organized form. He then inquires whether the kind of organization we observe in consciousness, which involves dynamic, cumulative integration and recapitulation, is to be found elsewhere in the universe. He suggests that the processes of cosmic and organic evolution furnish examples—such as that of the developing embryo. Thus he thinks that the embryo, as in the course of its growth and development it recapitulates the history of its race (thereby reflecting the dynamic whole of which it is a part) might be taken to represent a conscious moment in some more inclusive mind.

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[7] M. Bentley, The psychologist's use of neurology, this JOURNAL, 49, 1937, 247.

[8] Cf. two quotations from his *Conditions and Consequences of Human Variability*, 1931, 1-162, "Not only are there discoverable forms of organization within consciousness which characterize the systematic grouping of experience into units, temporal and spatial, practical and scientific, logical, ethical, and religious, etc., but we hold that consciousness is not essentially different from the observable phenomena of consciousness. It is conspicuous that we never catch conscious facts except in organized form" (p. 145); "Is there any evidence that elsewhere in the universe there are conditions for a similar form of integration of any factors whatsoever? Dynamic cumulative integration is a common phenomenon within the limits of our knowledge. Recapitulation is certainly rarer. It apparently recurs in the development of the embryo and possibly in some astronomical events. The hypothesis is not entirely fantastic that each embryo, as it grows and develops, recapitulating the history of its race, represents a conscious moment in some supra-individual mind: and that each developing nebula conditions an idea in some spirit of the universe" (p. 160).

So far my attempt has been to show that the duality of character exhibited by the products of the higher human activities is an empirically verified fact and not the result of confusing properly psychological with philosophical or even animistic methods of approach and interpretation. There is no idea of denying, however, that scientific thought is bound to regard any such duality as a perpetual challenge to its work of correlation and synthesis, or that it can ever cease from endeavor to surmount it. If we ask what it would mean to surmount such an opposition which presents itself as a fundamental, unassailable fact of our experience, the answer is, I presume, to discover some more comprehensive point of view from which the opposing features or factors show themselves to be functionally interdependent, to be complementary to one another in their respective contributions to some more comprehensive activity. The reason why such attempts have failed is, I believe, that they have been made from the standpoint of the individual human organism. Now so long as we confine ourselves to a view that is merely organismic, no progress is made in overcoming or surmounting the dualism which confronts us. But if we look at the problem from the standpoint of the intelligent social behavior of man, and of what we know of its achievements and the methods by which these have been accomplished, we may obtain at least a hint as to the solution.

The outstanding achievement of man's intelligence in the field of social behavior is the creation of an artificial environment, a cultural order which has the effect of stimulating coöperant activities in successive generations of group-members. This cultural order is the work of two inter-locking psychological processes, invention and communication, through whose joint operative effect the intellectual discoveries, the practical contrivances, and the artistic creations of innumerable individuals are contributed to a common social culture. Now invention and communication can together perform this central social function because of, and solely because of, that duality of feature which we have seen to be present in all psychological activity. Invention, whether in science, industry, government, or fine art, would be entirely beyond the power of the human individual if his organism were not responsive to the general properties and relations of the real world which make it comprehensible, to the causal uniformities and dynamic potencies which make its forces and materials available for human use, and to those harmonies of sense-imagery which give it aesthetic significance. Likewise and equally, inventions could never be communicated and converted into social possessions if every human individual were not in continuous interaction through sensory and motor response with the objects and events of his immediate environment, if in the everyday intercourse of life he were not continually responsive to the behavior of his fellows, to their speech and its meaning, to their actions and the methods and results of these actions, to their expressions of feeling and the perceptual patterns they set up. Thus we see that what looks like irreducible psychological duality when we confine our attention to the individual, turns out, when viewed from the standpoint of social behavior and evolution, to be a case of complete functional interdependence. In the social field these opposing aspects of man's psychological processes prove to be complementary and coöperative, and in their dynamic inter-relation to constitute the indispensable condition of all advance in civilization and the arts of social life.

The duality and interdependence of these two aspects of social activity and intercourse are exemplified in a striking way by the variety of social culture-products. For this reason they have a psychological importance that is not fully recognized. Each of these culture-products—a piece of writing, a mechanical appliance, a technical device, or an artistic creation—is a material object with its own history, an object which may be used by many people in endlessly differing circumstances, may be broken and repaired, worn out and thrown away. But each culture-product embodies for the intelligence which understands it and appreciates it an enduring pattern of properties and relations which remain the same for different individuals and succeeding generations of mankind because based on the orderly structure of the real world. This, the meaning of a culture-product persists, even if the incidence of wear and tear and the forces of decay and destruction bring it to ruin and require its replacement by copy or duplicate. The mechanical construction and economic and social uses of the automobile are suggested to the understanding mind as well by the battered remnant in the junk yard as by the brand-new machine in the dealer's show room. It is of course quite possible for culture-products to be made limited use of by individuals who have little or no understanding of the scientific facts and technological principles on which their efficiency and value depends. This is particularly true today of inventions which are being employed for convenience and enjoyment by vast numbers who are ignorant of the scientific discoveries and technological advances which have led to their production. While this limits the social value of the inventions in question and no doubt is, as present-day writers point out, a disquieting symptom in the social world-situation of today, it is in nowise inconsistent with the fact that these culture-products owe their controlling influence over the social history of man to the way in which they unite the opposing features which characterize all of man's psychological activities.

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THE 'BABOON BOY' OF SOUTH AFRICA

Although historically there has been much speculation regarding the personality characteristics of a human infant allowed to grow up in an "uncivilized" or animal environment, and although a number of allegedly "wild" foundlings have been reported in the popular press at various times, most of such accounts have been so exaggerated or distorted as to leave them of doubtful validity. There are on record, however, several authenticated cases of human children reared under extremely atypical stimulational circumstances. Itard's wild boy of Aveyron, found in a French forest in 1799, led a wild and animal-like existence.[1] The celebrated Kaspar Hauser, found at the gates of Nuremberg castle in 1828, had been forcefully imprisoned and reared in a dark and isolated cell.[2] The two more recently discovered "wolf children" of India, Amala and Kamala, were found in 1921 living in a cave with wolves.[3] Although these cases have been the only ones discussed at length in the psychological literature, Briffault mentions several similar cases, such as that of the "wild girl" of Chalons, whose details are more obscure.[4]

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[1] J. M. G. Itard, *The Wild Boy of Aveyron,* 1932, 1-104. Cf. also I. M. Mellen, this JOURNAL, 37, 1926, 309.

[2] A. F. Tredgold, *Mental Deficiency,* 1929, 290-292.

The present note reports another authenticated case—that of Lucas, a native South African boy who was nurtured by a group of baboons.[5] The data seem to indicate that the boy, aged 12 to 14 yr., was found around 1903, although reasonably convincing evidence regarding the case has just become available for the first time. It is interesting to note that this is the first authenticated case of a human child adopted and raised by infrahuman primates.

A United Press dispatch of June 19, 1939 reported an account given originally by Professor Raymond A. Dart, of the University of South Africa, regarding the "Tarzan-like existence" of a 12-yr.-old Negro boy kidnapped and reared by a tribe of South African baboons. Interested in the account, the writer communicated with Dr. Dart, Professor of Anatomy at the University of the Witwatersrand, Johannesburg, who kindly furnished the writer with a copy of all of the documents relating to the case.

Periodically, for more than a decade, the South African as well as European and American Presses have carried reports regarding this boy. One of the earliest of these accounts, sent from Johannesburg by The New York Times Company, appeared in *The New York Times* for Sunday, August 21, 1927. The story of the boy's capture and subsequent life is there related as follows.

> Two troopers of the old Cape Police were riding through one of the more remote and at that time wild parts of the South East Cape. Suddenly they came across a troop of baboons playing together in a clearing like so many children, and more out of spirit of downright cussedness than anything else, one of the troopers fired a revolver shot into the midst of the group.
>
> There was an instant exit on the part of the baboons, but the troopers noticed that one monkey trailed off behind his fellows and was soon out-distanced in the general scamper. At first the troopers thought this 'monkey' must have been wounded by the shot, and they galloped up to the laggard, only to find that instead of being a monkey he was a fully developed native boy. Developed, that is, with the exception that he had not got the gift of speech—at least as understood by human beings.
>
> He chattered like an ape when the troopers advanced to effect his capture and jumped about on all fours before he was overpowered.
>
> Having captured the boy, the policemen were puzzled as to the next step. Investigation through the kraals failed to discover his ancestry, and after having shown the native boy to practically all the natives in the neighborhood the troopers took him to a mental institution.
>
> He remained at the hospital for a year, and never at any time was he in the least vicious, although he was always very mischievous. It might be truly said that he was full of monkey tricks. But he could not talk and although he was offered the best fare of the hospital he preferred a tri-daily meal of raw corn and cactus. Even today he retains his old taste in food, and at one sitting he has been known to consume as many as eighty-nine prickly pears (cactus).

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[3] P. C. Squires, "Wolf children" of India, this JOURNAL, 38, 1927, 313-315; W. N. Kellogg, More about the "wolf children" of India, *ibid.*, 43, 1931, 508-509; A further note on the "wolf children" of India, *ibid.*, 46, 1934, 149-150.

[4] R. Briflault, *The Mothers*, 1, 1927, 23-30.

[5] The writer is indebted to Dr. Raymond A. Dart, Professor of Anatomy, University of the Witwatersrand, Johannesburg, South Africa, for making available to him a copy of the documents upon which this account is based, and for permission to quote them.

In spite of the fact that Lucas showed a strong desire to walk on all fours instead of relying on his legs alone, the hospital authorities came to the conclusion that he was not a fit subject for a mental institution, and that all he required was training. Mr. White heard of the boy through his brother, who was an official at the hospital, and offered to take him into his care.[6]

Lucas was covered with scars which looked as if they had been caused by bites, and some time after, when he had learned to talk, he told Mr. White that a particularly big scar on the top of his head was caused by the kick of an ostrich.

For a long time his monkey tricks caused Mr. White a great deal of bother, and he liked nothing better than to tease the cats kept on the farm, but right from the start he was civil and obedient to human beings and displayed such an affection for children that he proved a first-class nurse boy.

Nowadays he is without doubt the best servant in Mr. White's employ, despite the fact that he takes no account of time, or even of dawn or evening. He always has to be called to do a particular task, but once on the job his industry and reliability are remarkable, and he can outwork any native in the district. He is able to speak English fairly well, but understands none of the native languages, although his work brings him constantly into contact with natives.

Mr. Makepeace, of *The Star,* Johannesburg, reports that there are references to Lucas in their files as far back as 1927, the most complete accounts having appeared in *The Star* of October 4, 1930 ("A Native Romulus of Modern Times") and in *The Outspan* of September 4, 1931 ("The Truth About the Baboon Boy," by George H. Smith). In his communication to Dr. Dart, Mr. Makepeace states: "Our files show periodic references to appearances in various parts of the country of 'baboon boys,' but I am convinced that all of these are variations of the story of Lucas, or Luke, who might be termed the original 'baboon-boy' of the South African press."[7]

Mr. Makepeace reports the following account of the case, in a communication to Dr. Dart:

> The story told by Mr. George H. Smith, owner of the farm Thornhill, Shaw-Park, near the Kowie, on which Lucas lived for 28 years (up to 1931) is substantially the same as that told in the American Press, although it lacks some of the embellishments given to the story by the passage of time. In 1931 Lucas was about 40 years of age, and his master stated in his article that he was a good, hard worker, and a great help to him.[8] "He is very willing and remarkably intelligent, considering his human intelligence was never developed till he was about 12 years of age," says Mr. Smith, who goes on: "That he lived his early life in company with baboons is borne out, not only by certain facts contained in this story, but by my observations of him and that by others extending over the many years he has been on the farm. His profile, mannerisms, long arms, the abnormal development of the haunches, the constant jerking and nodding of the head, the scratching of parts of his body with the index finger, and his peculiar and frightened-looking grin, all tell their tale of his early association with baboons. . . . Facially the boy does not look like a baboon, for he was born perfectly natural. It is his habits which he acquired from the baboons that have made him different from other natives."

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[6] The "Mr. White" here mentioned is undoubtedly the "Mr. George H. Smith" referred to in the South African Police reports as well as in the communication to Dr. Dart from Mr. Makepeace, of the Johannesburg *Star,* which are presented later.

[7] Another alleged "baboon boy," whose appearance caused considerable stir in the South African press in 1937, was exhibited in Johannesburg until prevented, the boy having been found to be a case of neglected paralysis provoking the quadrupedal posture. This boy, named Ndola, had been found on a farm near Bedford, a curiously deformed creature tending cattle and goats. The boy was subsequently examined by medical officers, who were able to ascertain all the facts. This case is not, therefore, to be confused with the authenticated case of Lucas reported above.

[8] These statements place the approximate year of the boy's discovery around 1903.

Mr. Smith, on the point of Lucas's origin, quotes a 'prevalent' story, which he says he believes to be correct. This is to the effect that when Lucas was a young child he was put to sleep one day by his mother when she was hoeing mealies in some farmer's land. She returned to find him gone. It was generally believed at the time that the youngster had been stolen by baboons, in which the district abounded. The article goes on to relate the well-worn story of the finding of the child some years afterwards by two members of the Cape Mounted Police on patrol "somewhere in the Koonap district at a place called Driefontein." The lad, who behaved in the manner described in the newspaper cutting, was taken to the Mental Hospital at Grahamstown. He was kept in this institution for a month or so, and while here he exhibited mischievous traits. He was put in the private care of a Mr. Bruce, an attendant, but owing to illness in that family later the boy could not be looked after properly, and he roamed the streets. Mr. Robert Walters, the Head Attendant at the Hospital, suggested to Mr. Smith's father (then night attendant at the Hospital) that if the boy were put under a kind master, he would make a good farm hand. Mr. George Smith then agreed to take the boy on his farm, 36 miles away, and on arrival there he was given the name of Lucas.

Mr. Smith says that "it was months before I could do anything with him, he was so very mischievous and wild. The only way I could teach him was by thrashing him, especially as regards his dirty animal habits in and about the house. His habits were like a cat. He was not fond of water to wash himself. It was not a bit of use talking to him as he did not understand a human language; it was only when he got to understand a human language, a few words of English, and his memory and intelligence seemed to undergo a great change, that he recounted to me one day in later years how he, with the baboons, robbed the ostrich nests of eggs in his baboon days, and came by the scar on his head by the kick from a coolie ostrich. He knew no native language, and the few words he speaks are what he has picked up since being with me."

The article which appeared in *The Star* on October 4, 1930, is interesting because it tries to sift fact from legend. The writer, none other than the celebrated Ethelreda Lewis, of *Trader Horn* fame, states that she had first seen the 'baboon boy' in 1928. This present visit, on which the article was based, was made two years later. She says: "The facts about Luke are that he was adopted 22 yr. ago at about the age of 11 yr. by his present master; that he was taken by him from the Mental Asylum at Fort England, where, some time previously, he had been handed over by the police, who had found him."

The legends about Luke are that (1) he was taken to the asylum by the police, who, having noticed a human child among a troop of baboons, managed to trap and kill his monkey protectors, (2) that after this incident it was remembered in the district that, many years before, a native woman had lost her infant while weeding a large mealie field. Having left the child asleep at one end of the field, when she got back to the spot her child had disappeared, and nothing was heard of it again. The first part of the legend can only be proved by police records, which perhaps may some day be properly searched by those on the spot. As for the second part, many people discredit the idea of a baboon stealing a child. To me there seems nothing miraculous or improbable in it. Any nursing female monkey might find it convenient, or at least instinct, to steal a suckling animal so greatly resembling her own. . . . The part of the story that requires to be cleared up is the part played by the police. The article goes on to deal with the farm history of Lucas, the 'baboon boy' turned farm-hand.

Subsequent mentions of the 'baboon boy' in the South African Press repeat the information given in these earlier stories. As Mrs. Lewis said in 1930, the crux of the whole matter—what might be termed the missing link in this story—is the police record of the occurrence. So far as I am aware, no effort has been made to trace this original report of the finding of the boy with baboons, and similarly no effort was made, to my knowledge, to trace the troopers who found him.

Photographs of Lucas published in *The Star* and *The Outspan* show Lucas to be a normal looking native, standing erect, and helping his master with farm tasks.

In order to corroborate or test the validity of the story that Lucas was captured and taken to the Mental Hospital by police, Dr. Dart addressed communications to Dr. W. Russell, Commissioner for Mental Hygiene, Pretoria, and to the South African Police Headquarters at Pretoria. Inquiry at the Mental Hospital at Grahamstown indicated that since the boy was given the name of Lucas only after leaving the Mental Hospital, it is not known under what name he was admitted. Lieut. Col. O. J. T. Horak, Deputy Commissioner of the South African Police Commanding Cape Eastern Division, however, reports the following hearsay evidence regarding Lucas:

> No. 9563 (M) Constable W. J. Coetzer, a serving member of the Force stationed at Grahamstown, states that during 1921, whilst stationed at Springmount, Grahamstown Police District, he served under Ex-No. 1516 (M) Lance Sergeant Charles Holsen (ex-Cape Mounted Police who died at Grahamstown on 3, 9, 1924, whilst serving in the Force). That the said Lance Sergeant Holsen related to him how one day whilst on patrol with another member of the Force (name unknown but also said to have since died), they came across a troop of baboons which they chased and shot at with their revolvers. That one who was not as fast as the others was left behind and was caught by them, and found to be a native boy between 12 and 14 yr. of age.
>
> They took the child with them, and he was later admitted to the Mental Hospital, eventually being handed over to Mr. G. H. Smith, a farmer residing in the District.
>
> A statement has been taken from the Baboon Boy, copy attached, wherein he corroborates the version of his capture. This, however, cannot be relied on to any extent, as he is known to talk at random.
>
> I also attach copy of report dated 29, 4, 1939, by Constable Coetzer.
>
> There is no reason to believe that the story of finding of the 'Baboon Boy' by the Police is not authentic.

The complete statement made by Lucas is as follows:

> LUCAS Baboon Boy states, I am an employee of Mrs. G. Smith of farm Thorn Hill, Bathurst District. I can only recall a few incidents of my life amongst the baboons.
>
> My food consisted mainly of crickets, ostrich eggs, prickly pears, green mealies and wild honey. I was kicked on the head by an ostrich whilst raiding its nest and was often stung by bees whilst robbing their hives.
>
> I fell over a krantz whilst busy searching for food and broke my left leg.
>
> Whilst with the baboons I walked on all fours and slept in the bush entirely naked.
>
> I was hunting for food one day with my baboon companions when two policemen shot at us with revolvers. I tried very hard to escape but I was captured and carried away on a horse by one of the policemen. I cannot recall the place or district in which the police found me.
>
> LUCAS his X mark.
>
> Statement taken by me at Thorn Hill on the 8th day of May, 1939.
>
> (signed) G. G. WRIGHT No. 16448 (M) Cst.

The statement made by Constable W. J. Coetzer is as follows:

> During the year 1921 I was stationed at Springmount Police Station, No. 12 District Grahamstown, the Station Commander of Springmount station at that time was ex-Lance Sergeant C. Holsen (deceased). He was a member of the old C.M.P. Corps prior to the amalgamation with the South African Police. He informed me that he and another member, the latter I cannot quite remember but I am under the impression he said he was ex-Sergeant Venter also now deceased, were on patrol when they found a troop of baboons which they chased and shot at with their revolvers. The baboons fled and they noticed that one was remaining behind. They then went up to this one and on examining him they found that it was a native

boy about 12 to 14 years of age. This native boy followed the baboons on all fours. They then caught this native and found he was unable to speak any language but mimicked like a baboon. These members then took the native boy and he was eventually admitted to the Mental Hospital at Grahamstown. Later this native boy was handed to Mr. Smith who took him out to his farm where I saw this native boy myself. Mr. G. Smith of Shawpark told me that this was the native who the police found amongst the baboons.

The late Lance Sergeant Holsen told me he was stationed at Trappes Valley at the time they found this native boy.

The original reports, whenever available, have been reproduced in the present article in considerable detail, so that the reader might be more accurately informed regarding the data on which the case is based. Attention might be called to the following points which are of especial psychological interest in the accounts. The boy, when found, showed a rather atypical physical development, as evidenced by his "long arms" and the "abnormal development of the haunches." He "jumped about" and "showed a strong desire to walk on all fours." He "mimicked like a baboon" and exhibited other animal-like mannerisms, such as a "constant jerking and nodding of the head," "the scratching of parts of his body with the index finger," and "a peculiar and frightened-looking grin." He "was not fond of water to wash himself" and had to be thrashed repeatedly for "his dirty animal habits in and about the house." He "lacked speech," but "chattered like an ape." He "was always very mischievous" and "wild," and "full of monkey tricks." Although offered the best of fare, "he retained his old taste in food" and "preferred a tri-daily meal of raw corn and cactus," once consuming "as many as 89 prickly pears (cactus)." He took "no account of time, or even of dawn or evening." Although an excellent servant, he required constant direction, and "always had to be called to do a particular task."

The 'baboon boy' of South Africa thus represents an addition to the list of reasonably authenticated cases of human infants who have grown up under atypical stimulational circumstances, without access to human culture. As pointed out in an early paragraph, this appears to be the first case of a human child adopted and reared by infrahuman primates. It is also important to note that although with continued human contacts the boy retains traces of his infrahuman associations, his adaptation to human institutions appears to be markedly better than that of the previously reported wild foundlings, such as the Wild Boy of Aveyron and the 'wolf children' of India. He is said to be "a good, hard worker," "remarkably intelligent," and is even able to relate details of his past existence among the baboons.

The George Washington University JOHN P. FOLEY, JR.

Sigmund Freud: 1856–1939

Sigmund Freud, the creator and dictator of psychoanalysis, died in London on September 23, at the gray age of eighty-three years. He was born on May 6, 1856, in Freiberg, Moravia (once Czechoslovakia), the eldest child of Jewish parents, his father being a full generation older than his young and ever-idolizing mother. When he was four his family moved to Vienna, and here, in this cosmopolis of worldly pleasure-loving people, the boy grew up to be a quiet vigilant observer and a thinker schooled in rivalry, who, feeling himself an alien, adhered for all his days to an embattled cloistered form of life. From the start intelligent and probingly ambitious, he headed his class quite regularly; and in due course, being chiefly curious about human beings, chose medicine as his profession. At first he devoted most of his time to research in Brücke's physiological laboratory; but, deciding on neurology, he accepted, after graduation in 1881, the post of junior physician at the Vienna General Hospital. He also worked in Meynert's Institute, studying the histology of the medulla oblongata.

About this time Freud became acquainted with an able internist, Dr. Breuer, who told him about a case of hysteria that he had cured by encouraging the patient to recall, under hypnosis, forgotten memories of early traumata. This was the first time anything of the sort had been attempted, and Freud, seeing promise in it, decided to learn all he could about hypnosis. A travelling scholarship permitted him to spend a year (1885–86) in Charcot's clinic at the Salpêtrière in Paris, where he learned that hysteria could be produced and then relieved by hypnotic suggestion. Later (1889), during a short visit to Nancy, Liébault and Bernheim taught him that suggestion, without hypnosis, could do the same. Returning to Vienna, Freud married "the girl who waited for me in a far-off city longer than four years," and started practicing as a specialist in neurology. In collaboration with Dr. Breuer, he was soon engaged in testing the efficacy of what he had absorbed in France, including the later-published theories of Janet (whom Freud had not met in Paris). A preliminary report by the co-workers was followed in two years by *Studien über Hysterie* (1895). Up to this point Freud had merely helped in arriving at the best synthesis of the leading theories of neurosis, but from then on all his work was strikingly original.

In 1893, Freud chose an apartment with an office, in which he worked with indefatigable application for forty-five ascetic years. Here for ten to twelve hours a day he listened to what unhappy men and women had to tell him; here he spent his evenings in reading, writing and reflection; here in later years, not being a member of the Vienna faculty, he met his pupils. He had three sons and three daughters, the closest to him being Anna who eventually joined him in practice. From 1896, when his father died (the most important event in a man's life, he said), the biography of Sigmund Freud is the eventful history of his ideas, and the fervent fellowships and bitter quarrels they engendered. "It has always been necessary to my affective life," he confessed frankly, "that I should have both an intimate friend and a cordially detested enemy." Breaking with Breuer, Freud worked by himself for ten years, designing the great broad foundations of psychoanalysis: the procedure of free association, the theory of unconscious psychic processes and the necessity of interpreting dreams and fantasies to reveal them, the concepts of repression and infantile sexuality, and the theory that neurosis is the outcome of an unresolved conflict between the sex instinct and the moral prohibitions of society. His chart of the unconscious aroused the shame-faced embarrassment and hostility of the medical profession; and, as he expressed it, "an empty space soon formed itself about my person." But conviction was his ally; and he continued his researches, buoyed by the vision of a postponed victory, some day long after he was dead when his work would be discovered and men would start "moving to the measure of his thought." It was not so long, however, before his publications, *Die Traumdeutung* (1900), *Zur Psychopathologie des Alltagslebens* (1904), and *Drei Abhandlungen zur Sexualtheorie* (1905), attracted a small circle of enthusiastic followers—Ferenczi, Stekel, Adler, Rank, Sachs and others—and in Zurich, Bleuler and Jung became staunch advocates of his theories.

But Freud was still a name that commonly called forth reprobation when in 1909 Stanley Hall risked inviting him with Jung to deliver lectures at the twentieth anniversary of Clark University in Worcester. This, the first public recognition of his work, Freud said "encouraged my self-respect in every way. In Europe I felt as though I were despised; but over there I found myself received by the foremost men as an equal." William James, then sixty-seven, hearing both Freud and Titchener for the first time, summed up his impressions unofficially as follows: "I hope that he (Freud) and his disciples will push it (the dream theory) to its limits, as undoubtedly it covers some facts, and will add to our understanding of 'functional' psychology, which is the real psychology. The 'function' of Titchener's 'scientific' psychology (which, 'structurally' considered, is a pure will-of-the-wisp) is to keep the laboratory instruments going, and to provide platforms for certain professors. Apart from that it seems to me more unreal than any scholasticism." It was like James to want a theory pushed to its very limits; and Freud was not the man to disappoint him. Indeed, there seemed to be no limits to the Viennese's fertility of mind. Productiveness continued for thirty unabating years—until his collected works summed up to a dozen massive volumes.

Hardly had the International Psychoanalytic Association been founded in 1910 than it began to break up into factions; and within a few years the heretics, Adler, Jung, Rank, Stekel, unable to accept pansexualism as a doctrine, were pursuing independent paths. But many followers were left and a host of others gained who knew better than, or not enough, to oppose a man who had staked his life on an idea. "For psychoanalysis is my creation," Freud said, and "I feel myself justified in assuming that nobody knows better than I what psychoanalysis is." Freud wrote many short technical articles and these contain some of his best observations and ideas; but he also wrote at large, popular lectures and semi-philosophical essays, marked by a fine literary style, with vivid analogies clarifying his conceptions. And he indulged in many a scientific *jeu d'esprit,* in which he would start with disarming modesty from some specific fact, and with the help of principles he himself had formulated, advance from point to point, until he landed with an air of certainty on some sovereign conclusion. Some of these adventures of reflection were of dubious value (his *Leonardo* and his *Moses*), but the results of others were most suggestive, leading to confirmatory clinical observations. In *Totem und Tabu* (1913), he developed most fruitfully the concepts of ambivalence, animism and omnipotence of thought. Here he also presented a plausible, though now generally refuted, theory of the genesis of societal institutions. In *Massenpsychologie und Ichanalyse* (1922) he offered a theory of group action based on the power of the leader to attract the ego ideal projections of its members. This applied rather well to the state of affairs in the psychoanalytic society, as well as to the ideology of Fascism. In *Das Ich und das Es* (1923) the superego concept was established as a necessary component of his theory of personality. In later books, Freud weighed man and Western culture in the balance, and concluded from the nature of both that happiness in this world was impossible; and, as for the sustaining vision of another world, *this* could no longer be entertained by honest men. Such an all-inclusive sentiment is a symptom of temperament and personal experience; and the inference is justified that from the start Freud was over-determined by an hostility to culture and—differing from Romanticists—to instinct. "Our 'culture,' " he said to a reporter, "is only an imagination and a veneer. Our primitive natures, our anti-social, bestial emotions, are still just under the skin. During the war 'cultured' men grew wild-eyed and slashed each other to bits. When the government and the conventions which we admit as our masters gave the sign and said, 'There is an enemy,' we heaved a sigh of relief and cast overboard our fragile repressions in order to lie, to murder, rape and steal." Here, he forgot the thousands who went to war reluctantly, those whose "good" instincts were forced to yield to the governance of an "evil" superego.

In 1923, three years after his *Jenseits des Lustprinzips* in which he argued for his notion of the death instinct, the first signs of a cancer of the lower jaw were noted, but by successive operations the progress of the growth was retarded; and, though suffering much pain, he kept up the same steady pace of work in his sepulchral study, his immense collection of Egyptian figurines in serried ranks about him. He knew that he had conquered: his offspring were as the grasses of the earth, his books were scripture, he was visited by scores of idolatrous disciples. But yet happiness stood off from a mind so long bent down to sombre knowledge. There was no efficacy for *him* in his principles of therapy. When the Nazis captured Vienna in March, 1938, the soldiers entered his apartment, upturning his belongings while he, as unconcerned as Archimedes, sat at his desk, correcting his latest manuscript. His works were burned, his possessions seized, his printing-house destroyed. But by the resolute action of French and English friends, he and his family were permitted to leave Austria in June; and, ensconced in a comfortable residence in Hampstead, London, he found what he wanted most, "peace, study and work," until he died.

In the short history of abnormal psychology there is no one to compare to Freud. His contribution covers more than half the field; and today a therapist who attempts to get along without him is as helpless as a blind man fumbling here and there where others pick their way with some assurance. For it was Freud who distinguished the pathogenic forces in neurotic illness and showed how their signs could be discovered among involuntary processes: dreams, fantasies, free associations, absent-minded actions, slips of speech and writing, gestures and emotions, wit and laughter, unwarranted interpretations, behavior that is rationalized, accidents, creative art. Since inhibited tendencies are universal among men, the methods and theories Freud devised for the understanding of neurotics are just as useful, though not so indispensable, to the student of normal—especially supernormal—individuals. Thus, by revealing the vast realm that an over-rationalistic psychology had neglected, Freud pushed back the margins of obscurity. The enlargement of consciousness—*that* was Freud's best gift to man.

A synopsis of his work, as short as there is room for here, must confine itself to his main theories, now known the world over. Of these the most fundamental is his concept of unconscious psychic processes, taken in conjunction with his notion of the "id," its common drives, its complexes, its figurative emotive logic (similar to the mental processes of savages, children and psychotics), the displacements of its affects, and how it influences attention, perception, intellection, valuation and belief. Behaviorists may do without this concept, but not those who intend to understand subjective facts, who wish to interpret, control and get the creative most out of themselves and others. Freud's studies of the sex drive are subtle and important; and his constant emphasis upon this factor has proved of inestimable value. To put to shame the long conspiracy of silence was a Herculean achievement in behalf of truth. Above the sexually-pervaded "id," in the hierarchy of personality, Freud placed the partially-conscious "ego," which, according to his view defends itself against obnoxious impulses by certain mechanisms: repression, distortion, rationalization, projection, sublimation. These are pertinent to an understanding of forgetting, self-deception, hypocrisy, animism, delusion, imaginative creations. No less useful is Freud's concept of the "superego" as the internalization of parental mores and the knowledge that this integrate can work unconsciously, giving rise to moral conflicts that assume distorted forms in consciousness. This clarifies compulsions, religious rites and self-inflicted injuries (illness as atonement). Freud's ideas have proved an open sesame to child psychology: the determining significance of early traumata, the effects of various inescapable conditions (weaning), family relationships (oedipus complex), the fixation of cathexis (love or hate) upon certain objects, ambivalences and the introversion of instinct (narcism from sex, self-punishment from aggression.) Finally, of special interest to practitioners are Freud's studies of the transference situation which develops during the course of an analysis.

Freud's theories are most useful in interpreting highly personal relations and truly critical occurrences; and, as far as one can see, few of these are susceptible to experimental reproduction. It is scarcely possible to stage a masquerade of life, to manufacture twenty years of parenthood, to lure Eros to the laboratory. Freud's theories, consequently, must be tested in the field and in the clinic by imitating and varying his procedures. How many of his critics have done this? His followers, of course, have done it, and after thirty years are pretty well convinced that his hypotheses, though somewhat vague and slippery, are in the main correct. Such confirmations should not be underrated. History teaches us that 'pure' thinkers, removed from pragmatic operations, are capable of entertaining and logically defending the most unreal abstractions, but that men who are daily faced by actual emergencies are less apt to be indulgent to ideas that are not illustrated by events. The adulation that has been showered upon Freud may be partly apperceived as the respect and gratitude of practitioners who have been saved—pulled out of serious professional dilemmas—by his theories. This speaks well for their relative validity.

Freud's peculiar genius was shown in his ability to discriminate significance—he wasted no time on trivialities—and in his talent for perceiving a dynamic form amid a welter of apparently chaotic facts. That out of a hundred thousand hours of free association—a veritable Sargasso sea of psychic weeds—he saw and saved so much that was unobviously important, and made it so intelligible to others, is cause for wonderment. But his great strength in this department was offset by weakness in performing the other great office of a scientist: to furnish acceptable proofs of his conclusions. Freud was more interested in exploring new ground than in convincing others with accumulations of data, statistics and experiments; and, innocent of the dread of being wrong, he did not hesitate to publish the most far-fetched conceits of speculation. ("Now I conjecture that, in order to possess himself of fire, it was necessary for man to renounce the homosexually tinged desire to extinguish it with a stream of urine.") There would have been no fault to find if he and his followers had not insisted on the scientific character of their studies and the *truth* of their conclusions. Here they showed an ignorance of standards (a low intellectual superego). If Freud had been more of a scientist he would have pressed no claims to be one. Dogmatism is anti-scientific; and there are reasons to distrust a truth that forms a sect. Nevertheless, one can hardly suppose that the cause of science would have been served better if this great illuminator of obscure phenomena, who suggested enough problems for a century of workers, had confined himself to demonstrating scientifically one or two conceptions.

Surveying Freud's life as a single composition, one is impressed by the firm endurance of his dedication, the logical progressions—sometimes by detours—and the shrewd simplifications of his thought, the economy of effort, the forthright way he had of telling shocking facts, the lack of fanfare ("the Viennese will not forgive me, that I did them out of a spectacle"), his ceaseless interest in sex (clinging to the word as to a fetish), his evident intention to deflate the pride of consciousness. Sigmund Freud has made a difference and the future will take care of him. This can be said because for two decades or more his name has stood, like a monolith against the sky, to mark the boundary between two eras.

Harvard University HENRY A. MURRAY

Frank Angell: 1857–1939

Frank Angell, Professor of Psychology at Stanford University from 1892 to 1922, died at his home in Palo Alto, November 2, 1939, after a brief illness. He was 82 years old.

Angell was born July 8, 1857, at Scituate, Rhode Island, the son of Charles and Harriet (King) Angell. He was a nephew of President James B. Angell of Michigan, and a cousin of President James R. Angell of Yale. After attending private schools he entered the University of Vermont in 1874 and was graduated from that institution with the degree of B.S. in 1878, a year earlier than John Dewey. From 1880 to 1887 he was a teacher of physics in the high schools of Washington, D.C.

In 1887 Angell went to Germany, with the intention of studying physics at Würzburg, but a change of plans took him to Wundt's laboratory at Leipzig, where he received his Ph.D. in psychology in 1891. After one year as Assistant Professor of Psychology at Cornell, he was invited by President David Starr Jordan to become head of the newly established department of psychology at Stanford University. This position he held from 1892 until his retirement as Professor Emeritus in 1922. During that period he was away from the University only one year, 1915-16, which he spent in Belgium as special investigator for Mr. Hoover's Relief Commission. In recognition for this service he was awarded in 1919 the honor of Chevalier de l'Ordre de la Couronne, by the Belgian king. From 1895 to 1925 he was Associate Editor of THE AMERICAN JOURNAL OF PSYCHOLOGY, and from 1928 until his death was Associate Editor of the *Journal of General Psychology.*

Angell was profoundly influenced by his years at Leipzig and remained always one of the strongest admirers of Wundt. The Leipzig laboratory at that time was at its best with Külpe, Titchener, Angell, Kirschmann, and Meumann among its students. Angell's closest associations were with Külpe and Titchener. The following excerpt from a letter written long afterward by Titchener[1] will indicate the points of view which were then taking shape in the minds of these men.

> We were all naturally reacting at Leipzig against what I now should call 'biology'. More positively, we were trying to equate experimental psychology with physics, but in reaction against the deductive and constructional 'physiological psychology' of Wundt's book. . . . The addiction to physics was common to all of us, Angell included, and rather specially so, since he had been a teacher of physics. . . . The common tendency was certainly away from physiological psychology to an experimental psychology on the analogy of physics, and that tendency centered in Külpe, Angell, and myself.

Angell's friendship with Külpe was and remained particularly close. At various times in later years he tried to persuade Külpe to permit consideration of his name in connection with psychological positions in this country. His friendship with Titchener was destined to have a profound influence on American psychology, for it was at his suggestion that Titchener was called to Cornell at the time Angell resigned to go to Stanford. Angell's connection with THE AMERICAN JOURNAL OF PSYCHOLOGY coincided with Titchener's editorship; he joined the staff with Titchener and left with him. Later he went to the *Journal of General Psychology* because Titchener did and wanted him.

For Angell the science of psychology was coterminous with the experimental psychology of the laboratory. In that field he kept abreast of all important developments by reading practically everything pertinent that was published in English, German and French. He lived to appreciate some of the later extensions of psychology but he was ever highly critical of research that failed to utilize the most rigid controls. As a champion of introspection he was scornful of behaviorism and every variety of *angewandte Psychologie.*

Angell's own research was almost entirely in the field of psychophysics. It was of exemplary quality but did not bulk large. His greatest contribution was as teacher and inspirer of students. For thirty years his course in general psychology was one of the most popular and most valued in a university notable for the gifted teachers President Jordan had brought together. His lectures were carefully prepared, scholarly, and livened by a keen sense of humor. He was careful not to allow them to become stereotyped, and in order to avoid what he called the "canned" effect he refused to outline them in any kind of syllabus. The thousands of Stanford students who took this course with Angell look back upon the experience as one of the richest in their undergraduate careers.

For several years Angell taught all of the laboratory courses that were offered in the department, but after 1899, when Lillien J. Martin was added to the faculty, the beginning laboratory course was taught by her. Angell gave each year an advanced laboratory course and a seminar for senior and graduate students. Maud

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[1] To E. G. Boring, who has given me permission to quote.

A. Merrill has written the following account of the seminar as it was conducted during the last year before Angell's retirement.

> The seminar of 1921-22 was devoted to the experimental psychology of the thought processes. The problem of meaning goes back to the growth and development of the problem of perception; in the course of developing the problem we were concerned particularly with the investigations of Marbe, Ach, Watt, and other of the Würzburg group in Külpe's laboratory, the evaluation of the *Ausfrage Methode,* the evidence for a new imageless thought element, further developments in *Gestaltqualität,*—on down to the subvocal language habits of the behaviorists. The choice of this seminar topic was a particularly happy one for us because we were able through Dr. Angell to catch the spirit of the 'new psychology' as it must have come to life in the minds of the men who were his associates in the Germany of Wundt's day. We heard the thunders of Leipzig directed at Marbe's 'loose experimental methods', examined the protocols of the Würzburg observers for the evidence for *Bewusstseinslagen,* and learned the Wundtian respect for rigid experimental controls. Psychologists who had seemed to us hardly more than ghostly unrealities came to life; Külpe, for example, not alone as the faultless experimentalist, but as the courteous, somewhat shy and a bit old-fashioned gentleman who was Dr. Angell's closest friend. We came to know in Dr. Angell himself the high standards of critical judgment, the breadth of classical scholarship, the vigorous intellectual honesty of the experimentalist. One cannot recall those student days in Dr. Angell's laboratory without reviving again something of the zest that characterized the atmosphere he created.

W. T. Root gives the following account of Angell's methods of teaching in his advanced courses:

> Angell had little use for the lecture course except for beginners. In all my work in the Department, undergraduate and graduate, I took only one typical lecture course; that was his 'General Psychology.' Meanwhile I got my year of elementary laboratory, and after that Angell always suggested individual laboratory problems and seminars. He taught by the individualized, incidental method. He would wait several weeks until I had the technique and apparatus going smoothly and then would come in without comment and act as my reagent. He might keep this up for a month or two, introspecting, commenting, and suggesting readings, frequently in German. No teaching could have been more individualized and no professor could have given more generously of his time. Without using any of the pedagogical devices (papers, recitations, examinations) common even in graduate courses, he called forth sustained scholarly effort. He had the most enormous memory for specific citations I have ever encountered. I learned to go and ask his opinion on various disputed questions, first making sure that I had a fair grasp of what I was trying to talk about. For weeks thereafter he would hand me paged references with comment and the 'low-down' on the writer.

Franklin Fearing, who was Angell's laboratory assistant in 1917-18, writes as follows of his personal likes and dislikes.

> Angell's disapproval of applied psychology rested not so much on his doubts as to the validity of the applications as on his conviction that the goal and course of science should not be judged by its possible usefulness. His strong bias in favor of the empirical approach to problems was reflected in his interest in laboratory procedures, his impatience with uncontrolled introspection, and his dislike of philosophical speculation. Associated with his distaste for the technological aspects of psychology was his belief in the importance of classical training and his disapproval of college education that had a vocational slant.
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> For similar reasons he championed amateurism in athletics. He detested professionalism with its highly paid coaches, big stadia, and machine-like teams. From Angell's point of view, students who grubbed for A's, researchers who grubbed for

facts in order to write papers that would bring them better jobs, and coaches who turned out winning teams in order to fill the stadium, all came in the same category. So also he disliked Freudian theory and method, not only because of its slender empirical basis but also because he thought no gentleman would ever pry into the private affairs of another.

This enthusiasm of Angell's for amateur sports was deep and life-long. It was responsible for one of his most important services to Stanford, for, as chairman of the faculty committee on athletics, he was in effect director of university sports for more than twenty years and a sympathetic friend to generations of Stanford athletes. "His influence," said President Wilbur, "had more to do than that of any other man in the development of widespread wholesome sports among the colleges of the Pacific slope." Angell Field, Stanford's track and field stadium, was named in his honor.

A striking characteristic of Angell was his modest reticence in regard to himself. He liked to discuss psychological issues and the shaping of those issues by his early colleagues and others, but of his own part therein he rarely spoke. He was equally silent about the influences that shaped his early life and molded his later career. It is this reticence and this constant concern with the individual, in scholarship as in athletics, that mark the man—a gentleman and a scholar, if ever there was one, too assured of his own values to stoop to self-advertisement or mass production, and quite content, we need not doubt, that his chief memorial should rest in the minds of the many who felt his influence.

Stanford University LEWIS M. TERMAN

George Frederick Arps: 1874–1939

George Frederick Arps was one of the later Wundtians. He was born January 23, 1874, at Cary, Illinois. After a preliminary education in the public schools of his native State he went to the University of Illinois for two years. He then transferred to Stanford University where he obtained his A.B. degree in 1904. He pursued graduate study at Indiana University (M.A., 1905) and at the University of Berlin and finally went to the University of Leipzig to complete his formal study. There he received his doctorate with Wundt in 1908.

His experience with Wundt established permanently his field of scientific interest as far as psychology was concerned. He always identified himself with the experimental laboratory and the majority of his publications were of that type. In his later years when administrative duties absorbed increasing amounts of his time and interest he still had a hankering for the laboratory and was frequently observed in the laboratories of the Department of Psychology deriving vicarious satisfaction from discussing with his colleagues their experimental problems and their programs.

Dr. Arps came to Ohio State University in 1912 after teaching at Illinois and Indiana. At that time the Department of Psychology consisted of two men, a set of König tuning forks and a Hipp chronoscope. In the twenty-five years under his chairmanship the department grew to a personnel of over fifty, a payroll of $100,000, an equipment inventory of $64,000 and an output of over 100 Doctors of Philosophy. Much of this development was at a period when the importance of psychology was not widely recognized, when most University departments were small and when many of them were adjuncts of Departments of Philosophy. This administrative accomplishment may be attributed partially to Dr. Arps' constructive imagination, his facility in organizing materials for a 'case,' his everlasting persistence in a project in which he believed and his personality that carried conviction by its sincerity.

There was an interlude in his academic work during the war years. Leaving the department in charge of one of his colleagues he accepted a commission as Captain in the Sanitary Corps. He served at Camp Greenleaf, Camp Custer, and Camp Sherman. At the last of these he was Chief Psychological Examiner and Chief Morale Officer. Later at the U. S. General Hospital at Detroit he was Chief Educational Officer in charge of the rehabilitation of disabled soldiers, Chief Morale Officer, and Chairman of the Welfare Board and Board of Recommendations. He was discharged from active service in August 1919 with the rank of Major.

In 1920 he became Dean of the College of Education an office which he held concurrently with the chairmanship of the Department of Psychology. His contributions to the development of the College were equally notable. They included the gathering of a faculty of outstanding persons in all departments of the College. When the American Council of Education recommended a list of departments for graduate study, it included four at Ohio State and two of these were in the College of Education. Other of his significant accomplishments were the establishing of the Bureau of Educational Research, the Bureau of Adult Education, the Bureau of Special Education, and the University Schools. The last of these reflected his basic educational philosophy—a persistent and deep-seated concern for the welfare of the child.

In 1937 he laid aside these responsibilities to assume one which he felt to be still greater—the Deanship of the Graduate School. He was just in the process of making an extensive reorganization, seeking adequate budgetary support and causing the Graduate School to play a larger rôle in the life of the University. He was undertaking these duties with the same enthusiasm and energy at the time of his last illness. He died on September 16, 1939.

Some psychologists stand out in the perspective of history by virtue of their books, their theories, or their experimental results. While Dr. Arps' scholarly output in his early academic years was considerable, his greatest contribution from an overall point of view was providing facilities and support for others who were doing the active work. His greatest monuments are the Department of Psychology and the College of Education and the numerous students who have gone out from his Department to pursue psychological work in academic and applied fields of service.

Ohio State University HAROLD E. BURTT

Robert MacDougall: 1866–1939

Robert MacDougall, professor emeritus of descriptive psychology at New York University, died on November 1, 1939, at his home in Montclair, New Jersey. He was born in Dewittville, Quebec Province, Canada, on June 12, 1866. He was graduated from McGill University with the B.A. in 1890 and from Harvard University with the Ph.D. in 1895. Thereafter he worked at the University of Berlin and The Sorbonne. He held an instructorship in Western Reserve University in 1896-97 and an associate professorship in 1897-98. From 1898 to 1901 he was an instructor in Harvard University. Then he accepted a professorship in New York University, a position which he held until his retirement in 1932.

As a person, Robert MacDougall was kindly, humorous, thorough; as a teacher, he was scholarly and stimulating. His classes were relatively small but probably none of his graduate students left without a deep sense of having made a contact with a brilliant and cultured mind. As a psychologist, he was both theoretical and practical. His main interests were social psychology, psychological esthetics, number-habit, time-sense, speech, reading, spelling.

MacDougall's published output was comparatively small although he wrote on a variety of topics in the field. His notable volume, *The General Problem of Psychology (Conceptions)*, was published in 1922. Dealing with the general problems to which psychology like every science must give consideration, it gives a thoroughgoing analysis of methodological perspective of psychology. With great clearness the author points out the seriousness of misconceptions that arise when psychologists are not fully oriented with respect to the nature and purpose of the conceptions guiding their work. The volume is comprehensive and scholarly.

Thus, his contribution to psychology was primarily his logical delineations of a mentalistic psychology in the scheme of things, not only the relation of psychology to knowledge in general and to the arts of life, but the field, the limits, the methods, data, and specific problems of psychology. It was a discussion *about* psychology rather than a discussion *of* psychology.

New York University CHARLES E. SKINNER

BOOK REVIEWS

Edited by JOHN G. JENKINS, University of Maryland

Psychological Issues: Selected Papers. By ROBERT S. WOODWORTH. New York, Columbia University Press, 1939. Pp. x, 421.

Professor Woodworth's colleagues at Columbia have in this book printed a selection of his published papers in honor of his seventieth birthday. He himself was not consulted about the project in any stage of its progress, and the original articles have not in any way been altered. The result is a well edited, handsomely printed book consisting of twenty-five papers, a complete bibliography of his writings, and an index. There is also as a frontispiece a reproduction of the Blenner portrait.

The first paper is the autobiography written in 1932 for *A History of Psychology in Autobiography.* Thereafter the papers are classified under five headings: "Systematic Problems" (10 papers), "Abnormal Psychology" (4 papers), "Differential Psychology" (4 papers), "Motor Phenomena" (2 papers), and "Educational Psychology" (4 papers). The classification testifies to the wide range of the author's psychological interests, but the papers themselves will have to be read to appreciate his unique responses to the shifting psychological situations during the period of 41 years which the collection encompasses.

With the exception of an article on "Judgments of magnitude by comparison with a mental standard"—we would call them absolute judgments today—all of the papers of the first group deal with problems growing out of psychological systems that were in vogue at the time the papers were written. Five of these—"The cause of voluntary movement," "Imageless thought," "Non-sensory components of sense perception," "The consciousness of relation," and "A revision of imageless thought"—were all polemics against prevailing doctrines of sensation and image, particularly image. The first four were published during the years 1906-1908 inclusive, and the last was the author's presidential address in 1915. The remaining papers of this group are "Four varieties of behaviorism" (1924), "A justification of instinct" (1927), "Gestalt psychology and the concept of reaction stages" (1927), and "Situation-and-goal set" (1937). These four are still close to present day controversy. The others are historical and the graduate students of today will have to read the textbooks of the first decade of this century to discover why the author considered their topics as problems.

The first paper of the second group, a "Note on the rapidity of dreams" (1897) is Woodworth's first published article. It is still good reading and it is still useful for the student of dreams. The next article, "Psychology and experimental psychology" (1906) indicates a number of ways in which psychology might be helpful to psychiatry. Unfortunately psychiatry has given them little heed. The final article of the group is "Some criticisms of the Freudian psychology" (1917), a penetrating criticism that the psychoanalysts have ignored.

The Differential Psychology group opens with two articles which grew out of the author's experiments with primitive peoples at the St. Louis Exposition. The first of these is "Racial differences in mental traits" (1910, the vice-presidential address of Section H, read at the Boston meeting of the A.A.A.S. in 1909). Historically it is important not only because it is one of the first attempts in differential psychology to deal with the problem of the significance of the difference between two central tendencies, but also because despite certain French anthropologists its conclusions have not during these 30 years been undermined. In the second of the pair, "The puzzle of color vocabularies" (1910), the author argues successfully against the contention of several anthropologists that because a savage has no name for a color he cannot perceive the color. The third paper is "On factors contributing to a low scientific productivity in America" (1911). It belongs in this group only because it is granted that relative to the number of scientific investigators productivity is less among American than among European men of science. The last in this group is the paper on "Combining the results of several tests in statistical method" (1912). It is the only published paper of the author on statistical method although for two years of his life he had been a teacher of mathematics.

The two papers classified as Motor Phenomena hark back to the days when the author was an instructor and investigator in physiology. The one is "On the voluntary control of the force of movement" (1901), an experimental study that grew out of his dissertation which had been published two years earlier. The other is "Maximal contraction, 'staircase' contraction, refractory period, and compensatory pause of the heart" (1902). This is another experimental study begun in Cambridge in 1897 when he was a graduate student and completed in New York after he had received the doctorate in psychology. Both are models of scientific investigation and report.

The group called Educational Psychology commences with an investigation made in collaboration with Thorndike entitled "The influence on improvement in one mental function upon the efficiency of other functions" (1901). Historically it is the first extensive investigation of the transfer of training. It was originally published in three installments with three sub-titles: I. General statement; II. The estimation of magnitudes; and III. Functions involving attention, observation, and discrimination. The authors speak of these installments as articles. Consequently, a footnote to this effect, say on p. 368 of this book, would save the reader a little confusion. The remaining three articles in this group are "Present day methods of teaching philosophy" (1909), "Psychology in the college course" (1911), and "The teaching of psychology" (1920).

To properly evaluate these polemical, experimental, expositional, and popular articles would take a deal of research. There are innumerable hints, suggestions, and ideas that still await discussion and experiment. It is easy to see, however, that the author has from first to last been in the current of psychological development in the United States and he has himself contributed not a little to it. Indeed, it is remarkable how frequently the positive contributions of these papers have been incorporated in present day psychology.

In his polemical articles he has helped to clear the air of much fog and of some pedantic smoke-screens. They are marked by clear thinking, sincerity, kindliness, and a general persuasiveness that seems to derive from a simple appeal to fact and good sense rather than to formal logic. In the end his opponent is himself clearer if not convinced. His expository articles are always models of their kind, and his experimental studies leave one with a deep regret that there are not more of them.

It is almost impossible to escape the biographical aspect of the volume. It is as if the autobiography set the theme, and all that follows is its elaboration. Time and again the papers repeat, echo, amplify, illustrate what he has said of himself and of his work in the autobiography. If the papers are taken chronologically one can see how with his "situation-and-goal set" established almost from the beginning he has searched consistently for a point of view that would bear directly upon the problem of living and that would encompass what is being done in psychology.

The author may properly be proud of this volume; and his colleagues in psychology everywhere may join with his colleagues in Columbia in congratulating him upon reaching his 70th year of living, upon the distinction of his contributions to his chosen profession, and upon the promise which the contents of this book give for the future.

Cornell University H. P. Weld

The Place of Value in a World of Facts. By Wolfgang Köhler. New York, Liveright, 1938. Pp. ix, 418.

"The purpose of this book is philosophical." So also is its procedure. Phenomenological analysis is preferred to positivism; epistemological dualism is accepted on faith; isomorphism is the main thesis. The "world of facts" turns out to be a realm of hypotheses. Which hypotheses may be conceived to correspond to the experience of value ("requiredness")?—is the question. The answer, based on the assumption of isomorphism, is that values have as their correlates the macroscopic dynamics or "forces" of brain fields. But the way to this answer is discursive and has many detours.

There are ten chapters. In the first "The Case Against Science" is presented. In a long speech a hypothetical editor arraigns modern science for its negativism, its Nothing-But attitude, and its failure to provide insight into fundamental features of human destiny. The rest of the book may be conceived as an endeavor to escape from such a critique.

Chapter 2 touches lightly on some of the theories of value that have been proposed. Chapter 3 gives "An Analysis of Requiredness"—this being the general term substituted for "value." Particular attention is paid to "subjective" accounts of value, which are found to be, if not incorrect, at least incomplete. Phenomenologically "requiredness" is exhibited when vectors appear in a context, one part demanding or rejecting another. That one of the parts thus organized is often the phenomenal self is true. But items outside the self may be the source of vectors directed upon it, and, as in arithmetic, music and dress, the "acceptance" or "rejection" may involve items both of which are, in this not-self sense, "objective."

Requiredness is found in the world of phenomenal fact, regardless of human strivings and self interests. Tertiary qualities, like the primary and secondary, are right there where they are found, and phenomenologically, "facts" are at least not always just bare and neutral. Their bearings on one another, their demanding vectors, may be just as factual as the facts themselves. "We have thus given to values a logical place among facts."

But has value any extra-phenomenal locus? The following chapters consider this, which is of course the real question. The first 100 pages are wholly preliminary, for obviously if values were not phenomenological events there would be little excuse for a book about them. The resort to phenomenology is just a way of exhibiting the meaning of the concept "value" and of generalizing its application.

Chapter 4 is entitled "Beyond Phenomenology" and therein the author makes clear how strong is the demanding vector between his own logical self and the dogma of epistemological dualism. Support for the dualist dogma is sought by adducing experiences involving "transcendence," thus affording a "direct refutation" of phenomenalism. What the vector reveals as demanded is at least one good case in which "transcendence" is seen to occur and "while it lasts . . . implies transphenomenal existence." Trying to recall a forgotten name is made to bear the burden. The experience is analyzed to show how the lost (hence not phenomenal) name must *be* somewhere, for we feel now close, now far, etc., and it is clearly implied by and felt to fit into such details as are phenomenally given. "I do not hesitate to conclude," the author writes, "that 'transcendence' is a notion with a definite meaning, that the same is true of 'transphenomenal reality,' and that in principle we may ascribe existence to transphenomenal entities no less than to percepts and other phenomena."

Of course there is no accounting for all the things that people *may* do. Some readers may even feel that this illustration of what the word "transcendence" is to mean in this book is an adequate justification of epistemological dualism, overlooking the author's further surmise that the "reference" in imminent recall is actually not to a name but to a brain trace (which is not a name but only a neurological spook). Köhler, however, shares "the opinion of those who contend that all phenomena without any exception are the correlates of somatic processes in the nervous system" and brain traces are to be heavily leaned on in later chapters. The nervous system has for Köhler the same extra-natural status that it had for Titchener and is achieved by the same technique by which McDougall elaborated his "world of mental structure," although the latter outcome would be abhorrent to Köhler.

Even mediaeval demonology, it may be noted, observed a rigorous isomorphism; the destinies of men but reflected the conflicts of the spirits. The difference lies in the picture given of the possible nature of the transphenomenal. "Experimental facts," says the author, "of course are phenomenal facts." But an experiment does not occur in a flash; it covers a time span. So also does any act of recall. When one part of the experiment is immediate, is the remainder transphenomenal when its time span is lengthened? And if it does not, how does it exhibit any transcendence?

In such accounts Gestalt theory betrays its strangely asocial character. Phenomena, for it, are episodic individual events. Reality is, in these earlier sections of the book, an old-fashioned, nailed-up system. The social character of science and of "reality" are overlooked in favor of a Leibnizian pre-established harmony, which is chiefly structural and therefore mainly spatial (topological is the current argot). The author frankly admits the "it would probably betray exaggerated optimism if we should believe that after our argument in favor of transcendence the philosophical reader will feel entirely at ease"—but even a philosophical reader knows a good joke when he hears one!

There is a good deal of discussion in this chapter of the distinction between the body (a percept) and the organism (a transphenomenal entity) and a confusing attempt to show that the self is something of which the *I* is aware, along with other percepts. This is in the endeavor to save epistemological dualism from the barrage of the New Realism. The monetary identification of the self with the body serves a neat purpose but it fails to touch the epistemological problem.

The chapter ends with a neurological account of the nature of visual perception. Superficially it is a naïve account—images of objects are projected on the retina of the eye—the brain has two hemispheres, as everybody knows, etc. But actually this account is extremely esoteric. For it cannot be dealing with the eyes and brain we find in nature. Such organs are only "phenomenal," whenever we do find them, and as found they can only be parts of some "body percept," not parts of a superphenomenal, that is hypothetical entity, and who can say that "everybody knows" any particular thing about such fictions? The discussion of topological parallelism between visual perception and processes in the striate area never gets off of this epistemological level. It is appropriate therefore that Chapter 5 should wrestle with "The Nature of the Physical World" and its comparison with the phenomenal one.

It is now admitted that transcendence never reveals what is not already known. The physical world is a "construct" and its contents have always phenomenological models, although of course these latter may often be far to seek. However, the main point is that now "transphenomenal" objects become constructs, not a nailed-up system at all. Even the abstract quantitative aspects of science hark back to primitive phenomenal observations. For the reviewer the point is simply that, this being the case, objections may be forthcoming if the physical constructs should ever be put to any systematic use. Will not you, for example, find only such isomorphism as you have already allowed to creep in? If so, this will be nothing to exult over, and a book about it would be sheer tautology.

Considerable space is devoted to this project of showing how the most abstract physical terms or symbols must have a phenomenological footing, and for the first time it is incidentally intimated that it is the relative "invariance" of perceptual situations that dictates to physics its field of work. This point, if fully appreciated, might have led to a realization of the social character of "the real;" and it might have led away from epistemological dualism into a workable conception of the psychophysical continuum with its freedom from epistemological riddles.

The main point for the author, however, is that perceptual and physical systems have a common macroscopic structure—that is, are isomorphic. Obviously, of course, since as we have seen the latter are only revised reports, abstracts and summaries of the former from which many of the qualitative, subjective, *i.e.* variable items, have been deleted. But this the author would not admit. Actually, however, such isomorphism would appear to be so obvious that no startling use could conceivably be made of it, except perhaps in practical engineering. But engineers are naïve realists, not epistemological dualists.

Reverting to the main point, this is for the author that however different in other respects "thing percepts" and "physical objects" may be and often are, there is much macroscopic structural resemblance between them. Science, on the other hand, tends, especially recently, to ignore the molar aspects of the physical world, in favor of some form of atomism. The author emphasizes the need for holding fast to the macroscopic features as well, and he gives examples of the importance of context and field as genuine levels of organization. The chapter closes with critical consideration of the "principle of indeterminacy." The causal concept remains a valid notion and has special application to macroscopic objects. These having been demonstrated to be genuine entities, the way is clear for the next chapter "On Isomorphism."

First comes again the quaint distinction between the body and the organism—"they are two different entities." The former is a mere thing-precept, the latter a transphenomenal object. The author laments that we cannot "look upon the organism with quiet impartiality. The real trouble is that the organism he is going to talk about cannot be "looked upon;" it is a pure hypothesis or postulate (not to say "fiction") and whatever the author may say about it was not learned by looking. As a child the reviewer once hid a stolen object and then pretended to find it by accident, but even an unsophisticated parent sees through such a ruse. Yet that is the way isomorphism is presented to us. We observe phenomena; we invent a correlate in the beyond, patterned after them; then we pretend surprise that the two have a structural resemblance!

Isomorphism between phenomenal facts and cortical events is to be found, it is here alleged, on the plane of macroscopic brain dynamics. As is the Gestalt custom, hydraulic and electrical examples are used to establish a model for neurological fictions. It is recurrently lamented that modern physics pays slight tribute to macroscopic states and forces, so that the concept of "macroscopic dynamic contexts" is applicable only with difficulty and uncertainty to the organism. On the other hand, of course, this situation facilitates excursions into neural mythology!

The author, reluctant to admit that "cortical processes consist of independent events in individual cells" elaborates several examples of "psychophysical correlates" from a "macroscopic point of view." The "philosophical reader" previously complained of, will no doubt be ill at ease to learn that "points of view" are still tolerated in the alleged science of neurology. Hypothetical cortical field processes involving gradients of electrostatic potential in the "regions" of the striate area are described as possible correlates of visual patterns. Perceptual field and cortical partner have common structural features. Vision and its cortical correlate *are* to this extent isomorphic and the cortical macroscopic organization "mediates between the physical and the perceptual structure."

Although detailed knowledge of the state of individual neurones might reveal little, "the description of these processes in macroscopic terms would be a correct report, not only of the cortical situation, but also of some essential traits of the visual field"—but only, we should add, because the correspondence was originally smuggled in there when the hypotheses were made. Even the "outsideness" of things and the body in perception has its counterpart in the beyond—the "outsideness" of the respective cortical processes. Of course the "outsideness" has to be "functional," not merely "geometrical." With admirable restraint the author refuses to speculate as to what the cortical correlates of sensory quality, for example, may be.

Isomorphism is of course admitted to be only a "postulate," which "becomes a theory by virtue not of one hypothesis, but of a whole set of definite assumptions." In the words of a familiar household verse we might paraphrase this, "Oh, what a tangled web we weave, when first we practice," but the title of the book refers to a "World of Facts"!

Gradients in brain fields are suggested as the correlates of perceived relations. Again the technique is that of analogy, with a demonological flavor. Furthermore, gradients appear to have a geometrical as well as a functional implication. The citation of Jacobs' "experimental test" of the gradient hypothesis will disturb even the unphilosophical reader, for even if the results may be generalized they involve the simple fallacy of the affirmed consequent and give just no support to any hypothesis.

In Chapter 7 we return to "Memory and Transcendence" again, with defense of the brain trace hypothesis and alleged "tests" of the same. Mnemic processes in great variety, it is insisted, imply that we must assume "some entity which survives during the interval" as a representative or preservation of past events. The author refuses to "burden the system of my thought with the tremendous hypothesis of a third realm of entities, different from nature, yet not phenomenal." And the "remnants of past experience do not . . . really belong to the phenomenal world." Since "worlds should not be multiplied beyond strict necessity" the author proposes to "construct" the remnants of past experience as physical entities; namely, brain traces.

But why not apply the law of parsimony to remnants as well as to worlds? Since such remnants never phenomenally appear, why invent partners for them in the beyond? This is done because in mnemic processes there is often a long timelapse between cause and effect. Is there not always a time lapse between antecedent and consequent? And how long must this be to make necessary the postulation of remnants? The chasm is narrow indeed between a brain trace demonology and that of Freud or that of McDougall. The author defends his position by showing that in physics hypotheses have done useful work. Some of them have. The point is not that psychologists should not be permitted hypotheses—they should and are—, but they need not flaunt them prematurely nor elaborate them in a treatise purporting to deal with "the world of facts." Of course in a sense postulates are facts; they are, however, not transphenomenal and when nobody is making them they do not *exist*. Calling a critic "anachronistic" is as irrelevant to this situation as waving a flag would be.

The reader must consult the chapter for the exposition of details of the trace hypothesis and for the account of "tests" of it. All of these "tests," again, commit the elementary fallacy of trying to affirm a consequent without having eliminated alternative antecedents. The upshot is that the concept of traces is said to "refer to" purely physical entities and contexts; herein vectors originate so that specific "demands" and "requiredness" may issue. These "physical entities may fit into given phenomenal contexts because they have the properties which these contexts require for their completion."

Chapter 8, on "Organic Fitness," is a running commentary on a variety of biological concepts, such as selection, mechanism, regulation, equilibrium, vitalism, homeostasis, degrees of freedom, the "law of dynamic direction" of which a great deal is made, and the second law of thermodynamics. Organic events do not appear to exhibit "requiredness;" but "a general trend of nature is sometimes found to yield the same results as might be expected if the events in question were actually happening in order to fulfill a demand." Since this is a perfectly good "affirmed consequent" one is at a loss to understand why the author concludes that "we do not discover requiredness as such among the data of science."

"Facts and Forces" are treated in Chapter 9. The observations of "transcending requiredness" are recapitulated. Earlier parts of a context, now gone and no longer phenomenal, exercise demands by way of continuation, completion, or closure. How can the past thus operate when no longer phenomenal? Phenomenology cannot tell us, but for biological theory "the entity beyond can only be the trace of those processes which accompanied the earlier parts of the context when they were experienced." That is to say "a demand may issue from a trace, emerge into the phenomenal field and there decide what is wrong and what is right with reference to that trace." Imminent recall, again recapitulated, leads to a similar conviction: "an influence extends from a given phenomenal context to the trace in question and accepts it as right."

Tentative description of the characteristics of these requiredness situations shows their striking resemblance to the characteristics of situations involving "forces." The features are such as context, dependence, transcendence, demand. Now forces, in concrete physical situations, are "vectors which actually accelerate or retard displacements." The concepts of force and of requiredness must be brought together. They "occupy structurally identical positions in their respective contexts and the dynamic characteristics are similar." "We find that dynamic maintenance within a balanced state is the homologue of acceptance within a phenomenal context," etc.

Endeavors are then made to portray this parallelism concretely, between phenomenal requiredness and its cortical correlates (as constructed, of course). Pressures and resistances of electric forces in the brain field are sketched, corresponding to "good" and "bad" curves as seen, and again we are launched into the wholly mythical realm of neural electrostatics and gradients. "Requiredness has a neural correlate and that correlate is the tension which the neural substratum of the situation contains." Value and neural forces exhibit isomorphism. Not only in visual perception but elsewhere "forces play the same rôle within the macroscopic cortical process as that which various forms of experienced demands assume in the corresponding perceptual situations."

Values are brain forces, or at least they have a similar topology. The reviewer submits that it is just as instructive to say "Values are demons," or "Values are body fluids," if only you start with the right postulates and analogies. But the author is more at home in neural dynamics than the reader is likely to be. He is even so familiar with the factual nature of neural events that he can say confidently, "There are no inert velocities in cortical processes."

The final chapter is a series of discursive and informal remarks on "Man and Nature," and consists of a variety of after-thoughts and not too closely connected comments. The main outcome we have indicated in the brief survey we have given of an absorbing inquiry, an ingenious set of hypotheses, supported by scholarly insight, and a serious effort to show the critical straw editor who ambled through the introductory chapter that science may be able to make some progress in mapping out the innermost nature of human values.

The philosophical reader should not be too disappointed to see the search for value in a world of facts dwindle into a guess about the possible neural correlates of a "feeling of ought." The most valuable thing about the book is not its final guess on this point but its persistent and informed propaganda for a view-point—the Gestalt outlook, with its emphasis of macroscopic organization and the dynamics of contexts.

And what a pity that this book fell, for review, into the hands of a confirmed positivist, who is strongly allergic to imaginative neurology, dualism, analogy, and the fallacy of the affirmed consequent!

Barnard College, Columbia University H. L. HOLLINGWORTH

Theories of Sensation. By A. F. RAWDON-SMITH. Cambridge, England, University Press, 1938. Pp. xiii, 137.

This book does not discuss theories of sensation. In fact, the text contains no theoretical account of sensation at all. It presents many psychophysical facts, and most of them are well chosen. All of them are accurately and carefully described. As to the hows and whys of sensation, that is to say, as to theories of sensation, the reader will be impressed by their absence. Actually the author assumes two principle objectives: (1) to describe briefly how he thinks the eyes and ears work; and (2) to tell why he holds these views in preference to others. The first objective he achieves, but not the second. The book neglects completely to tell us the reason why the views are held.

The text is divided into two general sections: Vision and Audition. Each section considers histological, physiological, and psychological data. The histological facts (the eye and the ear) are sketchy and elementary. The physiological facts (of the eye, the ear, and their associated nerve trunks) are selected but representative. The psychological facts, except for visual acuity, are those usually encountered in the psychophysical studies of sensory quality and intensity.

The treatment of both Vision and Audition by a competent investigator offers unusual opportunities, but the author does not avail himself of them. He draws one or two comparisons between the processes of the two senses, but they are of little general importance (cf., for example, the parallel drawn between the muscles of accommodation and the tympanic muscles, p. 74). Thus, one of the most disappointing aspects of the book lies in its failure to indicate the important similarities and differences in the data and in the interpretation of the data secured in these two departments of sense. Even biologists, to whom the book is addressed, would have been interested in knowing about the similarities between the threshold functions for visibility and for audibility, that the principle of reciprocity with respect to time and intensity applies to hearng as well as to seeng, that functions comparable to visual dark-adaptation curves exist also for hearing, that hue (like pitch) varies as a function of intensity when frequency is held constant, and that the fundamental reason for the origin and nature of the prevailing theories of sense-organ activities in vision and audition lies, not in anatomical structure or physiological function, but in the need for an explanation of the facts of sensory experience.

The text treats the two senses differently. The first section places its greatest emphasis upon the facts of visual sensation and then deals with the implications of these facts for the physiology of the eye. That procedure is, of course, the fashion of the day. In the second section (Audition), the procedure is reversed. There, the text stresses the facts of the physiology of the ear, and then deals with the implications of these facts for auditory sensation. This difference in emphasis could have been explicated as a difference between visual and auditory theory, but the author misses this chance.

This asymmetry in treatment also contributes to the unevenness which characterizes the exposition of the monograph. Moreover, it obscures interesting points of fact, especially as regards the basic reasons for the theories concerned with the mechanism of wave-length analysis in the eye and of frequency analysis in the ear. The psychophysical facts of color discrimination and color mixture are shown clearly to constitute the *raison d'être* for the tri-receptor theory of wave-length analysis in the eye. What the author does not make clear is that an analogous reason was responsible for the resonance theory of frequency analysis in the ear. He gives the erroneous impression that Helmholtz and the others intuited the idea of frequency analysis in spite of their meager knowledge about the mechanics of the inner ear. A simple statement of Ohm's law (for all its deficiencies) would have set all this right.

Now let us turn to particulars.

The four chapters on Vision include an elementary account of the human eye, a careful summary of the results usually associated with the belief that scotopic and photopic vision are respectively mediated by rods and cones, a discussion of intensity discrimination and visual acuity, and an accurate presentation of the facts of color mixture. With the exception of Chapter 3, these chapters are valuable for the capable descriptions of the material presented.

Chapter 3 (intensity discrimination and visual acuity) is the weakest in the book. In the first paragraph the author presents the reader with a factually inaccurate and a logically untenable account of results for intensity discrimination. He uses Weber's function as a basis for his discussion, and makes two assumptions concerning this function: (1) that there is one and only one 'true' form of the Weber function; and (2) that he knows the 'true' form. Neither assumption is correct. Ever since Weber's original study, investigators have sought to discover the truth concerning the form of this function as determined under varied and varying experimental conditions. The net result of all the work done in the field of vision up to the present time is a convincing demonstration that the Weber function, $\Delta I = f(I)$, changes its form in an orderly manner as particular experimental conditions are systematically altered. At best, the author's argument is circular, for he assumes the constancy of $\Delta I_l/I$ at high intensities, and then dismisses as inadequate all experimental results which do not confirm his assumption.

The same type of adverse criticism is applicable to the discussion of visual acuity as a function of illumination. The author's treatment of neurophysiological results, on the other hand, is excellent, for he rejects the notion of mere neural 'correlation' and attempts to indicate some of the implications which follow when a visual process is considered as a constellation of causal events. Visual theory (like auditory theory) stands sorely in need of just this kind of theorizing.

The three chapters on Audition contain a summary description of the working parts of the ear, the arguments for and against the resonance theory of frequency analysis in the ear, an objective account of the cochlear response, and a discussion of sensory intensity. These three chapters are the best of the seven in the book.

The author is frankly skeptical of models, of the hair-cell theory of the cochlear response, and of the 'evidence' secured for the place theory by way of results by lesions produced directly and through long exposure to intense tones of low frequency. He discards the models of Ewald and of Wilkinson. It was Ewald's theory (1899), nonetheless, which predicted the changes in pitch which accompany changes in intensity, and which stimulated Zurmühl (1930) to make his important study of those changes. Although the question of the cochlear response is still regarded as unanswered, the author, fairly and convincingly, presents the case for the non-resonant mechanism. He summarizes the evidence for the volley idea of Wever and Bray clearly.

The work of Knudsen and Riesz forms the basis for the discussion of the Weber fraction, and the text emphasizes the fact that ΔI is a function of time as well as intensity. The relation between ΔI and time is not one of inverse proportionality, however, and the form of the Weber function in audition (as in vision) can be shown to change as conditions other than intensity are systematically varied.

In this book, the author has selected and condensed a large number of histological, physiological, and psychological facts. The selections are eclectic; the descriptions, terse and effective. In telling how he thinks the eyes and ears work, however, he has unwittingly subscribed to the view—a current view—that events in the sense-organ can be accurately assayed from a study of psychophysical results. Much of the 'adverse' criticism in the present review is directed toward making this fact explicit.

Harvard University ALFRED H. HOLWAY

The Child and His Family. By CHARLOTTE BÜHLER. New York, Harper & Bros., 1939. Pp. viii, 187.

The Child and His Family represents the first major attempt to study the structure of the normal child's family in a way that allows quantification of significant qualitative material. Although the chief stress is laid on the methodology, the content is equally important. The method consists of observational records of life situations regularly occurring in the family. A trained observer visited the same family for several weeks or months, at different times of the day, and participated as a member of the family in whatever activities were engaged in. Immediately after every visit verbatim recordings of verbal and non-verbal events were made. The range of agreement between observations of different persons extends from 66 to 83%.

The chief contribution of this study lies in the system of classifying the observational data. The dangers of quantification of such material arise from the character of the categories employed. To the extent that the units of behavior are on a reflexive, or logical rather than psychological level, their value is proportionately decreased. Dr. Bühler has not allowed her desire for reliability to blind her to the important aspects of the problem. Too often the observational studies in the United States have defeated their purposes by sacrificing meaning for reliability.

Parent-child contacts of 17 families were observed, six of these being studies over several months. The nature of these contacts were analyzed in terms of the means by which they were established (*e.g.* greetings and affectionate approaches; non-verbal activities as bodily contact, manipulating materials; and conversations), the *situations* in which parents approached their children (*e.g.* social intercourse, play, and biological situations), the *intended purposes* of these contacts, and the *direction* (*i.e.* positive, negative, zero) of the reactions of the children to the situation. The average number of each type of contact was obtained for the 17 families, and utilized later as a comparative standard for the six families studied intensively. Employing as far as possible the same categories, child-initiated contacts with parents were analyzed in conjunction with parent-initiated contacts. In similar objective fashion an analysis of the relationship between six pairs of siblings was made, four of these pairs being members of the six families mentioned above.

It is a commentary of the essential correctness of the schema of classification, that differences between family structures stand out clearly in those terms. Some of the categories, however, appear of doubtful value. Particularly striking in this respect is the classification of any internal response as "zero," in apposition to "positive" and "negative." Embarrassment is thus regarded as a zero response. In addition, it is difficult to conceive the value of breaking down the means of establishing contacts into such minute classifications as "Conversations; Statements; Stories; Guesses; Reflections; Objective means; Questions; Plans; Directions; Showing; Explaining; Informing; Correcting." This is illustrative of one of the very few digressions into distinctions of a non-functional nature.

With specific regard to the quantification of the data, it may be noted that the number of the cases are few, and the averages obtained probably somewhat spurious since the children scatter fairly widely in age. Tables are generously provided—but without measures of reliability. At one point there is made an awkward separation between the approaches and reactions of parents from those of their children. The comparisons one would like to make of these is unnecessarily hampered.

Dr. Bühler and her associates claim little for *The Child and His Family* other than a presentation of the technique of analysis they have developed. The nature of the content is a significant commentary on the valuableness of their contribution. Particularly noteworthy is its stimulating direction to the important investigations that should follow.

Brooklyn College ROSALIND GOULD

The Family Meets the Depression: A Study of a Group of Highly Selected Families. By WINONA L. MORGAN. Minneapolis, Institute of Child Welfare, Monog. Series No. 19, University of Minnesota Press, 1939. Pp. x, 126.

This investigation is a 1933 follow-up of Ruth Lindquist's *The Family in the Present Social Order,* which dealt with the circumstances of several hundred "normal" families in 1927—a "prosperity" period. Miss Morgan "has a dual purpose—to serve as a check-up on 331 of the highly selected families studied by Miss Ruth Lindquist in 1927, and to try to discover the effects of time and of the depression on these families" (p. 3).

This was accomplished by an analysis of written questionaries answered by the 331 families, plus oral interviews with mothers in 110 of them. The written questionaries included items concerned with the economic status of the families in 1927 and in 1933. As a typical comparison of 1933 with 1927: "The percentage of mothers who worried over their children had decreased from 26.7 to 8.8, this difference being 4.7 times its standard error. On the other hand, the percentage worrying over outside activities such as church or club had increased from 0.5 to 8.0, a difference that is 4.2 times its standard error" (p. 37). Are such sweeping conclusions justified by answers on a mailed questionary to, "What are the activities that are your chief source of worry?"

The real contribution to psychological research is in Chapter VIII on "The Personal Interview" and in Appendix B. The correlations between the results on the written questionary and the personal interview are remarkably high, and this is of especial significance because the answers on the questionary were unknown to Miss Morgan at the time of the oral interviews. The written and oral answers were of approximate equal accuracy on objective information, but the oral interview was apparently more accurate and complete on subjective judgments. Miss Morgan, however, committed the "generosity error;" *e.g.* she writes that "two-thirds of the mothers seemed to have personalities of more than average attractiveness, and only about 10% seemed to be less attractive than the average woman" (p. 89).

Several conclusions are of possible interest to psychologists. In 1933 as compared with 1927, the fathers were more willing to have their wives employed outside the home; twice as many families had increased their life insurance than had decreased it; fewer women checked "personal ambition," "a career," and "chance to indulge special tastes or gifts" as their greatest sacrifice in marriage, and the number of mothers had increased who stated that they had made no sacrifice in marriage.

The author says in her summary chapter that conclusions concerning the effect of the depression "would depend on whether one was an optimist or a pessimist. The optimist would dwell on the fact that about half of this group had not been greatly affected by the depression, while the pessimist would point out that about half of the group had been unfavorably affected economically, some of them seriously" (p. 99). Apparently Miss Morgan is an optimist, for in her final paragraph she says, "that collecting the information for this study, particularly visiting the families, was a genuine inspiration" (p. 102). Whether or not Miss Morgan was personally "inspired" by her own work seems somehow beside the point as compared with the possible value of her research for other workers.

The monograph includes a bibliography of 115 references and a complete index.

George Washington University STEUART HENDERSON BRITT

Population, Race, and Eugenics. By MORRIS SIEGEL. Hamilton, Ontario, M. Siegel (Davis-Lisson, Printers), 1939. Pp. x, 206.

This treatise is divided into two books of five chapters each. Book I, devoted to eugenic devices of a positive nature, treats of such topics as the birthrates in the various strata of society, the causation of the greatly reduced rate in the higher strata and the less reduced rate in the lower strata, constructive recommendations by which to achieve a reversal of the birthrate tendency, various racial theories as related to eugenics, and a defense of rational or intelligently planned marriages based on eugenic considerations. Book II treats of negative or constrictive eugenic schemes, notably sterilization, to prevent the hereditary transmission of feeblemindedness, manic-depressive psychoses, dementia praecox, and epilepsy. It ends with general conclusions arrived at throughout the entire treatise. The work refers to a bibliography of 72 articles, books, and reports.

The reviewer is wholly sympathetic to the author and to his point of view. Various eugenic schemes should be introduced and should receive wide application to increase the population of able persons and to reduce the population of the unfit. He regards this work, however, as containing little new or valuable matter of interest to professional psychologists. Obviously the author, a general practitioner of medicine, is versatile and erudite, but his book will be valuable chiefly to lay readers. Professional anthropologists, sociologists, and eugenicists will not seek it as a sourcebook of data or experimental proofs. One may, however, commend heartily four aspects of the work. First of all, it is written with moderation and shows an undogmatic, temperate tone. This is particularly evident where the author discusses the painful subject of Count Gobineau's rendition of the "great Nordic myth" and its current application in Germany, and where the difficult problem of heredity *vs.* environment is considered. Secondly, the book gives full *pros* and *cons* for nearly every issue that arises. Thirdly, the author takes the modern and widely held position that there are no 'pure' races of people, certainly no pure Aryans nor pure Semites. In fact, the reviewer found his discussion of the 'race' question to be most interesting. It was illuminating to discover that early Jews or Semites had mixed freely with Egyptians, Assyrians, Hittites, Babylonians, and Persians; that they had proselyted freely but wholly on a basis of credo; and that some of their settlements in Europe go back 2000 years. Finally, there is consideration of various national and state laws bearing upon sterilization procedures. For example, it is valuable to know that the Supreme Court of the United States, on May 2, 1927, handed down a decision upholding the sterilization law of the State of Virginia as not contravening the Constitutional rights of the defendant and upholding the purely eugenic purposes of the law. On the other hand, it is valuable to know that the Supreme Court of Utah, on April 9, 1929, refused to confirm an order which was designed to bring about *punitive* sterilization of a negro prisoner. Throughout, the author insists that eugenic considerations be the sole basis for sterilization. In fact, the treatise is thoroughly wholesome, conservative, and fair in both outlook and treatment. The diction and proofing, however, of the book leave much to be desired.

University of Oregon H. R. Crosland

Psychologische Zwillingsuntersuchungen. By Arno Lehtovarra. Helsinski, Finland. Akateeminen Kirjakauppa, 1938. Pp. 460.

In the decades immediately following Galton's publication of "The History of Twins as a Criterion of the Relative Powers of Nature and Nurture" (1876) it appeared certain that the basic problems of genetics would soon be solved. Today we have more unsolved problems than ever. Lehtovaara's psychological inquiry undertakes to organize and clarify these problems, and, wherever possible, to supply evidence which may lead towards their solution. The book is not, however, a storehouse of answers. Its principal contribution lies in the emphasis Lehtovaara places upon the need for strictly *psychological* data rather than those derived from biology and botany. The book's defects are consequently the defects of all attempts at exact psychological measurement, for if tests are but partially conclusive when applied to large samples of the general population, they are certainly not more so when employed with a small number of twin cases.

One hundred and forty-four pairs of twins were studied: 58 identical, 70 fraternal, 16 opposite-sexed. Their ages ranged between birth and 17 yr. but the majority were from 7 to 17 yr.; all were of Finnish parentage and most of the cases were chosen from the schools of Helsingfors. Intelligence ratings were found to be slightly closer among the identical than among the fraternal twins, but the difference is negligible. As regards their attachment to one another it was found that identical girl-twins were strongest, but in all cases the advent of puberty tended to decrease twin-attachments. In general it appears, however, that close resemblance between identical twins is being continually enhanced by their almost identical environment: similarity of interests ranks very high among the identical twins and hence it is natural that they should regularly be together in whatever they do.

When shown amusing or terrifying pictures the fraternal twins were more inclined after their initial response (laughter, startle) to *look towards one another* than were identical twins. Among the identicals there appeared a tendency which might be called *pair-polarization:* "A" usually looked towards "B" while "B" seldom looked towards "A." Reaction-time varies less between identical than between fraternal twins. Character and intensity of emotional responses were in much closer conformity among the identical than among the fraternal twins. No great differences were found between the two types of twin groups as regards eidetic imagery, although there was a slightly higher conformity between identicals than between fraternals.

A series of extremely interesting pictures is given at the end of the book. They are mostly stills taken from moving pictures made while the children were responding to various stimuli, but several every-day snapshots are also included. As a reference book for specialists in the study of eugenics or twin up-bringing, this book would undoubtedly prove very valuable; as a contributor of new knowledge, it offers but little.

University of Arizona WILLIS D. ELLIS

Social Forces in Personality Stunting. By ARNOLD H. KAMIAT. Cambridge, Mass., Sci-Art Publishers, 1939. Pp. 256.

General Hugh S. Johnson has a newspaper column, "One Man's Opinion." Arnold H. Kamiat has written a book which is certainly "one man's opinion." This opinion, the thesis of the entire book, is that "most of the physical adults of all races and nations of which history has any record, have been psychologically, that is, intellectually, emotionally and volitionally immature" (p. 11); and this is explained by the fact that society has always been exploitative, autocratic, and competitive. This three-fold division of "intellectually, emotionally, and volitionally" (*cf.* cognition, feeling, conation) is followed throughout, with many instances of the three-fold immaturity of leaders in industry, politics, and other fields. Granted that the rationality of man is too often naïvely assumed, Kamiat certainly does not prove the "collective paranoia" of men. If he were not constantly leading the charge of the Light Brigade, he might be more convincing.

Nor is he always consistent. How can he argue that "it is mere tautology to assert that people do wrong because human nature is wicked" (p. 21), and then insist that "man is engaged in a ceaseless struggle against against some sexual impulse" (p. 52)? From the reputed increase in the number of suicides among American business men, is it a *sequitur* to say that "thus competition, exploitation and autocracy . . . thwart the growth and integration of personality" (p. 62)? Or does it follow from the fact that people "esteem themselves very practicable and realistic," that "this itself is proof of their naïveté" (p. 89)? Would any psychologist agree that superior administrative and executive ability, information, and shrewdness are not "necessarily an index of intellectual superiority" (p. 67)?

The author solves the problems of mankind in terms of a visionary program including "democratization" of human activity, socialization of the instruments of production and distribution, establishment of freedom of thought, abolition of war, compulsory education. He somehow fails to tell us how to accomplish these schemes.

The author has read so widely that he is able to wander quite easily from a chapter called "An Interpretation of History" to one on Plato and Fichte, followed by a chapter on sex patriotism. "Male Masochism and Culture" is saved for the Appendix. Fortunately, the book has a complete index, both of names and of subjects. This is essential in a work which seems to consist so largely of free association of ideas.

George Washington University STEUART HENDERSON BRITT

The Mechanism of Thought, Imagery, and Hallucination. By JOSHUA ROSETT. New York, Columbia University Press, 1939. Pp. x, 289.

Dr. Rosett is Professor of Neurology in Columbia University and Scientific Director of the Brain Research Foundation. A work on the mechanism of thought by a neurologist of this standing should be an important event. Unfortunately the book shows an ignorance of psychology that is almost complete. A few quotations will serve for illustration.

> "The changes which take place in the living body as a result of its collision with the surrounding forces constitute *the physical basis of the emotions. . . .* These are indexed by certain changes in the nervous system known as *the feelings of an emotion*" (p. 20). "The person who is easily 'moved' to expressions of joy or sorrow is not as a rule endowed to any high degree with the associative functions of the cerebral cortex" (p. 30). "It is such nerve impulses which constitute a sensation . . . it is the relatively exact appraisal of a situation by the cortex which constitutes *sensation*" (pp. 89 f.). "*Sensation,* which unlike *feeling,* is graded not in terms of intensity, but in terms of clarity and accuracy" (p. 100). (All italics are the author's.)
>
> "THE PYRAMIDAL TRACT AS THE NERVE PATHWAY FOR THE CONDUCTION OF THE WILL" (p. 105, original capitals).

No psychologist will maintain that the present terminology of our science is perfect, or that it is the only possible one; but it is, after all, the product of a hundred years of hard thinking and experiment, experiment as competent as that on which neurological science has been built. Ignorance of it, and of the distinctions which have been hammered out by a hundred years of research, wantonly invites confusion. The quotations here given, and others which could be given, show an innocence of psychology which ranges from lack of acquaintance with accepted terminology to statements some of which are questionable and others wrong.

These are psychological matters. One is surprised further to find the remark that in Pavlov's well known experiment "hunger . . . [is] in this case the 'unconditioned' stimulus" (p. 144). Pavlov, at least, intended his experiments to be of a purely physiological nature.

Nevertheless, in passing psychological judgment upon this book, one feels uneasily like a pedant. There are so many acute neurological observations, so many valuable diagrams. For example, Figure 10 shows the "manner in which a point in space can be reached by any one of a number of movements and postures." According to the text, records made of simultaneous movements at twelve joints of a person who is tracing a cross on the wall in front of him, "show that even under what appeared to be exactly the same conditions, the movements at each of the twelve joints in successive repetitions of the same act were different" (pp. 197-198). This observation has fascinating possibilities for the theory of learning. In the same way, the author's observations on the effects of picrotoxin are of great significance to psychologists. According to Dr. Rosett, picrotoxin is a drug which, when suitably administered, paralyzes the functions of the cerebral cortex of the cat. Negative adaptation to a handicap is stated to be abolished by its use. The reader will have no difficulty in finding many other observations which will be new to him—at least they were so to the reviewer—and which will provoke psychological thought.

In the opinion of the reviewer, the book has not succeeded in advancing the theory of the *Mechanism of Thought*. This, the reviewer believes, is because Dr. Rosett's fruitful and ingenious mind was not provided with the easily accessible psychological material on which to work.

Queen's University GEORGE HUMPHREY

The Psychology of Making Life Interesting. By WENDELL WHITE. New York, Macmillan Co., 1939. Pp. xv, 215.

This is a popular book. As a representative of its class, it clearly indicates some of the reasons for the layman's distrust of psychology. For one thing, the title is so much bait. It implies that the author is approaching his problem armed with facts and theories which arise from and are peculiar to the science of psychology. This is not true. Five of the chapters might well have been written by a teacher of composition and rhetoric. The remainder is less a scientific discourse than an essay on some commonly accepted notions concerning social relations. The title also indicates that the reader will be given specific and practical plans for the construction of an interesting existence. If such plans are given, I could not find them.

Dr. White's basic principle is that "by providing well-varied experiences for other persons and by achieving well-varied experiences ourselves, we make life for others and for ourselves interesting." This may seem a somewhat oversimplified doctrine. That it seems so to Dr. White is shown by the fact that each of his discussions of methods for obtaining variety is accompanied by warnings that the method can be "carried too far." How far is far enough remains a mystery.

For the most part, the reader is bombarded with a series of old saws expressed with an enthusiasm of discovery which borders on the naïve. In writing, we are told, sentence structure should be varied. The use of dialect in conversation may give tang to the speech if it is not employed more often than once in two years. A look may convey much. Children who have nothing with which to amuse themselves may start fires. Hobbies, leisure, vacations, and nature are good things.

There is some evidence that Dr. White has not examined all of his statements with a critical eye. Thus, in making a case for variety in work as opposed to uniformity, he presents two tables of comparisons of accomplishment on days of uniform and on days of varied routine. The tables are contradictory. They are based on an insufficient number of cases and the differences between varied and uniform work are insignificant. The author admits all this and then proceeds to draw three very definite conclusions from his tables.

On the whole, however, the frequent use of modifying phrases and the subjunctive case gives evidence that Dr. White has attempted to make his book an honest one. So much is not, however, of prime importance. What is important is the question of whether these generally accepted notions need to be put into yet another book and, if they do, whether the result should be left on psychology's doorstep.

Tulane University S. RAINS WALLACE

Mental Hygiene. By W. H. MIKESELL. New York, Prentice-Hall, 1939. Pp. xvi, 456.

"Without a knowledge of the principles of suggestion a person is handicapped in seizing the throttle of mastery of his own mind. A study of mental hygiene must be embedded in the study of suggestion if it is going to be comprehensive" (p. 83). This quotation contains the essence of Dr. Mikesell's treatment of mental hygiene. According to the author the individual "wants a psychology that may help him solve his personal problems and eradicate tendencies that interfere with his success" (p. viii). Such a psychology is based upon a knowledge of suggestion. This "dynamite of the mind" (p. 85) fits into the psychological scheme of things in the following manner.

There are five "sources of human behavior or levels of conduct," viz. impulse and instinct, reflex, habit, conscious thought, and the subconscious (p. 76). Conscious thought has been neglected by psychologists, but the author analyzes it as follows: there are four categories of ideas, viz. suggestion, information, reason, and mental background. By a process of elimination (p. 85) it is discovered that ideas of suggestion are the kind that cause our mental entanglements. Therefore, if mental health is to be achieved, we must get our suggestions straight. Thirteen out of twenty-one chapters are devoted to the elucidation of the intricacies of suggestion. There can be no doubt of the importance of suggestion in human behavior; there is very definite doubt if any useful purpose is served by treating suggestion as a specialized type of mental energy that is directly responsible for our acts.

If we read the book as a technical, even scientific, treatise as the preface and the publisher's advertising consider it, we can only be dismayed at the naïveté. For example, "the neurasthenic could easily throw himself into activity and enjoy it without fatigue, if it were not for bad suggestion" (p. 130). Also, women are more suggestible than men probably because of "their high-strung nervous systems" (p. 179). A phlegmatic disposition is attributed to "slow transmission of the nerve impulse" (p. 337), as is also the sluggish type in which "the nerve is heavy and ponderous; the nerve current is slow in transmission" (p. 387). Removal of passages like the above from their context is always a little suspect, but I do not believe that the procedure in this instance has done the author any injustice.

This book is in no sense an important contribution to either psychology or mental hygiene. The publisher's claim that it is well-documented is hardly borne out by the 125 footnote references of which 56% (including duplication) were to works published earlier than 1920. It cannot be recommended for use as a college textbook. It might have real possibilities as a trade book if its title were made more attractive—and if the book were more accurate. As a trade book it would be a great improvement over the self-help manuals now available.

Indiana University C. M. LOUTTIT

Fulcra of Conflict: A New Approach to Personality Measurement. By DOUGLAS SPENCER. Yonkers, New York, World Book Co., 1939. Pp. 306.

The author starts off as if he were concerned with personality in general, but it dawns upon the reader sooner or later that he is interested in maladjustment, which is taken as synonymous with inner conflict. The book has nothing to say about the kinds, causes or treatment of conflict. It is a history, lucid and succinct, of the development of a new paper-and-pencil test for measuring the extent of conflict among normal adolescents.

Impressed with the efficacy of intelligence tests, the author deplores the comparative unreliability of the "self-report type" of personality inventory. With admirable brevity the several sources of large error in all such procedures are enumerated. Then the author, proceeding on the hypothesis that an aberrant form of behavior is a sign of maladjustment only if it has engendered inner conflict, sets out to devise an instrument to measure the latter, assuming that intensity of conflict is equal to the extent to which the subject's performances fall below the following six standards: his ideals, his father's ideals, his mother's ideals, his father's behavior, his mother's behavior, and the behavior of his closest associates. These standards by some feat of the imagination are termed the *fulcra* of conflict; all the information about which are supplied by the subjects, who fill out an inventory of 53 items seven times, the items in each form being appropriately reworded.

The author's idea is a good one and the internal validity and reliability of his instrument has been checked statistically; but he has withheld from this volume the crucial information. There is only the promise of a later publication to appease the reader's reasonable curiosity on two points: Does this test measure inner conflict? Is inner conflict a good index of maladjustment? Maladjustment as measured by this test should prevail among children with high ideals and superior parents, and conflict (guilt feelings) should be common among those who are free and uninhibited in their behavior. Common experience tells us the opposite; high ideals tend to lift a person out of the slough of mediocrity (some tension between ambitions and attainments being normal and invigorating); and dilemmas of conscience are more frequent and disastrous among inhibited individuals whose conduct is exemplary. Nevertheless Dr. Spencer's test should be efficient in distinguishing most of the subjects with inferior complexes, even though some of the least normal cases will pass unnoticed. There should be a law against employing such a test to separate goats from sheep without adequate supplementary interviews by an experienced psychologist. *Fulcra of Conflict* is a neat report of work in progress rather than a finished book.

Harvard University HENRY A. MURRAY

Marihuana, America's New Drug Problem. By ROBERT P. WALTON. Philadelphia, J. B. Lippincott Company, 1938. Pp. 223.

In eleven short chapters, the authors discuss, in simple and very readable style, the history and distribution of the Hashish Vice, the present status of the Marihuana Vice in the United States, the characteristics of the hemp-plant source of the drug (*Cannabis Sativa L*), the technique of administration, the acute and chronic effects of addiction, the therapeutic application, and the chemistry and pharmacology of hashish. They point out that though hemp has been grown in the United States since colonial times, its use in this country for euphoric or narcotic purposes was practically unknown before 1910. Since that time, however, the habit of smoking "reefers," or marihuana cigarettes, has spread quite widely over the country and has resulted in prohibitory legislation in every state in the Union.

The fact that the Cannabis, or hemp plant, grows as a weed in our vacant lots creates a serious problem in control and the authors cite the New York City figures for June and July, 1935—("260 lots were cleared of 170 tons of Cannabis Sativa plants")—as an indication of the size of the problem from the plant-eradication standpoint.

Quite at variance with certain other workers, the authors definitely state that "We are . . . convinced that there is a certain degree of addiction when this drug is smoked for an extended period of time, not all, but a great many uses, find it necessary to take larger doses. . . . Still more serious than the degree of addiction to marihuana, is the number of instances in which its use leads to the heroin habit. From a survey of 58 heroin addicts in New Orleans, we have ascertained that 44% of them began their drug career by smoking marihuana. . . . There are occasional, but recognizable, instances in which a narcotized individual loses complete control of judgment and restraint and commits totally unpremeditated acts of violence."

For the psychologist the hundred pages devoted to descriptions of the hashish experience, and to the acute and chronic effects of hashish intoxication, have considerable interest and it is worth noting that these authors do not subscribe to the popular notion—probably derived from Dumas' description in the Count of Monte Cristo—that hashish narcosis is particularly characterized by erotic stimulation and aphrodisia. They explain any erotic effects that might be observed in the hashish experience as most likely due to the shutting off of the impulses from the higher brain and the release of the lower more primitive structures as in alcoholic intoxication.

For the general problem of marihuana addiction, the authors offer no simple panacea. The particular value of this volume lies in the fact that it pulls together and attempts, quite successfully, to evaluate the observations made during the past several generations by over 400 different authors. This monograph, though it leaves many questions still open, removes marihuana addiction from the ranks of the obscure or unknown hazards and places it along with such well-known hazards as alcoholism, heroin addiction or morphinism, and for this we owe Dr. Walton a real debt of gratitude.

Cornell University D. F. SMILEY

Dr. Juan Huarte de San Juan und sein "Examen de Ingenios." By MAURICIO DE IRIARTE. Münster, Aschendorff, 1938. Pp. 208.

Huarte's *Examen* seems to have had for more than a hundred years a considerable influence on scientific methodology in Europe. This remarkable book is comparable to Galton's *Inquiries into Human Faculty* (this comparison is the reviewer's) in spite of having been written three hundred years earlier and lacking the advantages of three centuries of progress in anatomy, general physiology and neurology. The full title is *Examen de ingenios para las sciencias* which may be translated *Investigation of differential aptitudes for various professions.* It was written in the Spanish vernacular and not in Latin, the customary language for scientific books of that time, thus showing the practical ideals of its author. Huarte was of Basque origin, born about 1530, taken by his parents to Castile; he studied medicine in Alcalá and elsewhere, lived most of his adult life in Linares and was official municipal physician in the neighboring Baeza. His doctrine that "the souls" in spite of being created free and equal had to use the biological properties of the human body as tools and thereby became virtually unequal, aroused the suspicion of the Sanctum Officium, so that he had to use more and more cautious language in order to satisfy the Inquisition in successive editions of the book.

The author of the present biographical and bibliographical investigation, Iriarte, is a Spanish Jesuit who has done a very meritorious piece of scholarly work in demonstrating the highly developed state of Spanish science in the sixteenth century in the person of one who might well be called the pioneer and founder of modern differential psychology, personnel research and vocational guidance. Huarte's *Examen* was translated into all the important European languages, a total of seventy editions. Iriarte believes that many of the ideas in Francis Bacon's *Novum Organon* have their origin in Huarte's *Examen* or at least can be said to have been pronounced there first. Huarte's book seems to have had some influence also upon Gall, although not in the sense of being a precursor in the theory of brain localization.

The biological concepts of Huarte are naturally those of his epoch. The different humors of the body play their rôle in his system. They were in harmony as long as Adam lived in the Paradise. After his expulsion their harmony was lost and this explains that Adam could become the father of such a son as Cain. Theories of heredity are of great importance in Huarte's thinking and are applied to social and political problems such as the probable value of a monarchy and an hereditary nobility. That good heredity without proper education is ineffective is also stressed. Iriarte shows in his book written in German and published in Germany that differential and educational psychology in Spain date back to the sixteenth century.

University of Miami MAX F. MEYER

The Emotions: Their Nature and Influence upon Human Conduct. By ROBERT PARIS CARROLL. Washington, D.C., Daylion Co., 1937. Pp. 208.

If one reads this book holding in mind the question, "What is an emotion?," a variety of answers emerge. In the preface is a statement that emotions are the "most powerful, most far-reaching of all the driving forces of human conduct." In the first chapter, however, emotion is defined as a "response state or general, stirred-up condition of the body as a whole;" emotions are said to vary in degree of agitation, or "klonesis," from acute turmoil to the more quiescent attitudes, sentiments and wishes.

The influence of Shand and McDougall is clearly apparent in the author's classification of emotions. Emotions are said to be distinguishable as lower or higher, as simple or complex, and from the standpoint of intensity as stronger or weaker, the weakest being the sentiments or attitudes.

The content of the book is varied. It deals with emotional sensitivity, Darwin's principles, the neural and endocrine factors in emotion, the James-Lange and Cannon theories of emotion, the effect of emotional states upon digestion and health, exercise and the control of emotion, emotion in relation to learning, and other traditional topics. In the last three chapters reference is made to psychoanalysis, the psychoses, mental hygiene, adjustment, and the psychology of everyday life—forgetting, slips of the tongue, and delinquency.

The book is planned for elementary students of psychology and education. It was not intended for psychologists, nor does it pose as a scientific contribution to the analysis of emotions. The lack of a consistent treatment of the nature and rôle of emotion is a reflection of the discrepancies existing in current psychology.

Throughout the treatment is elementary, the statements for the most part clear and accurate. There is a practical, human emphasis which makes the book readable.

Parts of the work could be used profitably to supplement an elementary study of emotion, but as a whole it is too light for a college text. At the close is a bibliography.

University of Illinois PAUL THOMAS YOUNG

Psychology of Personality and Social Adjustment: A Handbook for Students. By ROBERT W. LEEPER. Mt. Vernon, Iowa, Cornell College Bookstore, 1937. Pp. 79.

Prepared as an aid to student's self-learning under guidance of occasional lectures and personal consultations, this book is not intended to serve as an orthodox textbook. Rather the plan features constant use of a list of references covering about twenty-five titles and featuring relatively specialized books on personality. Flexibility is hence possible by permitting a selection of particular topics in accordance with the student's interests.

The volume is divided into four parts. An introductory section gives some suggestions that are designed to motivate the student and also to secure efficiency in his work. Part II deals with the abstract functions of the psychology of personality including motivation, personality development, and motivational frustrations. Part III takes up the more specific phases of the psychology of personality. This section is easily the more significant portion of the book, dealing with methods of meeting normal social relationships and mild personality defects of one's own. Sex motivation and sex adjustment and other aspects of the affective life are included together with personality scales and tests. Part IV is given over to supplementary optional topics.

The reviewer believes that Dr. Leeper has made a contribution to the problem of interesting students of the large category who are not primarily interested in following psychology either in teaching or research, but who want concrete information and suggestions regarding matters of particular interest to themselves. In that sense, the book will be found useful and, in the reviewer's opinion, it may further the end of motivating students to enter and continue in other courses in the field. Certainly it is at least debatable that most orthodox texts might stand improvement in this general direction.

University of Iowa NORMAN C. MEIER

The Nineteen Thirty-Eight Mental Measurements Yearbook. By OSCAR KRISEN BUROS. New Brunswick, New Jersey, Rutgers University Press, 1938. Pp. xv, 415.

In 1935 the School of Education of Rutgers University published a modest 40-page handbook, *Educational, Psychological and Personality Tests of 1933 and 1934.* The work thus started was kept up-to-date by more ambitious volumes issued in 1936 and 1937, and the series reaches maturity in the yearbook here under review. This volume restricts itself largely to material published during 1937 and the earlier part of 1938. The first section is devoted to tests of all sorts; tests of achievement in the various subject-matter field, of intelligence, of character and personality, of interests, attitudes, habits, etc.—over 300 in all. The second and third sections list over four hundred books which fall within the fields of mental measurements and research. There is also a list of recent regional reports of testing programs, and directories of periodicals and publishers.

An outstanding feature of this Yearbook is the critical reviews which accompany the listings of most of the tests and books. Many of them were culled from educational journals; the rest were written expressly for the Yearbook by selected reviewers. In any event it can fairly be assumed that each review reflects the opinion of an expert, and for many of the more important books and tests we are given several such authoritative opinions. For educators faced with the problem of selecting appropriate tests from the vast reservoir of material on the market these reviews offer a very great service; they make unnecessary most of the guesswork which now characterizes much of the purchasing of standardized tests. Many of the reviews, of course, even though written by experts, will not be above criticism, but this can hardly be avoided, and it would be difficult to offer any important suggestions for the improvement of Buros's work. He is giving us a series of handbooks which should prove indispensable to those engaged in the work of educational measurement and research.

University of Toronto JOHN A. LONG

The Effect of Praise and Competition on the Persisting Behavior of Kindergarten Children. By THETA HOLMES WOLF. Foreword by John E. Anderson, The Institute of Child Welfare, University of Minnesota. Monograph Series No. XV. Minneapolis, University of Minnesota Press, 1938. Pp. 138.

The author has compared the lengths of time which 5-yr.-old children would voluntarily and continuously spend at tasks of varying degrees of intrinsic interest under the following conditions; when the child worked alone with no incentive experimentally produced in the environment; when the child worked in the presence of an adult who praised the performance; and when the child worked in the presence of another child similarly employed and with both children under instructions to compete with each other. A carefully developed statistical treatment of the time records of these children indicated that the effect of praise and competition on persisting behavior varied with the intrinsic nature of the task. Varying factors involved in the tasks were: the degree of fine- and large-muscle motor skill; monotony; and demand on intellectual resourcefulness. The factors of sex, socio-economic status, age and intelligence quotient gave "no reasonable evidence of affecting differentially the no-incentive, persisting-time performances." Analyses of individual modes of procedure against the home and school background, as determined through interview and questionary, indicated the presence of complex attitudes and personality trends affecting the performance of the children in this experimental situation. The author concludes that persisting behavior is highly dependent upon several variables which include the kind of task, the specific conditions of the incentive situations and broad personality characteristics. The reviewer regrets that the factor of companionship was not subjected to the same careful experimental control which was used with other variable factors in this experiment. The "no-incentive" situations were always without immediate companionship for the child; the "praise incentive" situations always contained adult companionship for each child; and the "competitive" situation always contained the companionship of another child. Some children would engage in any form of activity no matter how disagreeable to them merely to be with another child. Other children find practically no form of activity can compensate for having to remain alone with an adult. This uncontrolled variable somewhat blurs the comparison of praise and competition as motives which would prolong the presence of the child within the situation.

Wichita Child Guidance Center, Wichita, Kansas EDWINA A. COWAN

The Book of Time. By GERALD LYNTON KAUFMAN. New York, Julian Messner, 1938. Pp. xvi, 287.

The volume is a series of 36 random chapters, in 7 parts, on some of the puzzling things about temporal experience and time concepts, chiefly in their mathematical and metaphysical aspects. An effort is made to popularize the philosophical and speculative riddles and to present lightly and often facetiously some of the problems and conclusions of Kant, Newton, Eddington, Jeans, Ouspensky, Einstein and others, without probing very profoundly into any of them.

Definitions, dimensions, measurement, the calendar, entropy, relativity, the 'absolute,' physiological rhythms, and various spatial and temporal paradoxes are lightly touched on, often in a humorous and imaginative vein.

The volume presents nothing of special interest to professional psychologists. A few psychological studies are given hasty and deprecatory comment in Part VII. The author's psychological orientation is perhaps indicated by his assertion that "Dr. Edward L. Thorndyke" coöperated with Kennedy in the repetition of the Michelson-Morley experiment at Pasadena. The intrinsic heaviness of the theme is relieved by occasional jocose observations such as that "the Egyptians were the first people to take Time 'Siriusly.' "

The treatment has sufficient vivacity and simplicity to interest the general reader or the young college student in a wide variety of theoretical topics, and the author has a talent for presenting the abstract issues in concrete and somewhat Lewis Carroll style, without trying to carry them too far. The review copy was loaned to a class of 11-year-old "rapid learners" in Speyer School, P.S. 500, New York City, who were pursuing an enrichment unit on "Time and Time Keeping." These bright children were enthusiastic over the book, read it with interest and a good deal of comprehension, and offered to write this review if they might thereafter keep the book in their library.

Barnard College H. L. HOLLINGWORTH

Psychology and Teaching of Secondary-School Subjects. By HOMER B. REED. New York, Prentice-Hall, 1939. Pp. vii, 684.

Controlled studies in educational psychology and the hundreds of pedagogical applications are scattered through numerous small journals, reports, and the limited publications going to various teachers' organizations. Many of these studies are hidden away in unpublished masters' and doctors' theses. Dr. Reed has painstakingly reviewed, organized and interpreted these studies with the specific aim of pointing out their practical application for the study and teaching of the secondary school subjects.

The usual standard high school subjects are treated in methodical order and the usual questions on methods, material, intelligence, individual difference, possible reforms in teaching and so on are treated in logical order and in somewhat similar pattern from subject to subject so far as compatible with the nature of the material.

The author has consistently maintained an eclectic viewpoint with the aim, undoubtedly, to get all available contributions of a constructive nature that will aid the high school teacher. The book is not critical from the standpoint of basic theories in education and psychology, but is shrewd and practical and of great value to any teacher in making today's teaching a "going concern."

The book would seem to have its chief value in state teachers colleges and schools of education. It is a mine of information and will save the reader a lot of tedious and aggravating search for the minor literature in this field so widely scattered during the last ten years.

Students in the field of secondary education will find this book an excellent starting point in the search for a master's or doctor's problem. Teachers of educational psychology will find it an excellent reference book.

University of Pittsburgh W. T. ROOT

Papers on Psycho-Analysis. Fourth Edition. By ERNEST JONES. Baltimore, William Wood & Co., 1938. Pp. viii, 643.

In its present form this distinguished work remains one of the finest available expositions of psycho-analytic theory. The author presents all of the basic Freudian principles with force and clearness. His own criticisms and those of some others are given effective, though brief, treatment.

Since the publication of the Third Edition of the book in 1923, developing psycho-analytic theory has centered around the nature and dynamic activities of the ego and the concept of the death-instinct. In the Fourth Edition these problems have been discussed less completely than seems desirable.

Papers on Psycho-Analysis is a republication of thirty-two articles previously published in various scientific journals. Twelve largely conventional papers among these have not appeared in earlier editions of this book. These include discussions of the origin and structure of the super-ego; the nature of the instincts; fear, guilt, and hate; the Mantle Symbol; an especially fine treatment of jealousy; and three papers on the sexual development of the female.

University of North Carolina ENGLISH BAGBY

The Influence of Nursery School Experience on Children's Social Adjustments. By ARTHUR T. JERSILD and MARY D. FITE. New York, Bureau of Publications, Teachers College, Columbia, 1939. Pp. xii, 112.

This, the twenty-fifth and apparently the last of the Columbia Child Development Monographs, uses the modified diary technique described by Jersild and Markey, but gives much more emphasis to the group situation in which the individual behavior is to be observed and described. The structure of the group, indeed, proves so overwhelmingly important as a determiner of social behavior as to outweigh nearly all the other factors. The authors, therefore, are rightly very careful about making generalizations concerning the influence of other social factors. The intensiveness of the study also restricted it to a small number of cases—only eighteen. Within these limitations, the conclusion is drawn that 3-yr.-old children with previous nursery school experience are initially more receptive to social contacts than 'new' children. The 'new,' however, make large initial gains and by spring have, as a group, overcome their initial disadvantage.

"While the general trend," the authors state, "is toward a sharply increased amount of fraternizing—not all participate in this trend." Individual differences in social contacts—save those associated with previous nursery schooling—are probably little reduced by the year's experience. Lastly and most important, is the insistence that it is necessary "to consider his various acts in relation to the record of his behavior as a whole." One wishes sometimes that the last were not true; it makes the protocols discursive. But no careful investigator of child behavior dares ignore this principle. Indeed, investigators of later stages would do well to bear it more constantly in mind.

A condensed statement of the findings has been published by the authors under the title, Children's social adjustments in nursery school. (*J. Exper. Educ.*, 5, 1937, 161-166).

Ohio State University H. B. ENGLISH

Phonetics and Phonology. By BAREND FADDEGON. Amsterdam, Noord-Hollandsche Uitgevers *Bull. Roy. Netherland Acad. Sci. Arts,* N.S., 1, 1938, (no. 10), 1-45.

Several decades ago psychology was loath to admit the existence of her stepsister, psychoanalysis. The latter, it may be recalled, did not wait for sisterly recognition; she introduced herself to a somewhat unfriendly world. Phonetics, too, has a so far but poorly introduced stepsister, phonology, a teleologically inclined discipline which holds to a "relational subconsciousness," as something or other which is composed of "perceptional and motoric" parts. It was to justify the conversion of the philologist, Faddegon, to phonology that the monograph under review was written.

The major data were gathered from a psychological experiment performed in 1901. In that year Faddegon constructed several sets of nonsense syllables. These were taught to 7 *S*s who were tested for recall 2 min. later. In analyzing the errors Faddegon found that "closely related" vowels were most often substituted. There was a tendency while making the consonant errors to substitute "mute for mute, sonorous for sonorous, and especially stop for stop, continuative for continuative, nasal for nasal and . . . to interchange homorganic mutes. Thus the crossing system of acoustic associations creates the vocalic phonemes, and the double system of acoustic and kinaesthetic associations the consonantal phonemes."

Many years after this psychological experiment was undertaken, Faddegon came upon somewhat similar substitutions in the final rimes of certain specimens of folk poetry and in an accentric 44 line poem of his own construction. To explain these 3 sets of data the author offers his theory of a subconscious bidimensional system of associations. The theory is so similar to that of psychoanalysis one might guess that its future advocates will employ the now quite familiar Freudian arguments.

Stanford University PAUL R. FARNSWORTH

Social Ecology: A Critical Analysis. By MILLA AÏSSA ALIHAN. New York, Columbia University Press, 1938. Pp. xiii, 267.

The author essays to make a critique of the ecological school of sociology. It may be questioned whether there is any such school in the sense which has been implied by various writers. Rather there are certain sociologists who consider ecological analysis to be a useful method of understanding social reality.

The book gives very little insight into the contributions which have been made by this ecological approach, because it is chiefly concerned with an appraisal of the writings of social ecologists as a body of sociological theory. Miss Alihan attributes to them an integrated sociological system based on human ecology and reads into their writings various doctrines; whereas they were not attempting to create a sociological theory, but to explore and demonstrate the value of a new method of description and analysis of social phenomena. Thus she implies that Park and Burgess wrote their *Introduction to the Science of Society* as an exposition of their ecological point of view, which was by no means the case. Ecological ideas were incidental to their main discussion and most of their studies in social ecology were made subsequent to its publication.

The author confines her discussion almost entirely to the work of Park, Burgess and McKenzie on cities, and seems to have no knowledge of a considerable literature on human ecology which has resulted from the studies of rural sociologists.

The book has little of interest for psychologists, as it is chiefly devoted to a philosophical discussion of concepts of sociological theory and entirely neglects the significant findings of social ecology.

Cornell University DWIGHT SANDERSON

Baby's Point of View. By E. JOYCE PARTRIDGE. New York, Oxford University Press, 1937. Pp. 94.

This little book appears at first glance to be built around the thesis that early infancy is an exceedingly important period in the formation of an individual's later habits and personality. The reader soon finds that the treatment is more sentimental than factual. "Evidence" is asserted, rather than cited from clinical reports, that "memories" go back to events that "must have happened" as early as the third week of life. Such "memories" are very specific and elaborately verbalized. The assertion is made that "at birth an infant has a store of knowledge which he has inherited from former generations, and amongst this knowledge he is a fairly clear idea of the difference between male and female and all that this implies." Wherefore mothers are warned not to "allow themselves to express their disappointment as to the sex of a new-born baby in his or her actual presence." Maternal instinct and intuition are preferred to instruction. "The most successful mother is the natural mother, the mother who does not think too much, the mother who does not know too much, but just uses her instinct."

Of fact, then, the book has little to offer. Whether the aim of arousing in young and uninstructed mothers an all consuming devotion to their offspring justifies the overwhelming sentimentalism is to be seriously doubted.

Hobart College FORREST LEE DIMMICK

Life Without Fear. By PETER FLETCHER. New York, E. P. Dutton & Co., 1939. Pp. vii, 111.

This book attempts to correlate psychological and Christian principles. There are chapters on the inferiority complex, delusions, day-dreaming, rationalization, and other reactions to frustration. The author asserts that one can achieve an integrated personality through will-power and adherence to Christian precepts. The closing paragraph summarizes Fletcher's viewpoint: "The reader will have observed in the foregoing chapters that although I have been discussing what may properly be called psychological conditions, I have thought of them always as the manifestations of spiritual attitudes to life and its responsibilities; and wherever I have suggested any remedial measures, my emphasis has always been upon the necessity for a religious approach to the problem. I have done so because, as the result of my own clinical work, as well as from discussions with psychologists better qualified than myself to form an opinion, I have reached the conclusion that no method of therapy will effect permanent and constructive changes in human behavior except to the extent that it is based upon a realization of the spiritual nature of man, and reverence for personality as the instrument of God for the expression of His nature and the fulfillment of His will."

Western Reserve University CALVIN S. HALL

Principles of General Psychology. By ELLIS FREEMAN. New York, Henry Holt & Co., 1939. Pp. xiii, 530.

Fortified by experiences gained from "nine years of teaching . . . large classes of average ability," Dr. Freeman offers here his conception of a suitable text for students in elementary psychology. In *Principles of General Psychology* he has attempted to answer regularly recurring student questions and to indicate the relations of psychology to life in general and particularly to the nature of knowledge. As bearing upon the last mentioned topic there are chapters on "Language and Number" and "Science, Technology, and Invention."

The text follows a modern trend in presenting but a very brief introductory chapter and in following this by sections on the nature-nurture problem, intelligence, and individual differences. The chapters on perception (including thinking) come in the last section. The text has a strong Gestaltish flavor and exhibits what many will consider an environmental bias.

Freeman's text offers much to the beginning student and to the teacher. Its figures and tables are well chosen and are adequately tied into the main body of discourse. There is repeated reference to the experimental literature. This is accomplished without apparent choppiness—no mean feat in an elementary text. Moreover, the treatment is simple and pleasant. Because of these and other considerations, *Principles of General Psychology* should secure many adoptions.

Stanford University PAUL R. FARNSWORTH

Psychology Applied to Nursing. By LAWRENCE AUGUSTUS AVERILL and FLORENCE C. KEMPF. Philadelphia, W. B. Saunders, 1938. Pp. 471.

The authors stress the approach of mental hygiene in the solution of many practical problems of the nurse. They consider adjustment from the point of view (1) of the nurse herself, and (2) of the nurse in relation to her patients. Their style is clear and direct, and illustrative applications are given at the end of each discussion. Following each chapter a section of 'thought problems' and a series of questions are presented to bring out the practical aspects of the material.

The first chapter suggests that the nurse is emotionally 'grown up,' and lists several criteria of the adult. Following this is a discussion of motivation and adjustment. Three chapters are devoted to the physiological bases of behavior and three more are concerned with the learning process and associated problems. There are chapters on emotion, the psychology of childhood, adolescence, and the family. The concluding chapter places the nurse in the practical situation as a teacher and as a minister to the 'spiritual' as well as the bodily needs.

Although the authors grossly misinterpret the meaning of correlation (p. 295), and although their use of the term *instinct* is questionable, the reviewer feels that, in general, the book offers sound advice and basic facts of use to the beginning nurse.

University of Maryland LESTER P. GUEST

The Activity of Young Children During Sleep. An Objective Study. By CHESTER ROY GARVEY. Minneapolis, University of Minnesota Press, 1939. Pp. x, 102.

This monograph is published posthumously from the author's records and manuscript by the members of the staff of the Institute of Child Welfare of the University of Minnesota, with the assistance of H. M. Johnson. The techniques for recording activity of the child while in bed and the analysis and treatment of results are similar to the procedures used by Johnson and associates in the Simmons Investigation of Sleep. The subjects of the study were 22 nursery school pupils observed for periods of one month to more than one year. Usable records were obtained for over 3300 nights.

The monograph, in keeping with the circumstances of its publication, is primarily factual. The principal contributions are the demonstration of reliable indices of activity, presentation of norms on a considerable group of children and the correlation of activity indices with personal and environmental variables. In doing this, the author has well attained the objective of his study. Individual differences in pattern of nocturnal motility were found to be less than Johnson has reported for adults. Mean rest periods (intervals between movements) were shorter for children, 6.4 to 9.4 min. with an average of 8 min., than for adults, 6 to 22 min. with averages from 7 to 13 min. Children required about twice as long as adults to go to sleep. Quietness of sleep was not related to length of sleep nor to the time of retiring. Suggestive results on the influence of body temperature, age, day naps, daytime activity, posture when going to sleep and ultra-violet radiation are presented and utilized in formulating problems for future investigation.

Washington University School of Medicine C. F. JACOBSEN

Bachofen—Freud, zur Emanzipation des Mannes vom Reiche der Mütter. By ADRIEN TUREL. Bern, Hans Huber, 1939. Pp. 216.

This book is volume 9 of a series of publications called *Bücher der Werdenden* edited by the most ardent present advocate of Freud's psychoanalysis, Heinrich Meng of Basle. The aim of the author is to show the identity of some of the fundamental ideas of Freud with that of Bachofen, the author of *Das Mutterrecht.* The method of the book is chiefly literal and historical rather than psychological although dealing with a fundamental problem of sex psychology. The main topic can be learned from this sentence (p. 128): "As a mass—as 'foule' in the sense of Le Bons—men are always feminine. . . . Without Freud's *Totem and Tabu,* without the major works of Bachofen, we would neither be able to ask this question so important for sociology, nor to answer or solve it."

New York City ERNST HARMS

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THE INFLUENCE OF DEGREE OF INTERPOLATED LEARNING ON RETROACTIVE INHIBITION AND THE OVERT TRANSFER OF SPECIFIC RESPONSES

By ARTHUR W. MELTON, University of Missouri, and
JEAN MCQUEEN IRWIN, Scripps College

In the analysis of retroactive inhibition and its relation to forgetting, one of the relationships most in need of precise determination is that between the amount and locus of the retroactive inhibition and the degree of learning of the interpolated activity. A knowledge of this relationship is essential to the interpretation of many other studies of retroactive inhibition, because other modes of variation in the interpolated activity frequently increase or decrease the degree of learning of that activity even though the frequency of repetition or the criterion of mastery has been held constant. In addition, the form of the relationship has particular significance for theories of retroactive inhibition, as will be shown later.

There have been four systematic investigations of this relationship. McGeoch found,[1] in a study of the learning of serial lists of 8 nonsense syllables by the anticipation method, that retroactive inhibition[2] (RI) as measured by recall scores

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[1] J. A. McGeoch, The influence of the degree of interpolated learning upon retroactive inhibition, this JOURNAL, 44, 1932, 695-708.

[2] The terms *retroactive inhibition* and *proactive inhibition* will be represented by the abbreviations RI and PI, respectively. Some abbreviation is necessary, and these seem preferable to "retroaction" and "proaction" which, especially in the case of "retroaction," emphasize the portions of the terms most subject to misinterpretation. By *retroactive inhibition* is meant merely the fact of poorer retention when some activity, usually a learning activity, has been interpolated between an original learning and the measurement of its retention, than when a period of comparative rest

increased as the frequency of repetition of the interpolated list increased from 6 to 11, and remained relatively constant thereafter. On the other hand, when in the same experiment the RI was measured by the trials required to complete the relearning of the original list to 3 successive perfect trials, and the *Ss* were well practiced (Cycle II), it increased as the frequency of interpolated learning was increased from 6 to 11, remained constant with further increases in the frequency to 16 and 21, then dropped sharply as the frequency was increased to 26. In a second experiment,[3] with lists of 10 nonsense syllables which were learned for various amounts of time by the method of complete presentation, McGeoch found that study of the interpolated list for amounts of time beyond 150 sec. was accompanied by a decrease in the RI as measured by written recall. Lahey has recently used the method of complete presentation and written recall with lists of words to study this relationship,[4] and has verified McGeoch's conclusion only in the case of children with IQ above 105. Average and dull children show no decrease in the amount of RI when the interpolated material is studied for the longest time used in the experiment. She rightly notes, however, that this finding is not inconsistent with the hypothesis that the maximal amount of RI occurs when the interpolated activity is learned to an intermediate degree. A recent study by McGeoch,[5] in which the anticipation method was again used, is not relevant to the question of the effect of very high degrees of interpolated learning since such high degrees were not included among the work conditions.

Although in general these experiments suggest the truth of this hypothesis, there is no case in which the *decrease* in RI with increased learning of the interpolated material is statistically significant. Furthermore, different results were obtained when RI was measured by anticipation recalls in which only 2 sec. were allowed for the recall of each syllable, written recall in which several minutes were allowed for recall of the whole list, and by the trials required to relearn the list to a criterion of perfection. An hypothesis that permits a rationalization of these differences is, however, suggested by McGeoch's study with the anticipation method. If it can be reliably demonstrated that a high degree of learning of the interpolated activity reduces the RI as measured by trials to relearn to a criterion, but leaves unaffected the RI measured by the recall on the first relearning trial, this supports the hypothesis that the RI becomes localized in the early portion of the relearning process when the interpolated activity is overlearned. In other words, the persistence of the interference effects in the face of renewed reënforcements of the original activity becomes less as the degree of interpolated learning is increased from some intermediate degree

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has intervened. By ***proactive inhibition*** is meant either (a) the retardation of the learning of an activity when some other activity has occurred as a prior condition (sometimes called negative transfer), or (b) the fact of poorer retention of an activity when some other activity has occurred as a prior condition of the original learning of that activity, than when a period of comparative rest preceded the original learning.

[3] *Op. cit.,* 701-704.

[4] M. F. L. Lahey, Retroactive inhibition as a function of age, intelligence, and the duration of the interpolated activity, *Cath. Univ. Amer. Educ. Res. Monog.,* 10, 1937, 1-93.

[5] J. A. McGeoch, Studies in retroactive inhibitition: VII. Retroactive inhibition as a function of the length and frequency of presentation of the interpolated lists, *J. Exper. Psychol.,* 19, 1936, 674-693.

to some high degree, although the initial amount of interference, as measured by recall, may remain constant.

Verification of these hypotheses regarding the amount and the locus of the RI in serial verbal learning is given by the experiment to be reported. The design of this experiment follows closely the one with serial learning by the anticipation method reported by McGeoch.[6] Four differences were introduced in order to permit a detailed analysis of the recall and relearning processes: (1) a greater number of *S*s were used; (2) a control condition was introduced to enable an evaluation of the proactive inhibition (PI) of the interpolated learning by the original learning; (3) very difficult lists of nonsense syllables were used in order to prolong the relearning process for all *S*s; (4) *E* recorded not only the correct anticipations and failures to anticipate during learning and relearning, but also the specific erroneous anticipations. In particular, the analyses of these erroneous anticipations have provided important corroborations and interpretations of the results obtained with the usual measures of retention and RI.

TABLE I

SEQUENCE OF EVENTS IN EXPERIMENTAL CONDITIONS DESIGNED TO MEASURE RI AFTER VARIOUS AMOUNTS OF INTERPOLATED LEARNING

Condition	Original learning	Rest	Interpolated learning	Rest	Relearning original list
I	5 trials		30 min. rest		to 2 perfect
II	5 trials	1 min.	5 trials (3.5 min.)	25.5 min.	to 2 perfect
III	5 trials	1 min.	10 trials (7 min.)	22.0 min.	to 2 perfect
IV	5 trials	1 min.	20 trials (14 min.)	15.0 min.	to 2 perfect
V	5 trials	1 min.	40 trials (28 min.)	1.0 min.	to 2 perfect
VI			Continuous learning to 2 successive errorless trials		

METHOD

The *S*s were 24 college men who learned lists of 18 nonsense syllables under each of the 6 conditions outlined in Table I. In the 5 conditions that had directly to do with the relationship between the amount of RI and degree of learning of the interpolated material, the practice on the original list was held constant at 5 repetitions and the relearning was begun 30 min. after the completion of the fifth trial of learning. In the rest condition (Condition I) this 30-min. period was filled with the reading of humorous magazines, and all the shorter rest periods indicated in Table I were filled in this way. The 4 RI conditions were alike in that the first trial on the interpolated lists occurred 1 min. after the completion of the fifth trial of original learning. They differed in the number of trials spent in learning the interpolated list and in the amount of rest between the last trial on the interpolated list and the first relearning trial on the original list. The sixth condition shown in Table I involved merely the continuous learning of a list to complete mastery or for 40 trials, whichever occurred last. One purpose of this condition was to provide a measure of the rate of learning of the interpolated lists when there was no prior learning of an original list for 5 trials, *i.e.* to provide a control for the PI of the interpolated list. A second purpose of this condition was to prevent the *S*s from becoming set for a cessation of the learning of the first list after 5 trials.

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[6] *Op. cit.*, 696.

The syllables were presented at a 2-sec. rate on a pin-drive memory-drum powered by a G.E. synchronous motor, such as described by Ward,[7] and the anticipation method was used throughout. There was a 6-sec. interval of rest between trials, exclusive of the 2-sec. period during which the cue for the anticipation of the first syllable in the list was being presented. Special instructions were used to minimize common misinterpretations of the instruction "to anticipate" and thus to improve the reliability of intra-trial learning records. The obvious, though apparently infrequently recognized, need for such special instructions, and their importance for our detailed analysis of errors during learning and relearning, warrant the following verbatim statement of them.

(1) Alway spell out the nonsense syllable which you think will turn into the aperture next; do not pronounce it.

(2) Never attempt to anticipate a syllable before the one just preceding it has appeared in the aperture; that is, do not get ahead of the drum.

(3) Always check the accuracy of your anticipation by noticing the syllable that turns into the aperture.

(4) Always try to anticipate each syllable in the list, and try to anticipate as many as possible during each trial. You must not concentrate on a few of the syllables during the whole of one trial, then concentrate on a few more during the next trial, etc.; instead, you must study each syllable for the time that it is exposed. If you should be unable to anticipate all the syllables after only a few trials or if you fail to anticipate a syllable that you have correctly anticipated in previous trials, do not permit this to disrupt your learning process because such things are the rule rather than the exception. Under no circumstances should you give up trying to anticipate as many syllables as possible.

(5) If you anticipate a particular syllable, thinking that it will appear next in the aperture, but it does not appear and you think that it will appear on the next turn of the drum, you must call out the syllable again. In short, the syllable must be called out immediately before it turns into the aperture if it is to be counted as correct.

(6) If you are unable to remember a complete syllable, but can remember 1 or more letters of it, call out these letters since these partially correct anticipations are counted in your favor.

During the 3 preliminary practice days, *E* interrupted the learning at any time to point out a failure to follow these instructions.

Control of motivation is a major problem in every learning experiment in which the *S*s sometimes learn to a criterion of perfection and at other times for a fixed number of trials regardless of the degrees of mastery achieved. An attempt was made to establish a general task-set by the following instructions:

(1) You will continue your attempts to anticipate the syllables in the list until

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[7] L. B. Ward, Reminiscence and rote learning, *Psychol. Monog.*, 49, 1937, (no. 220), 1-64.

you have satisfied the degree of perfection required of you or until you have studied the list for a number of trials which the experimenter will determine without your knowledge.

(2) Remember that the time you spend in this experiment is to some extent determined by the speed with which you learn the lists, and that the time you save is your profit if you get through before the hour is up. You are not being paid by the hour; you are being paid by the number of experimental sessions you complete. (This instruction was necessary because all *Ss* were paid for serving in the experiment.)[8]

(3) (Before the relearning of the original list) I am now going to present again the first list you learned today and you are to study it further and relearn it until I stop you. If you can recall any of its syllables during the first time it is presented, be sure to spell them out at the proper time. It is very important that you make a great effort to recall every syllable which you possibly can on the first trials. What you can remember on these first few times is very valuable for our records. Remember what your job is, and at no time fail to put forward your best efforts.

TABLE II

SAMPLE EXPERIMENTAL SCHEDULES TO ILLUSTRATE COUNTERBALANCING OF PRACTICE EFFECTS AND MATERIALS LEARNED

(Arabic numerals refer to the lists of nonsense syllables given in Table III; roman numerals refer to the experimental conditions described in Table I.)

S	Experimental day											
	Cycle I						Cycle II					
	1	2	3	4	5	6	7	8	9	10	11	12
1&13	2 VI	3–4 IV	5–6 II	7 I	9–10 V	11–12 III	14 VI	15–16 IV	17–18 II	19 I	21–22 V	23–24 III
2&14	1–2 IV	3–4 II	5 I	7–8 V	9–10 III	12 VI	13–14 IV	15–16 II	17 I	19–20 V	21–22 III	24 VI
Etc. for 3&15, 4&16, 5&17, and 6&18.												
7&19	14 VI	15–16 IV	17–18 II	19 I	21–22 V	23–24 III	2 VI	3–4 IV	5–6 II	7 I	9–10 V	11–12 III
8&20	13–14 IV	15–16 II	17 I	19–20 V	21–22 III	24 VI	1–2 IV	3–4 II	5 I	7–8 V	9–10 III	12 VI
Etc. for 9&21, 10&22, 11&23, and 12&24.												

The 24 *Ss* went through 2 complete cycles of the 6 experimental conditions after 3 days of practice during which they worked under conditions VI, I, and IV. The method used to control practice effects and differences in the difficulty of the lists is illustrated in Table II. The lists used during Cycle I with one group of 12 *Ss* were used during Cycle II with the second group of 12 *Ss*, thus permitting intercycle comparisons. It will also be noted that the odd-numbered lists were always used as lists for original learning and the even-numbered lists were always used for interpolated learning.

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[8] We are greatly indebted to the Department of Psychology at Yale University and to the National Youth Administration at the University of Missouri for the funds used to pay these *Ss*.

Luh's rules,[9] for the construction of lists of syllables, could not be used in constructing 18-syllable lists. The following rules were substituted.

(1) All syllables must have association-values of less than 53.33%, as determined by Glaze.[10] The average association-values of the completed lists ranged between 24.5% and 29.3%.

(2) The letter *Y* was used neither as a vowel nor as a consonant.

(3) No vowel or consonant was repeated in any 4 consecutive syllables, and any letter used in the first syllable of a list was not used in the last syllable of the same list.

(4) Not more than 3 consonants occur twice as initial letters in syllables within the same list, and not more than 3 consonants occur twice as final letters. No consonant occurs more than 2 times as an initial letter, nor more than 2 times as a final letter, although a consonant may appear 4 times within the list.

(5) No 2 syllables within a list have 2 letters in common.

(6) The odd-numbered and even-numbered lists of a pair which is to be used on one experimental day, contain a minimal number of repetitions of 2 letters in the syllables in the same serial position.

The lists given in Table III were the ones used in the experiment, and the numbers are the ones referred to in Table II. On the memory drum the first unit of each list was preceded by the printed symbol, O-O-O, which served as the cue for the at-attempt to anticipate the first unit.

The inadequacy of the counterbalancing procedure for the control of practice effects in studies of RI and PI will be stressed in several connections during the presentation of the data. In the tables of data to be presented later there is ample evidence that the *Ss* were becoming more efficient in the learning of nonsense syllables during the first cycle of the experiment even though the 3 days of preliminary practice probably decreased this intra-cycle practice effect. When there is a residual practice effect from day to day in an experiment on RI or PI, it cannot be assumed that the level of practice of the *Ss* under the rest and work conditions has been equalized by the systematic rotation of the experimental conditions within the cycle. The reason for this is that relearning after the learning of an interpolated list must of necessity benefit from the fact that the *Ss* have had practice on 1 more list than under a rest condition in which no interpolated learning occurred. Similarly, in comparisons of learning with and without prior learning on the same day, such as those involved in determinations of PI, the measurements which the *E* expects to reveal negative transfer are always obtained when the *Ss* have reached a higher level of habituation to the task. In view of this, it may be maintained that experiments on RI and PI should be made only after the complete habituation of the *Ss* to the learning task if the specific inhibition is to be obtained without dilution by general positive transfer effects.

This dictum cannot, however, be applied without reservations because there is some evidence that susceptibility to RI decreases with repeated exposure of the *Ss* to such influences.[11] The investigator is, therefore, caught in a dilemma. If he studies RI with relatively naïve *Ss* he cannot equalize the general practice of the

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[9] C. W. Luh, The conditions of retention, *Psychol. Monog.*, 31, 1922, (no. 142), 1-87.

[10] J. A. Glaze, The association value of nonsense syllables, *Ped Sem.*, 35, 1928, 255-269.

[11] Cf. *e.g.* McGeoch, *op. cit.*, 701, and The influence of degree of learning upon retroactive inhibition, this JOURNAL, 41, 1929, 252-262.

Ss before the relearning under the rest and work conditions; if he uses *Ss* who have been thoroughly habituated, the *Ss* may have learned to avoid the inhibitory effect. The only solution the systematist can reach from these considerations is that rigid standardization of these basic conditions among various experiments must be made if integration of the results of different experiments is to be effected.

In our experiment, the data from Cycle II show, with few exceptions, greater absolute and relative amounts of RI than the data from Cycle I. Since the control of general practice effects in Cycle I may be considered inadequate, for the reasons cited above, the data from Cycle II will be given the greatest weight in our interpretations.

LEARNING OF ORIGINAL AND INTERPOLATED LISTS

In this study the degree of learning of the original and interpolated lists has been controlled in terms of frequency of repetition under conditions of motivation known to produce learning, and since the total time per repetition was constant, the total time spent in learning was likewise subject to direct control. It is important, however, to determine whether this method of control produced a constant frequency of correct anticipations in the case of the original learning. In Cycle I the mean correct anticipations during the 5 trials or original learning were 16.4 ±1.6, 16.8 ±1.4, 16.4 ±1.3, 16.3 ±1.1, and 15.0 ±1.6 for Conditions I through V, respectively, and in Cycle II the corresponding means were 21.3 ±2.0, 20.6 ±2.0, 21.9 ±1.6, 20.0 ±1.5, and 21.5 ±1.3. The largest difference in Cycle I was 1.8 ±1.2, and the largest difference in Cycle II was 1.9 ±1.4.[12] The variations in the mean correct anticipations on the fifth learning trial were also insignificant. In Cycle I these means were 5.8 ±0.6, 6.3 ±0.5, 5.7 ±0.6, 6.0 ±0.4, and 5.8 ±0.6 for Conditions I through V, respectively, and in Cycle II the corresponding means were 7.6 ±0.6, 7.9 ±0.8, 7.7 ±0.6, 7.3 ±0.5, and 8.0 ±0.5.

As would be expected, the correct anticipations were not evenly distributed over the whole original lists. Thus, when the data from Conditions I through V were combined, the mean number of syllables correctly anticipated 0, 1, 2, 3, or 4 times was 11.1, 2.0, 1.8, 1.6, and 1.6, and 1.5 during Cycle I, and 9.3, 2.4, 2.2, 2.0, 2.1 during Cycle II. In short, more than 50% of the syllables in the original lists were not correctly anticipated at any time during original learning.

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[12] All measures of reliability are sigma. All sigma of the mean differences have been corrected for the correlation between the arrays. For this experimental design there are 23 degrees of freedom, and a *t* value of 2.807 is taken as an indication of a very significant mean difference. Cf. R. A. Fisher, *Statistical Methods for Research Workers*, 1934 (5th ed.), 158.

In Table IV we have presented 3 indices which serve to reveal the level of mastery of the interpolated lists after 5, 10, 20, and 40 trials. It should be noted that 5 and 10 repetitions of these lists produced only partial learning for all but 2 *Ss*; that almost all of the syllables were correctly anticipated at least once by every *S* when the lists were learned for 20 trials, and that 40 trials produced a high degree of learning in all *Ss*.

The most significant results in Table IV, are, however, those that

TABLE III

Lists of Nonsense Syllables of Low Association Value

(The average association value of the syllables in each list, as computed from Glaze's values, is given in parentheses.)

1	2	3	4	5	6	7	8
(25.2)	(26.3)	(26.7)	(26.3)	(26.7)	(24.5)	(26.7)	(25.9)
PUK	GOX	VAW	MOF	NAF	MEC	XAR	ZIN
ZAC	JEP	DUF	KEB	VUK	GIX	KEH	CUW
NOL	VID	GEJ	GIZ	PEQ	TUD	BIW	QOD
XIF	FUB	JOH	NAL	BIH	NOJ	GOJ	KAB
BUJ	ZAH	RIX	HUQ	DAJ	WEF	VUC	SIF
QAP	TEG	ZUB	FEP	RUZ	QIB	MAF	MEP
WEZ	PIW	MAQ	GOC	NIW	KUG	XIN	XUG
CIH	KUD	TOC	BIV	HEG	VOP	PEZ	JAL
VUT	FOJ	SIJ	XAZ	MOJ	ZEL	WOS	FOH
DOQ	XEC	RUW	LEQ	DUP	WIJ	DUH	WEQ
KAM	NIQ	FAZ	WOH	KIF	XAT	KIQ	VUN
LEH	TUV	QET	JIK	XEB	CUQ	GEB	DAC
GIW	BOP	GIH	ZUP	QAR	GED	NAW	XOB
ZOB	CAX	WOJ	CEX	ZOD	ZIF	XUM	QIH
DAF	ZEQ	XUV	QAB	JUH	XOH	JIV	ZUM
XUQ	KIG	KEC	MIW	WEM	QAS	ZOF	GAW
CEJ	WUH	QIF	TOZ	XIC	TEJ	CEQ	VOC
ZIL	PAF	ZON	VUX	QOV	BUV	WUK	JEX

9	10	11	12	13	14	15	16
(26.7)	(25.6)	(27.0)	(29.3)	(26.7)	(26.3)	(26.7)	(26.7)
JOD	NIH	XAL	KEZ	BUP	FAP	KOJ	ZEC
QIM	FUP	WIK	QUB	FAJ	TIJ	BEW	JUW
PEX	VAD	HUZ	VOH	KEV	QOC	LAH	VIB
RUC	ROJ	VEC	JIW	DIH	DUZ	VIG	KOG
KAJ	GEW	GOQ	NAX	QOS	HEB	ZUR	ZAL
ZIT	QUC	TAJ	KUF	XUW	XIV	MEF	PEM
SEH	KAL	BIP	CEG	JEG	GUF	QOL	FIW
XUB	TIV	XES	ZIQ	NIV	ZOP	TIH	COJ
VOF	HEP	CUG	WOB	KOR	CEK	XAP	NUX
LIW	NUB	JOM	HUJ	ZAB	NAQ	JEC	HAQ
DEQ	CAZ	QID	LEX	MUQ	SIH	GUD	TIZ
ZAM	DIQ	TEF	PAQ	LIJ	PUW	NIZ	JEK
NUK	KEX	RUK	TOF	CEF	XOJ	TAW	BOF
GOW	VUH	ZAP	RIH	VAP	FEQ	VOB	XAC
JIC	ZOW	GIJ	SEJ	ZUH	GID	SUQ	PID
QUS	QAJ	XOF	WUP	WIB	KAH	KED	GUQ
REZ	GIC	MUW	KOC	XOL	MUX	XIM	VEH
TAH	XUL	NEQ	GAF	KAG	ZER	ZAV	MOX

TABLE III *Continued*

17 (27.0)	18 (27.4)	19 (26.7)	20 (26.7)	21 (25.6)	22 (26.7)	23 (26.7)	24 (28.1)
MUN	QAM	GOK	VAQ	XIP	NEF	NID	CUH
VOJ	VEB	RUX	KEM	LUB	TOV	ZUC	BAJ
HES	JUC	JEZ	POH	WAH	LIH	PEJ	GEV
QAT	XOR	TIS	BIX	QEM	BAZ	XOT	KIX
RIW	ZAN	FAQ	LUD	TOJ	QUN	QAV	WOQ
MOG	TIW	XOM	ZEG	CIZ	GEK	MIF	PAH
BEP	CES	DEH	WAP	RUV	DIW	CEB	LEB
ZUF	FOV	KUW	NOF	KEF	XOP	WUS	VUM
VIQ	HAJ	FIP	DIJ	XOD	TUZ	HOZ	NIR
TOH	GUK	LOJ	SEB	ZAT	SAJ	LIX	QAF
DAK	ZED	GAX	QUZ	NUW	MIG	VEF	ZOL
GEX	MIP	HUC	VOM	MIV	LOQ	QUP	XET
PUV	XAW	DIB	XAG	FOQ	XUR	SOJ	VIH
CIJ	KOB	LER	NEH	GAJ	BEJ	KIW	QUG
ZAS	QEN	ZOT	ZIR	CEH	CIK	NAC	POB
QOB	JID	VUQ	QOW	SIW	GAQ	XUH	MEJ
WUG	VAF	JIH	LAJ	ZUK	ZOH	MEQ	DAX
KER	TUH	XEW	FUH	DAQ	WUB	WOG	ZIW

reveal the proactive inhibition of the interpolated learning by the prior original learning. Whereas, 5 trials of original learning produced 16.4 correct anticipations during Cycle I and 21.3 during Cycle II, 5 trials of interpolated learning produced only 12.3 and 16.5 correct anticipations. If we compare the mean correct anticipations in each of the first 10 trials of continuous learning of a single list under Condition VI with the same measures for interpolated learning (Conditions II, IV, V), the PI effect of 5 trials of original learning is shown to be transitory. The essential data are presented in Table V. In Cycle I the PI decreased steadily until the sixth trial, and was followed by an apparent facilitation effect; in Cycle II the PI decreased steadily until the seventh trial, and was not followed by a facilitation effect. No significance other than methodological should be attached to the facilitation effect in Cycle I, because it probably reflects merely a net difference in the general practice effect

TABLE IV

Indices of the Degree of Learning of the Interpolated Lists

	Cycle	Interpolated frequencies			
		5	10	20	40
Mean total correct anticipations	I	12.3	57.1	192.0	508.8
	II	16.5	71.9	219.0	557.8
Mean syllables correctly anticipated one or more times	I	6.0	13.2	17.0	18.0
	II	7.5	14.7	17.7	18.0
Number of Ss achieving one perfect trial	I	0	0	11	21
	II	0	2	17	24

resulting from the unavoidable difference in the number of lists learned by the *S*s before learning an original and an interpolated list. When the *S*s are thoroughly habituated to the learning task, as in Cycle II, the facilitation effect does not appear. On the other hand, the significance of the differences in the early trials that we have attributed to the PI cannot be questioned. In Cycle I the ratios of the differences to their standard errors in the case of trials 2 and 3 are 2.79 and 3.87, respectively, and the corresponding ratios in Cycle II are 2.54 and 3.64. None of the other differences is significant. In a later section, evidence for overt proactive transfer will be presented.

TABLE V

MEAN CORRECT ANTICIPATIONS ON TRIALS 2 THROUGH 10 WITHOUT PRIOR LEARNING OF ANOTHER LIST AND AFTER 5 TRIALS ON ANOTHER LIST

Cycle	Condition of learning	Successive trials during learning								
		2	3	4	5	6	7	8	9	10
I	Without prior	1.92	3.54	4.67	5.96	6.92	7.46	8.08	8.88	9.83
	With prior	1.11	2.38	4.08	5.72	6.78	7.97	8.90	9.70	10.63
	% PI	−42.2	−32.8	−12.6	−4.0	−2.0	+6.8	+10.1	+9.2	+8.1
II	Without prior	2.13	4.38	5.54	7.38	8.79	9.58	10.96	11.46	12.50
	With prior	1.42	3.18	4.68	6.64	8.50	9.54	10.83	11.82	12.48
	% PI	−33.3	−27.4	−15.5	−10.0	−3.3	−0.4	−1.2	+3.1	−0.2

INTERPOLATED LEARNING AND RETROACTIVE INHIBITION

As previously indicated, in a study of serial learning by the anticipation method, McGeoch was unable to show a reliable decrease in the amount of RI when the interpolated material was greatly overlearned.[13] There was a suggestive decrease in the number of trials, but this was not paralleled by the recall scores (the number of correct anticipations on the first relearning trial). Our results verify and reconcile these apparently contradictory results with recall and relearning scores, and at the same time substantiate unequivocally the hypothesis that overlearning of the interpolated material tends to reduce the interference with the recall and relearning of the original material.

In Table VI are shown the mean trials required by the *S*s to complete the mastery of the original list to criteria of 1 errorless trial and 2 successive errorless trials. The criterial trial or trials have not been included. Our primary concern is the comparison of the trials to relearn under the work and rest conditions, and the percentages of inhibition under work conditions have been computed by the formula 100 (Rest—Work)/Rest. In both cycles of the experiment the maximal interference occurred when the interpolated list was learned for 10 or 20 trials and the minimal interference occurred when the interpolated list was learned for 40 trials. The only differences between the means which are at least twice their sigma, after correction for the correlation between the arrays,[14] are found in the relearning to 1 errorless trial in Cycle II. In this case the ratios of the differences between the rest condition and the 4 work conditions and their respective sigma are 1.57, 2.62, 3.43, and 0.20. Of greatest importance, however, is the fact that the difference between the means of the relearning trials after 20 interpolated trials and after 40 interpolated trials is 2.60 times its sigma.

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[13] *Op. cit.*, this JOURNAL, 44, 1932, 695-708.

Although this evidence for a decrease in the RI with an increase in the degree of interpolated learning is not of unquestionable reliability, the close approach in the case of relearning scores is made more significant by the evidence for the transitoriness of the RI during relearning under all work conditions. It will be noted in Table VI that the retroactive inhibition is consistently less when measurement is in terms of the trials required to relearn to 2 successive errorless trials—the more stringent criterion.

TABLE VI

DEGREE OF INTERPOLATED LEARNING AND RETROACTIVE INHIBITION AS MEASURED BY TRIALS TO RELEARN TO 1 PERFECT AND 2 SUCCESSIVE PERFECT TRIALS

	Condition	Continuous	Rest	Trials on interpolated list			
				5	10	20	40
Trials to 1 Perfect	RL Trials, Cycle I	16.2±1.7	19.5±1.9	19.8±1.9	21.0±1.7	19.3±2.0	18.6±1.8
	RL Trials, Cycle II	13.0±1.5	12.5±1.3	13.6±1.5	14.6±1.2	14.9±1.3	12.3±1.2
	% RI, Cycle I			1.5	7.7	+1.0	+4.6
	% RI, Cycle II			8.8	16.8	19.2	+1.6
Trials to 2 Perfect	RL Trials, Cycle I	23.6±2.4	25.8±2.7	25.5±2.8	27.6±2.7	25.4±3.2	24.4±2.7
	RL Trials, Cycle II	16.3±2.0	18.0±2.1	17.4±1.8	18.1±1.7	18.5±1.9	16.5±2.0
	% RI, Cycle I			+1.2	7.0	+1.6	+5.4
	% RI, Cycle II			+3.3	0.6	2.8	+8.3

The characterization of RI as a transitory phenomenon is given further substantiation by the recall scores. As shown by a comparison of Table VII with Table VI, the percentages of RI as measured by recall are many times greater than the percentages of RI as measured by relearning scores.[15] But there is an added complication. The evidence for a reduction in the amount of RI as the interpolated trials were increased from 20 to 40 is not nearly so clear-cut in the case of the recall scores as it was in the case of the relearning scores. There is a slight increase in the number of correct anticipations at 40 trials, which is more pronounced in Cycle II than in Cycle I, but these differences in Cycles I and II are only 0.59 and 1.88 times their σ and are insignificant in comparison with the increases in the RI as the number of interpolated trials was increased from 5 to 20. In short, our data conform with those of McGeoch in that the tendency for a decrease in RI with overlearning of the interpolated material is more pronounced in the case of relearning scores than in the case of recall scores, even though our data are more consistent than McGeoch's in showing such a decrease when either measure is used.

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[14] The product-moment *r*s between the trials to relearn to 1 errorless trial under the rest and work conditions averaged 0.668 in Cycle I and 0.742 in Cycle II. The corresponding average *r*s for trials to relearn to 2 successive errorless trials were 0.647 and 0.653. These are averages of the 10 possible intercorrelations of 5 conditions. There was no systematic variation in the *r*s as the degree of interpolated learning increased.

[15] It should be noted that McGeoch (*op. cit.*, 698-699), obtained percentages of RI in relearning which were nearly as large as his percentages of RI in recall. Although the present percentages of RI in relearning have been computed directly

TABLE VII

DEGREE OF INTERPOLATED LEARNING AND RETROACTIVE INHIBITION AS MEASURED BY RECALL

Condition	Continuous	Rest	Trials on interpolated list			
			5	10	20	40
Recall, Cycle I	6.92±.59	3.50±.56	1.50±.27	0.71±.15	0.50±.18	0.67±.24
Recall, Cycle II	8.79±.77	5.58±.69	2.04±.36	1.33±.38	0.92±.25	1.54±.30
% RI, Cycle I			57.1	79.7	85.7	80.9
% RI, Cycle II			63.4	76.2	83.5	72.4

These apparently divergent relationships for recall and relearning scores may, however, be made intelligible if it is assumed that the transitoriness of the RI varies with the degree of learning of the interpolated list, being more transitory—of less duration in the relearning process and, therefore, less likely to affect the relearning scores—the greater the degree of interpolated learning. This assumption has been verified, in the first instance, by determining the average number of correct anticipations (an inverse measure of RI when compared with performance after rest) on the first, second, third, fourth, etc., relearning trials under the various work conditions. As shown in Fig. 1, the tendency for the amount of RI to increase and then decrease as the degree of interpolated learning is increased becomes more pronounced in successive relearning trials after the first (recall) trial. Apparently, the RI disappears most rapidly when 40 interpolated trials are given, and next most rapidly with 20 interpolated trials. In Cycle II the ratios of the difference between the 10- and 40-trial work conditions in relearning trials 2, 3, and 4 to their respective sigma were

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from relearning trials, rather than from saving scores,' this fact is not responsible for the departure from McGeoch's results. Several points in the theory of RI presented at the end of this paper suggest that this difference occurred because McGeoch used a higher degree of original learning in his study.

2.45, 2.91, and 2.34. In Cycle I the corresponding ratios were 1.94, 1.22, and 1.31.

Analysis of the transitoriness of the RI in terms of mean correct anticipations during any one relearning trial must be limited to a few trials because some *S*s achieve the criterion of mastery after only a few trials. A modification of the Vincent-Kjerstad technique such as employed by Melton in studies of the form of the learning curve,[16] may be used to study the entire relearning period of each *S*. Thus, for the rest and work conditions we have computed the mean relearning trials required by the *S*s before they could correctly anticipate 1, 2, 3, 4 . . . 18 units within a single trial. Only the results for the 10- and 40-trial work conditions and the rest condition have been shown in Fig. 2, since the curves for the 5- and 20-trial work conditions follow closely the curve for the 10-trial work

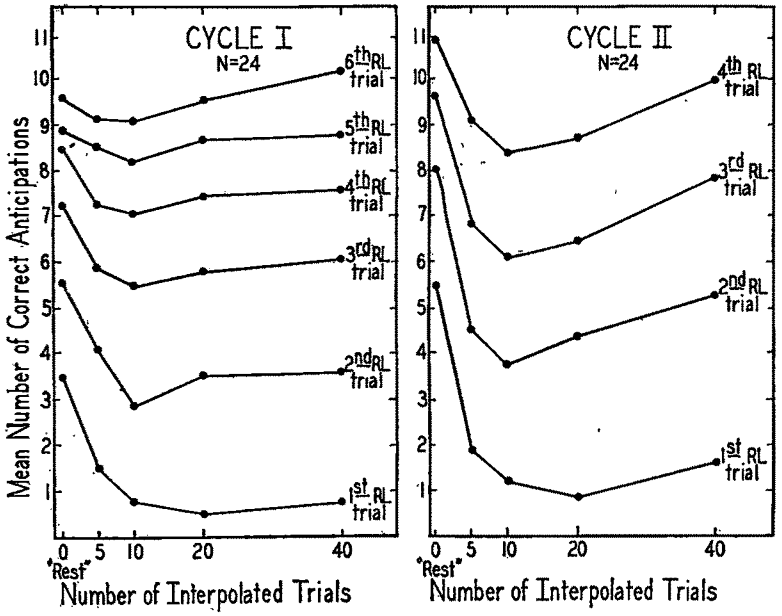

Fig. 1. Relationship Between the Mean Number of Correct Anticipations in Successive Relearning Trials and the Degree of Learning of the Interpolated Material

The series of curves shows the more rapid disappearance of the RI with 40 interpolated trials than with 10 or 20 interpolated trials.

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[16] A. W. Melton, The end-spurt in memorization curves as an artifact of the averaging of individual curves, *Psychol. Monog.*, 47, 1936, (no. 212), 119-133.

conditions. It is apparent that the RI persists throughout the relearning of the original list after 10 interpolated trials, in the sense that there is greater difficulty in relearning to every criterion of partial mastery. On the other hand, with 40 interpolated trials the *Ss* show greater difficulty in relearning only until they have reached a criterion of 12 or 13 correct anticipations out of the 18. From that point on to the attainment of the criterion of 1 errorless trial there is no difference between this work condition and the rest condition. That the ease of mastering the last part of the list is apparently greater under the 40-trial work condition than under the rest condition in Cycle I, must again be discounted because it probably reflects the general practice effect from the learning of the interpolated list.

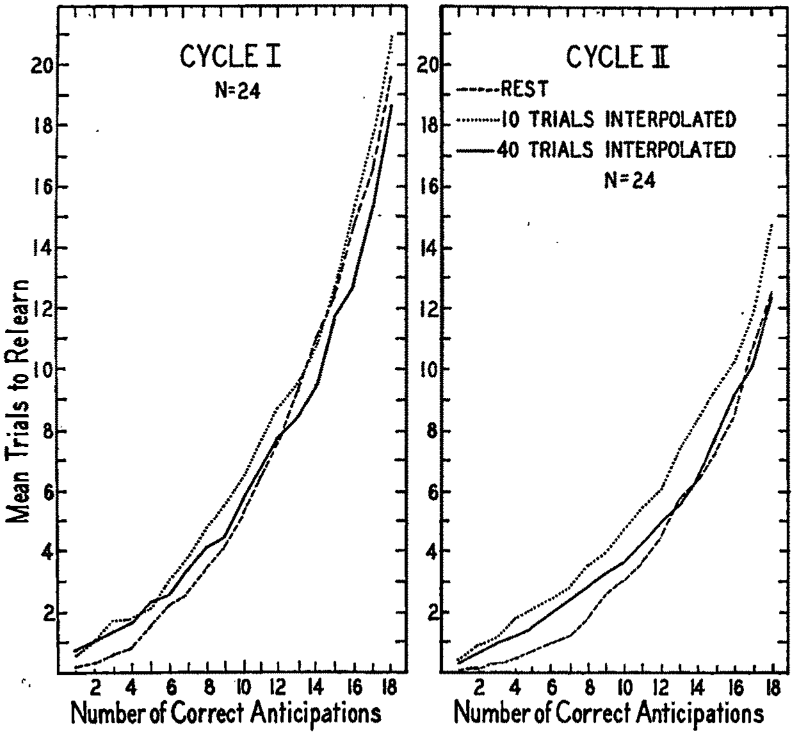

FIG. 2. MEAN TRIALS TO RELEARN THE ORIGINAL LIST TO VARIOUS LEVELS OF MASTERY

The RI after 40 interpolated trials is shown to be transitory.

Integration of the various lines of evidence which have been presented unquestionably supports the hypothesis that the maximal inhibition of an original learned activity occurs when the interpolated activity is learned to a moderate degree. Very low or very high degrees of learning of the interpolated material give the smallest amounts of RI. The apparent discrepancy between the relationships obtained when the RI is measured by recall scores and when it is measured by relearning scores is to be attributed partly to a general tendency for the RI to disappear as the relearning of the original material proceeds toward complete mastery, and partly to the more rapid disappearance of the RI during relearning when the degree of interpolated learning is high. A decrease in the RI with overlearning of the interpolated material may not be evident in the first relearning trial but will be clearly shown in the second or third relearning trial because the RI disappears so much more rapidly with the overlearned material than it does with moderately learned interpolated material.

SPECIFIC INTER-LIST INTERFERENCE EFFECTS

Of theoretical importance in studies of RI is the analysis of evidence of active competition between the original and interpolated response systems during the learning of the interpolated material and, particularly, during the recall and relearning of the original material. For, if a syllable from the interpolated list occurs as a response during the attempt to recall and relearn the original list, there is *prima facie* evidence that active competition of the two response systems is occurring and this behavioral fact must be considered in explanations of RI. McKinney and McGeoch have recently reported such intrusions of interpolated word responses during the written recall of an original list of words,[17] and a corresponding but less pronounced tendency for words learned in an original list to intrude in the immediate written recall of interpolated words. No attempt, however, has heretofore been made to observe such intrusions during learning and relearning by the anticipation method, nor to discover the relationship between the frequency and conditions of occurrence of such intrusions and the degree of learning of the interpolated material.

In order to make such data available in this experiment, every incorrect response made by the *Ss* during the original learning, interpolated learning and relearning of the original material was recorded. All records were then searched to locate those instances in which an entire interpolated syllable was spelled out during the relearning of the original list and in which a syllable from the original list was spelled out during the learning of the interpolated list. Since some syllables which were identified as intrusions might have occurred as errors even though no interpolated (or original) list had been learned, 'control' tabulations were necessary. The control for the intrusions of interpolated syllables during the relearning of the original list was obtained by noting the number of 'intrusions' during relearning after rest (Condition I). In this case, an 'intrusion' syllable was one that appeared in the list which would have been interpolated if a work condition had been scheduled for that day. The control for the intrusions of original syllables during interpolated learning was obtained by noting the number of 'intrusions' during the continuous learning of a list without prior learning (Condition VI). In this case, an 'intrusion' syllable was one that appeared in the list which would have been given as original learning if such original learning had been scheduled. The arrangement of the schedule of lists and conditions which permits these control measurements has been illustrated in Table II.

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[17] Fred McKinney and J. A. McGeoch, The character and extent of transfer in retroactive inhibition: Disparate serial lists, this JOURNAL, 47, 1935, 409-423.

The inter-list intrusions were classified according to the apparent source of the confusion. When a syllable in serial position n in an interpolated list displaced a syllable in position n or $n \pm 1$ in an original list, and neither the displaced syllable nor the syllables in adjoining serial positions had 2 letters in common with the intruding syllable, the intrusion was said to be based on identity of serial positions. When a syllable in serial position n in an interpolated list displaced a syllable in a position farther removed than $n \pm 1$ in the original list, and the displaced syllable or the syllable in the adjoining serial positions had 2 letters in common with the intruding syllable, the intrusion was said to be based on formal similarity of the materials learned. When a syllable in position n in an interpolated list displaced a syllable in position n or $n \pm 1$ in an original list, and the displaced syllable or a syllable in an adjoining serial position had 2 letters in common with the intruding syllable, the intrusion was said to be based on both identity of serial position and formal similarity. A fourth class includes all those intrusions which failed to satisfy the criteria of the first three classes.[18]

A further problem arises in the tabulation of the frequency of intrusions, since the same intrusion syllable is sometimes repeated several times in succession during a single trial, and the same intrusion syllable may be repeated during several successive trials or intermittently throughout the relearning process. Although the very occurrence of such intra-trial and inter-trial repetitions is significant, our data are too few to permit their adequate treatment, and we have chosen the number of *different* intrusion syllables as the most stable and significant measure for our present purposes. In all the detailed analyses to follow, the *first* occurrence of an intrusion syllable during the relearning process determines the serial position of the displaced syllable and the trial during which the intrusion is said to occur.

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[18] This analysis of inter-list intrusions is obviously crude, because the pairs of lists used in the RI experiment must be constructed according to special rules if the factors of serial position, formal similarity, and meaningful similarity are to be isolated. Such pairs of lists are being used in studies now in progress.

The data on the frequency of overt intrusions of interpolated syllables during the relearning of the original lists are presented in Table VIII.[19]

TABLE VIII

FREQUENCY OF OVERT INTRUSIONS OF INTERPOLATED SYLLABLES DURING THE RELEARNING OF THE ORIGINAL SYLLABLES
(Both cycles combined)

Intrusions	Trials on interpolated lists				
	Rest	5	10	20	40
Total	9	65	92	53	58
Different syllables	6	45	64	37	43
Based on ser. pos.	0	22(48.9%)	39(60.9%)	18(48.7%)	11(25.6%)
Based on similarity	3	8(17.8%)	14(21.9%)	10(27.0%)	24(55.8%)
Based on ser. pos. and similarity	1	4	2	2	4
Unclassified	2	11	9	7	4

Of primary importance is the fact that relatively few 'intrusions' occur during the relearning after rest; whereas, many intrusions occur during relearning after any amount of interpolated learning. Furthermore, the frequency with which the intrusions occur during relearning after various amounts of interpolated learning roughly parallels our findings regarding RI as measured by recall and relearning scores. The maximal number of intrusions occurs when the interpolated list is learned for only 10 trials, and the number of intrusions after 5 interpolated trials and 40 interpolated trials is approximately the same. Since there was no significant change either in the number of different *S*s showing intrusions under the 4 RI conditions, nor in the number of different relearning records (N = 48) showing intrusions, it may be concluded that the change in the frequency of intrusions as a function of the degree of interpolated learning was not the result of a selective sampling effect. The maximal total number of intrusions given by any *S* during the 8 relearning periods after interpolated learning was 17, the minimal number was 2, and the mean and S.D. were 7.9 and 4.2, respectively. The median number was 7.5 intrusions. Such intrusions are clearly a part of the normal process of relearning an original list after the learning of an interpolated list, under the conditions of this experiment.

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[19] The data of both cycles of the experiment have been combined in making this analysis of intrusions. There is, however, an interesting shift in the frequency of intrusions with practice which will have considerable significance if it is verified by later experiments. During Cycle I the total number of different intrusion syllables during relearning after 5, 10, 20, and 40 interpolated trials was 18, 28, 20, and 25. During Cycle II the corresponding frequencies were 27, 36, 17, and 18. When repeated intrusion syllables were counted, the frequencies for Cycle I were 23, 38, 31, and 32, and the frequencies for Cycle II were 42, 54, 22, and 26. Thus, the number of intrusions during relearning increased with practice when there was a low degree of interpolated learning and decreased with practice when there was a high degree of interpolated learning. The frequencies of intrusions during the first relearning trial failed to show this relationship to practice; instead, the frequencies increased with practice under all RI conditions. Thus, in Cycle I there were 10, 15, 3, and 0 different intrusion syllables in the first relearning trial, and in Cycle II there were 18, 17, 8, and 3 such intrusions. When repeated intrusion syllables were counted, these frequencies were 12, 18, 4, and 0 in Cycle I, and 24, 20, 10, and 3 in Cycle II.

Two sources of the confusion which leads to such intrusions are likewise clearly indicated by the data in Table VIII. Intrusions based on identity of serial position, as previously defined, increased in frequency absolutely and relatively as the trials on the interpolated list were increased from 5 to 10, and declined precipitously with further increases in the degree of learning of the interpolated list. On the other hand, the intrusions based on formal similarity of the syllables increased as the degree of interpolated learning was increased. These diverse relationships between the frequency of intrusions and the degree of interpolated learning suggest that the specific relationship between RI and degree of interpolated learning, as revealed by the analysis of recall and relearning scores in the previous section, would have been materially altered if there had been greater or less formal inter-list similarity of the syllables learned. The obtained relationship seems to have been a consequence of the dominance of serial position factors in determining the interference.

Another finding relative to these intrusions must, however, be considered in the formation of such an hypothesis. As shown in Table IX, the locus of the competition between the original and interpolated responses occurs later and later in the relearning process the greater the degree of learning of the interpolated syllables. Three different, though correlated, indices show this effect. As the degree of interpolation increased, the proportion of all the different intrusion syllables occurring on the first relearning (recall) trial decreased from 62.2% to 7.0%,[20] the mean relearning trial during which these intrusions occurred increased from 2.0 to 9.4, and the mean frequency with which the displaced syllables in the original list had been correctly anticipated before their displacement increased from 1.2 to 4.3. These relationships for all intrusions are closely paralleled by those for the intrusions based on identity of serial position, but the relationships in the case of intrusions based on similarity are extremely irregular. Nevertheless, it is clear that for every

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[20] In the later theoretical interpretation of these results, an important datum is the absolute amount of RI on the first relearning trial which is attributable to overt intrusions. If it is assumed, following McKinney and McGeoch, *op. cit.*, 420-421,

TABLE IX

Locus of Intrusions During the Relearning Process

	Rest	Trials on interpolated lists			
		5	10	20	40
Number of intrusions					
different, on the first relearning trial	0	28(62.2%)	32(50.0%)	11(29.7%)	3(7.0%)
based on serial position during the first relearning trial	0	17(77.3%)	25(64.1%)	7(38.9%)	1(9.1%)
based on similarity during the first relearning trial	0	2(25.0%)	0	0	2(8.3%)
based on serial position and similarity	0	1(25.0%)	0	0	0
unclassified	0	8(72.7%)	7(77.8%)	4(57.1%)	0
Mean trial of occurrence					
of all intrusions	16.7	2.0	4.7	7.3	9.4
of those based on ser. pos.		1.4	1.9	6.9	12.0
of those based on similarity		3.4	12.7	10.4	8.6
Mean correct anticipations					
of all displaced syllables	8.8	1.2	2.9	2.2	4.3
of those displaced on basis of ser. pos.		1.8	2.4	4.2	8.8
of those displaced on basis of similarity		0.8	6.4	2.9	3.7

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that a *correct response* has been blocked by each *different* overt intrusion in the first relearning trial, then the average absolute RI attributable to such intrusions is 0.58, 0.67, 0.23, and 0.06 syllables with 5-, 10-, 20-, and 40-trial interpolations. Since the average total RI in both cycles of the experiment was 2.77, 3.52, 3.83, and 3.44 (Rest — Work) for these RI conditions, the percentages of the obtained RI attributable to different overt intrusions were 20.9, 19.0, 6.0, and 1.8. If, in addition, it is assumed that repeated intrusions block a different correct response each time they occur, these average absolute RI values become 0.75, 0.79, 0.29, and 0.06, and the percentages of the total RI attributable to overt intrusions become 27.1, 22.4, 7.6, and 1.8. These values do not, of course, represent the total amount of RI attributable to overt intrusions, because they refer only to the intrusion of *whole* syllables. Attempts to analyze the data of this experiment for partial intrusions, such as would be involved in the displacement of single letters, have been unsuccessful.

degree of interpolated learning except the highest, the intrusions based on similarity come later in the relearning than the intrusions based on serial position. These relationships serve to emphasize, therefore, another important determiner of the specific relationship between the amount of RI and degree of interpolated learning found in this study; namely, the degree of learning of the original list. In fact, these data warrant the hypothesis that the maximal amount of interference occurs when there is a certain ratio of the degrees of learning of the original and interpolated materials.

This hypothesis cannot be elaborated properly without consideration of the frequency of intrusions of original syllables during the interpolated learning. As shown in Table X, the frequency of such intrusions of whole syllables during the first 5 trials was greater under the conditions which involved the prior learning of an original list for 5 trials.[21] For example, 8 such 'intrusions' occurred during the first 5 trials of interpolated learning when there had been no prior learning: whereas, there were 14, 15, 11, and 19 intrusions when the learning followed 5 trials on an original list. All 8 of the 'intrusions' occurring in the control condition were based on the formal similarity of the syllables, and may be attributed

TABLE X

THE FREQUENCY OF OVERT INTRUSIONS OF SYLLABLES FROM THE ORIGINAL LISTS DURING THE LEARNING OF THE INTERPOLATED LISTS
(Both cycles combined)

Experimental condition	Trial during which intrusion occurred				Total intrusions
	1–5	6–10	11–20	21–40	
Continuous learning without prior learning: control (Condition VI)	8	3	7	1	19
5 trials on interpolated list after 5 original trials (Condition II)	14				14
10 trials on interpolated list after 5 original trials (Condition III)	15	9			24
20 trials on interpolated list after 5 original trials (Condition IV)	11	5	0		16
40 trials on interpolated list after 5 original trials (Condition V)	19	5	1	2	27

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[21] McKinney and McGeoch, *op. cit.*, 413, found that intrusions occurred more often in the delayed recall of the original list than in the immediate recall of the interpolated list. In the present experiment the *S*s averaged 7.88 different intrusion syllables during relearning and 3.25 intrusion syllables during the interpolated learning. These averages include the intrusions during the 4 work conditions and the 2 cycles of the experiment. The r between the number of intrusions during relearning and the number of intrusions during the interpolated learning was 0.66 ± .12.

to normal tendencies to substitute letters or transpose letters in spelling nonsense syllables. In the 4 work conditions the number based on formal similarity was 10, 12, 7, and 17, and the number based on serial position was 4, 3, 6, and 6. Although obviously insufficient for a detailed analysis, it is significant that the frequency of these intrusions is greatest in the first 5 trials of the learning of the interpolated list. The frequency of all proactive inter-list intrusions on trials 1, 2, 3, 4, and 5 was 0, 16, 20, 13, and 10, respectively. With 5 trials on the first list, the specific inter-list interferences occur most frequently in the third trial on a second list learned after a rest of 1 min.

THEORETICAL CONCLUSIONS

From the radically empirical point of view the scientific study of retroactive inhibition may be formulated in the pseudo-equational form RI = f(a,b,c, . . . z), where RI is the amount of loss in retention of an original S-R relationship, X, resulting from the interpolated S-R relationship, Y, and a,b,c, . . . z refer to identifiable aspects or dimensions of the stimuli, the responses, the organism, or the method of measurement employed. Studies involving the systematic variation of the formal, meaningful, or process similarities of the two learning activites, of the degrees of learning of the original and interpolated activities, etc., contribute to the completion of this analysis of the conditions of RI. Clearly, this dimensional analysis is essential to the formulation and validation of a theory of RI. However, some studies contribute directly to the characterization of the behavioral correlates of the measured loss in retention. They cover such problems as the *locus* of the inhibitory processes—whether in the interpolated learning, in the recall and relearning of the original activity, or both, the *direction* of the inhibition—whether the inhibition is mutual or occurs only in the attempt to recall and relearn the original activity, and the *essential nature* of the inhibition—whether it is central or is attributable to the competition between reaction systems. Such observations are sure to play a dominant role in the formulation of a theory of RI, even though the theory must be congruent with the generalizations based on the dimensional analysis of the conditions of RI.

(a) Conditions of retroactive inhibition. It was the intent of the present experiment to hold constant all the known conditions of RI except the degree of learning of the interpolated list of nonsense syllables, in order to determine the relationship between the degree of interpolated learning and the amount of RI when (a) the original list of nonsense syllables was poorly learned; (b) the original and interpolated lists were near the minimum in formal or meaningful similarity, but the

environmental situation and the process of learning—the anticipation method—provided some similarity of the two learning activities; (c) retention of the original list was measured by the recall and relearning methods 30 min. after the end of practice. For this complex of conditions, the data favor the general principle that the amount of RI first increases and then decreases as the degree of learning of the interpolated list is progressively increased. This relationship is clearly present when the retention of the original list is measured by the number of trials required to complete the relearning of the original list to mastery. However, this statement must not be taken to imply a constancy of the relationship under varying conditions.

The form of the relationship depends in part on the measure of retention used. All measures of retention show an increase in RI with early increases in the degree of interpolated learning, but the *decrease* in RI with further increases in the degree of interpolated learning is much less apparent in the recall scores on the first relearning trial than in succeeding relearning trials and in the number of trials required to relearn to mastery. A joint appraisal of these facts leads, however, to a clarifying generalization: RI dissipates when the correct responses are repeatedly reënforced, as is the case during relearning by the anticipation method, and this rate of disappearance of the RI during relearning is at a minimum with intermediate degrees of interpolated learning and at a maximum with low and high degrees of interpolated learning. After 5 trials on an original list of nonsense syllables, the maximal interference with recall (on the first relearning trial) and the maximal persistence of the inhibition during the relearning process occurs with intermediate degrees of interpolated learning, minimal inhibition of recall and minimal persistence of the inhibition occurs with a low degree of interpolated learning, and near-maximal inhibition of recall is combined with *minimal* persistence of the inhibition during relearning when there is a very high degree of interpolated learning.

This conclusion is not at variance with results obtained by McGeoch in an earlier study in which comparable controls and measures were used, although the failure to obtain reliable differences in RI led him to conclude that the intermediate and high degrees of interpolated learning gave 'nearly' equal amounts of RI, however measured.[22] McGeoch's *Ss* learned an original list of 8 nonsense syllables for 11 trials, then learned a second list for 6, 11, 16, 21, or 26 trials, and, after 10 min., relearned the first list to 3 successive perfect trials. In the second cycle the per cents of RI, 100(Rest — Work)/Rest, with the degrees of interpolated learning listed above were 50.1, 89.1, 92.9, 81.2, and *84.4* when measured by the recall on the first relearning trial and 16.6, 50.0, 43.5, 55.0, and *30.6* when measured by the number of trials required to relearn to the stated criterion.

Although the effect of the degree of original learning and the similarity factor on the form of the obtained relationship must await further systematic investigation, suggestions regarding the nature of these interrelationships may be obtained from the data on inter-list intrusional responses in the present study. It is assumed that the inter-list intrusions are responsible, at least in part, for the measured losses in retention.

Some formal similarity of the syllables in the original and interpolated lists was necessarily present in this study, and there were a few instances in which the lists contained syllables that had 2 letters in common. In the analysis of the inter-list intrusions it was found that some intrusions were based on these formal similarities, and other intrusions were based on the identity or nearness of the serial positions of the intruding and displaced syllables. Furthermore, the frequencies of these two types of intrusions did not vary concomitantly as the degree of interpolated learning increased. The intrusions based on the identity of serial positions reached a maximal frequency when the interpolated list was learned for 10 trials; whereas, the intrusions based on syllable similarity reached a maximal frequency when the interpolated list was learned for 40 trials. It follows, therefore, that one would expect the maximal total number of intrusions (serial position + syllable similarity) to be obtained with higher degrees of interpolated learning the greater the similarity of the syllables in the two lists. That is, with the degree of original learning used in this study and a greater frequency of similar syllables in the two lists, the maximal frequency of intrusions during recall and relearning, and the maximal RI, would have been obtained with a high degree, rather than an intermediate degree, of interpolated learning. This hypothesis is admittedly crude, but it is suggestive. A precise formulation of it must, of course, take into consideration (a) the possibility, defended in a later section, that the inter-list intrusions during relearning are responsible for only a part of the measured RI, and (b) the fact that intrusions based on the serial position factor occur earlier in the relearning process than the intrusions based on syllable similarity when the degree of interpolated learning is low, and that they occur later in the relearning process than the intrusions based on syllable similarity when the degree of interpolated learning is high.

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[22] *Op. cit.*, 707.

The obtained form of the relationship between the amount of RI and the degree of interpolated learning is probably a function of the definition of 'degree of learning' and almost certainly a function of the degree of learning of the original list before the interpolation of the second list. McGeoch has surveyed adequately the problems relating to the definition of 'degree of learning,'[23] and in the light of his discussion the present set of generalizations must be made conditional upon a definition of 'degree of learning' in terms of the frequency of repetition of the nonsense syllables and the correlated frequency of correct anticipations. More than this cannot be said until the significance of the other suggested measures of degree of learning has been revealed through special methodological studies.

As for the question whether the same functional relationship between RI and the degree of interpolated learning would hold if the degree of original learning in the present study had been higher or lower, a certain conclusion must again await a direct experimental test. Nevertheless, the data on the frequency and locus of intrusions during relearning offer pertinent evidence on a closely related problem, viz. the relation between the degrees of learning of the intruding and the displaced syllables. As the degree of learning of the interpolated syllables increased there was an increase in the degree of learning of the original syllables displaced by these syllables during relearning. This was especially clear in the case of the intrusions based on identity of serial position. In fact, when the intruding syllable had been very well learned during the interpolated work period the intrusion not infrequently occurred very late in the relearning of the original list and displaced a syllable that had been correctly anticipated a number of times *during the immediately preceding relearning trials.*

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[23] *Ibid.,* 704-706.

These data suggest that the portion of the RI attributable to the competition of original and interpolated responses during recall and relearning is at a maximum when the degrees of learning of the competing responses are approximately equal. This hypothesis has received an earlier statement by Siipola and Israel as a result of their analysis of proactive inhibition in code learning and their reanalysis of the data obtained by McGeoch in serial verbal learning.[24] The only evidence against this hypothesis is the fact that the degree of learning of the syllables displaced by *similar* syllables failed to show a positive correlation with the degree of learning of the intruding syllables. Even so, the hypothesis is validated when the two types of intrusions are considered together. There is, therefore, sufficient evidence to warrant the prediction that, under the conditions of the present experiment, the degree of interpolated learning that produces maximal RI will be higher the higher the degree of learning of the original list.

There are undoubtedly other conditions in the present study which determined the obtained form of the relationship between degree of interpolated learning and RI. Such is the basic assumption in any dimensional analysis of the conditions of a phenomenon. However, none of the data of the experiment are relevant even to the formulation of hypotheses concerning these other conditions.

(b) Essential nature and locus of retroactive inhibition. As previously indicated, the dimensional analysis of the conditions of RI serves to suggest the phenomenal characteristics of the inhibition and to test the validity of hypotheses regarding those phenomenal characteristics. The most thoroughly documented theory of RI has emerged from the long series of dimensional analyses conducted by McGeoch. On the basis of these studies he has proposed an identification of the phenomena of RI and of reproductive inhibition.[25] Reproductive inhibition is defined as the decrement in the retention of an original learned stimulus-response sequence when one term of the pair has, between learning and the test of retention, been connected with another and different term. The identification of RI as reproductive inhibition squarely affirms, therefore, the dependence of RI on the identity of either the stimulus or response terms of the original and interpolated learning, and is a specific transfer theory of RI.

McGeoch's transfer theory is clearly favored by the observations of overt transfer of original and interpolated syllables in the present experiment. Similar behavioral data have been reported by McKinney and McGeoch and by Johnson,[26] but these earlier reports were restricted to observations of overt inter-list intrusions during written recalls; whereas, the use of the anticipation method in the present study permitted observations of the locus of these intrusions during the entire relearning process. Furthermore, it has been clearly shown that the frequency of such intrusions during recall and relearning is correlated with the amount of RI as traditionally measured, and that the intrusions may in most instances be traced to one of two forms of identity of the intruding and displaced syllables, namely, an identity of serial position or a partial identity of the letters in the two syllables. Such direct evidence that overt transfer of responses is the behavioral correlate of the measured RI forces acceptance of a transfer theory of RI which identifies at least a portion of the RI with reproductive inhibition.

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[24] E. M. Siipola and H. E. Israel, Habit-interference as dependent upon stage of training, this JOURNAL, 45, 1933, 205-227. The McGeoch studies are those previously referred to, this JOURNAL, 41, 1929, 252-262; 44, 1932, 695-708.

[25] Cf. J. A. McGeoch and G. O. McGeoch, Studies in retroactive inhibition. VI. The influence of relative serial positions of interpolated synonyms, *J. Exper. Psychol.,* 19, 1936, 1-23; J. A. McGeoch, Studies in retroactive inhibition. VII. Retroactive inhibition as a function of the length and frequency of presentation of the interpolated lists, *ibid.,* 19, 1936, 674-693; J. A. McGeoch and Fred McKinney, Studies in retroactive inhibition. VIII. The influence of relative order of presentation of original and interpolated paired associates, *ibid.,* 20, 1937, 60-83; and J. A. McGeoch, Fred McKinney, and H. N. Peters, Studies in retroactive inhibition. IX. Retroactive inhibition, reproductive inhibition and reminiscence, *ibid.,* 20, 1937, 131-143.

[26] Cf. McKinney and McGeoch, *op. cit.,* and L. M. Johnson, Similarity of meaning as a factor in retroactive inhibition, *J. Gen. Psychol.,* 9, 1933, 377-389.

It does not necessarily follow, however, as McGeoch and McKinney and McGeoch maintain,[27] that all the measured RI may be attributed to competition of the original and interpolated responses at the time of the attempted recall and relearning of the original material. Obviously, the theory of RI must provide an interpretation of any marked discrepancy between the total amount of RI and the amount directly attributable to overt intrusions of interpolated responses. Thus, in the present study, on the first relearning trial after 10 and 40 interpolated trials the percentages of RI were 78 and 76 (averages of both cycles); whereas, the percentages of RI attributable to the intrusions of whole interpolated syllables were 14.8 and 1.8, and the RI persisted throughout the relearning in the first case but disappeared after a few relearning trials in the second case. Some factor or factors other than the overt intrusion of specific responses must be postulated to account for these differences McKinney and McGeoch suggest that the residual RI in recall may be attributed to subliminal or implicit intrusions. Although this interpretation has the advantage that it avoids the postulation of a different form of inhibition, another interpretation is possible. It may be that there is a direct weakening of the original responses at the time the interpolated responses are being learned, and that this unlearning factor accounts for the discrepancies noted.

The residual RI which must be accounted for by postulating one or more factors other than overt specific intrusions increases in amount as the degree of interpolated learning increases. In Fig. 3 the uppermost curve shows the obtained relationship between the total absolute RI (Rest — Work) on the first learning trial and the degree of interpolated learning, and the curve labeled "Overt Competition" shows the relationship between the absolute RI attributable to overt intrusions and the degree of interpolated learning. Since only the intrusions of whole syllables were identifiable in the recall records, and there were undoubtedly many partial intrusions, *e.g.* 2- and 3-letter responses made up of parts of different interpolated syllables, the points on this curve have been fixed by multiplying the percentages of RI attributable to overt intrusions of whole syllables by 2.[28] If it is then assumed that the RI attributable to overt intrusions combines additively with the RI attributable to the unknown factor, X, the relationship between the latter factor and the degree of interpolated learning is shown in the curve labeled "Factor X." This factor was almost solely responsible for the RI in the first relearning trial after 40 interpolated trials, and this RI has been shown to be extremely transitory. Therefore, the unknown factor must be one that increases progressively as the degree of interpolated learning increases, but ceases to interfere with the recall of the original syllables after they have reënforced 3 or 4 times.

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[27] Cf. J. A. McGeoch, Studies in retroactive inhibition. I. The temporal course of the inhibitory effects of interpolated learning, *ibid.*, 9, 1933, 24-41; Studies in retroactive inhibition. II. Relationships between temporal point of interpolation, length of interval, and amount of retroactive inhibition, *ibid.*, 9, 1933, 44-55; McGeoch and McGeoch, *op. cit.;* and McKinney and McGeoch, *op. cit.*

[28] It is assumed that a correct response was blocked each time an entire syllable from the interpolated list occurred as a response during the first relearning trial on the original list. This total includes repetitions of the intrusion syllables, and is

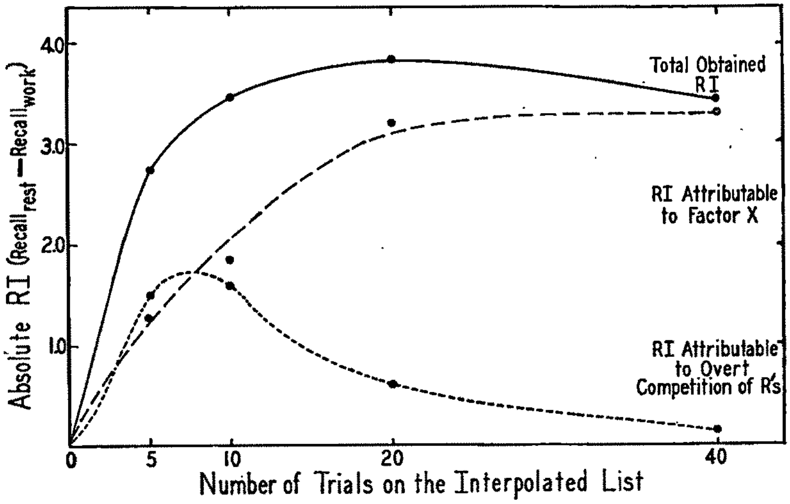

FIG. 3. RELATIONSHIP BETWEEN THE AMOUNT OF RI AND THE DEGREE OF LEARNING OF THE INTERPOLATED MATERIAL

The total obtained RI is the number of syllables forgotten as a consequence of the interpolated learning. The RI attributable to the overt competition of original and interpolated responses has been taken as 2 times the average frequency of intrusions of entire interpolated syllables during the recall trial in order to take into account the unidentified overt intrusions of parts of syllables. The curve for Factor X represents the absolute decrement in recall attributable to the factor or factors other than the overt competition of original and interpolated responses.

The primary question becomes the identification of Factor X. The theory of McGeoch and McKinney and McGeoch involves the parsimonious assumption that this factor is merely another form of competition between the original and interpolated responses at the time of recall and relearning; namely, that it is the sub-

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greater than the number of *different* intrusion syllables. See footnote 20, page 190, for the derivation of the values used in Fig 3. The use of 2 as the multiplier is arbitrary, and the form of the curve for "Factor X" is a function of the multiplier used. For example, if 4 were used as the multiplier the curve for "Factor X" would be sigmoid in form. The argument for a 2-factor theory of RI is not dependent, however, on the multiplier used; no multiplier would make the curve for the RI attributable to the competition of responses fit the curve for the obtained RI.

liminal and implicit intrusion of interpolated responses. That implicit intrusions produce some of the RI cannot be questioned. In recall by the anticipation method the *S* sometimes starts to say a syllable and then rejects it as incorrect. The identification of the tentative response as incorrect is clearly indicated by expressions of disgust and by direct statements. Since the *S* may not have time to spell out the correct syllable before the anticipation interval is over, this implicit intrusion effectively blocks the recall of the correct syllable.

Although implicit intrusions undoubtedly occur, there are at present no data regarding their frequency in recall after various amounts of practice on the interpolated responses, and such data are crucial for the evaluation of the McKinney and McGeoch hypothesis. In justice to the hypothesis it should, however, be noted that the frequency of implicit intrusions, relative to the frequency of overt intrusions, should be positively correlated with the availability, in the Thorndikian sense,[29] of the intruding responses, and that this availability of response should increase as the degree of learning increases. Furthermore, the rapid disappearance during relearning of the inhibition attributable to the implicit intrusions is a reasonable expectation. Presumably, both the overt and implicit intrusions have the strength of their association with the current stimuli lessened by their "punished" occurrence in the early relearning trials, and the weakening effect of a single "punished" occurrence should be greater the greater the availability of the response.[30] Nevertheless, the number of such supporting hypotheses necessary for the defence of the major hypothesis indicates clearly the need for direct experimental tests, rather than a further logical elaboration, of the hypothesis.

Available alternative hypotheses regarding Factor X identify it with some process which occurs during the learning of the interpolated responses and weakens the original S-R relationships. McGeoch has argued against such hypotheses,[31] especially the perseveration hypothesis, on the basis of evidence of a relative (percentage) constancy of the degree of RI over intervals varying from 20 min. to 1 week. The thesis is that the percentage of RI should increase with an increase in the interval before recall if a direct weakening of the original S-R relationships occurs during the interpolated learning, because such weakened connections should show the more rapid forgetting characteristic of weak associations when they are subjected to normal intercurrent activities. The failure of realization of this deduction may not, however, be considered conclusive because (a) all the factors in the experiment, particularly the factor of degree of learning of the original and interpolated materials, could not be accurately evaluated, and (b) an arbitrary zero-point was used in computing the percentages of RI. It should be added that the deduction stated above does not necessarily follow from a 2-factor theory of RI which considers only one of the factors to be a weakening of the original S-R relationships at the time of the interpolated learning. At any rate, there are at present no data which definitely prohibit the hypothetical localization of Factor X in the interpolated learning period.

The perseveration theory of Müller and Pilzecker, recently defended by Skaggs, would identify Factor X with a disruption of the normal perseveration of the neural processes involved in the original learning.[32] This theory lacks precise statement and an attempt to evaluate it in the light of particular data usually ends in a theoretical impasse in which one endows neural perseveration with all the properties implied by the results. It is sufficient in the present instance to suggest that the perseveration theory, although perhaps consonant with the negatively accelerated relationship between Factor X and the degree of interpolated learning, would have to be supported by additional postulates in order to explain the transitoriness of the RI attributable to Factor X.

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[29] E. L. Thorndike, *The Fundamentals of Learning*, 1932, 345-352.

[30] This is most clearly suggested by the study of R. E. P. Youtz, The weakening of word-number connections by "punishment," *Psychol. Bull.*, 35, 1938, 690-691.

[31] McGeoch, *op. cit.*, *J. Gen. Psychol.*, 9, 1933, 24-41.

The "disruption hypothesis" of Webb, recently defended by Britt, and the "generalization-conditioned inhibition" hypothesis of James and Gibson are attempts to attribute at least a portion of the measured RI to inhibitory processes localized in the interpolated learning period.[33] Both hypotheses are consonant with a transfer theory of RI since they assume that the original responses transfer (generalize) to the interpolated activity as a result of the similarity of the stimulus or response terms of the two activities. Both likewise assume that this transfer of responses causes the associations between the transferred responses and the stimuli involved in the original learning to be weakened. The difference between these hypotheses is to be found in the assumptions regarding the cause for the weakening of the original S-R relationships. In the "disruption hypothesis" it is said to result from the incorporation of the original responses in the interpolated activity and their consequent dissociation from the original stimulus factors. In the "generalization-conditioned inhibition" hypothesis the weakening is attributed to the unreënforced or 'punished' occurrence of the original responses during the learning of the interpolated responses. Britt's recent elaboration of the "disruption hypothesis" suggests that this difference is chiefly a matter of terminology. A precise formulation of each hypothesis, with due regard for the known conditions of the presence and amount of RI, is lacking.

In lieu of a precise formulation of these theories at this time, it is sufficient to point to the implications of the data of the present experiment. Unless the observed transfer (generalization) of responses from the original to the interpolated activities, and their subsequent inhibition through non-reënforcement or 'punishment,' is to be given no significance, these data clearly favor a theory that attributes a portion of the RI to a weakening or unlearning of the original S-R relationships during the interpolated learning. Overt intrusions of entire original syllables during the interpolated learning occurred with sufficient frequency, especially during the first 10 trials of the interpolated learning, to warrant the assumption that transfer or generalization of response occurred. The fact that most of the intruding and displaced syllables were similar or were in the same serial position in the two lists lends support to this interpretation, and the generality of the phenomenon may be inferred from the fact that it has been observed by McKinney and McGeoch in written recalls of interpolated lists after learning by the method of complete presentation.[34] Presumably, there were also many unidentifiable partial overt intrusions and implicit intrusions in both studies.

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[32] G. E. Müller and A. Pilzecker, Experimentelle Beiträge zur Lehre vom Gedächtniss, *Zsch. f. Psychol., Erganzungsband,* 1. 1900, 1-300; E. B. Skaggs, A discussion of the temporal point of interpolation and degree of retroactive inhibition, *J. Comp. Psychol.,* 16, 1933, 411-414; and The concept of retroactive inhibition, *Psychol. Rev.,* 33, 1926, 237-244.

[33] L. W. Webb, Transfer of training and retroaction: A comparative study, *Psychol. Monog.,* 24, 1917, 1-90; S. H. Britt, Theories of retroactive inhibition, *Psychol. Rev.,* 43, 1936, 207-216; H. E. O. James, The problem of interference, *Brit. J. Psychol.,* 22, 1931, 21-42; E. J. Gibson, Retroactive inhibition as a function of degree of generalization, *Psychol. Bull.,* 35, 1938, 626.

In order to complete this interpretation it must be assumed that the S-R relationships responsible for these complete and partial intrusions suffered inhibition and weakening when the intrusions occurred. This is justified since it is commonly assumed that an unreënforced or 'punished' response will be weakened, and the intruding responses are clearly 'punished' when the anticipation method is used. The question remains, however, whether this 'unlearning' factor could be expected to have the functional characteristics assigned to Factor X.[35] These functional characteristics present theoretical difficulties. Most of the intrusions of entire syllables occurred during the first 5 trials of the interpolated learning, and their frequency did not increase regularly as the number of trials on the interpolated list increased. These facts are consonant with Hovland's recent discovery that generalized conditioned galvanic responses extinguish very rapidly,[36] but, when it is assumed that the total amount of unlearning of the original syllables is a function of the frequency of such unreënforced or 'punished' intrusions, the curve for the relationship between this unlearning factor and the degree of interpolated learning does not coincide with the curve for Factor X in Fig. 3. Furthermore, the rapid dissipation of the inhibition due to Factor X during relearning requires the assumption that the unlearning of the original responses which results from their 'punished' occurrence disappears very rapidly when they are again 'rewarded.' There is no independent evidence on this point either in studies of reward and punishment or in studies of conditioned discrimination.

The identification of Factor X with conditioned inhibition or the weakening of a connection through punishment is not clearly affirmed by the data of the present experiment. However, the data are not incompatible with the hypothesis when allowance is made for the fact that partial overt intrusions and implicit intrusions during the interpolated learning were not subject to observation. Neither are they incompatible with the hypothesis which attributes all the measured RI to the competition of original and interpolated responses at the time of attempted recall and relearning. In fact, the data raise many more problems than they solve. Especially do they emphasize the necessity of an experimental analysis of the temporal locus of the factor or factors responsible for the RI. Without question, a systematic study of the initial amounts and the relative persistence of RI with various degrees of original and interpolated learning will be a necessary preliminary to the formulation of a precise theory; and such a study must involve a careful observation and analysis of the behavioral correlates of the RI, such as the inter-list intrusions.

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[34] *Op. cit.*

[35] It is premature to speculate at this time regarding the nature of this "unlearning process," be it called the weakening of a connection through punishment of it, conditioned inhibition, or experimental extinction. Thorndike, *op. cit.*, 277-313, and G. R. Wendt, An interpretation of inhibition of conditioned reflexes as competition between reaction systems, *Psychol. Rev.*, 43, 1936, 258-281, have suggested that the inhibition or 'unlearning' of one response occurs when it is unreënforced or 'punished' because another incompatible response has been fixated. As Helen Peak and Louise Deese suggest (Experimental extinction of verbal material, *J. Exper. Psychol.*, 20, 1937, 244-261) this incompatible response in the elimination of an available verbal response may be merely the set not to respond. If this interpretation is given to the 'unlearning' factor postulated in the present connection, it is clear that the difference between the 'unlearning' and 'competition' factors is to be found in the nature of the competing response at the time of recall. It might then be maintained that the 'true locus' of the processes responsible for all the RI is at the time of attempted recall and relearning. However valid this contention may be, the separation of the 'unlearning' factor and the 'competition' factor, and the emphasis on the dual locus of the processes responsible for the RI, is a worthwhile theoretical device at the present time.

[36] C. I. Hovland, The generalization of conditioned responses. III. Extinction, spontaneous recovery and disinhibition of generalized responses, *J. Exper. Psychol.*, 21, 1937, 47-62.

SUMMARY

(1) In the experiment, 24 *S*s learned 1 list of 18 nonsense syllables for 5 trails, rested for 30 min. or learned a second list for 5, 10, 20, or 40 trials, and then relearned the original list to a criterion of 2 successive errorless trials. The anticipation method was used throughout and the erroneous anticipations were recorded verbatim, which permitted an analysis of the relationship between retroactive inhibition and the frequency and locus of overt inter-list response intrusions.

(2) The results support the following generalizations.

(a) All measures show an increase in retroactive inhibition with early increases in the degree of interpolated learning and a *decrease* in retroactive inhibition with very high degrees of interpolated learning. The decrease in retroactive inhibition with high degrees of interpolated learning is much more pronounced in the relearning trials after the first and in the number of trials required to relearn to mastery, which suggests that the retroactive inhibition produced by interpolated learning disappears during relearning more rapidly the greater the degree of interpolated learning.

(b) Syllables from the interpolated list occur frequently as erroneous responses during the recall and relearning of the original list. In general, the frequency of these overt inter-list intrusions is correlated with the amount of retroactive inhibition as revealed by the traditional measures.

(c) The intrusions of interpolated syllables during the recall and relearning of the original list can be attributed to either the similar serial positions of the intruding and displaced syllables or to the identity of letters in the 2 syllables. Intrusions based on the similarity of serial positions occur earlier during the relearning process than intrusions based on the formal similarity of the syllables, and the latter increase in frequency as the degree of interpolated learning is increased; whereas, the former first increase and then decrease in frequency as the degree of interpolated learning is increased. With early increases in the degree of interpolated learning the intrusions based on both factors occur later during the relearning process, but with further increases in the degree of interpolated learning the intrusions based on the formal similarity of the syllables tend to occur earlier in the

relearning process and the intrusions based on serial positions continue to occur later and later during the relearning process.

(d) There are overt signs of proactive inhibition of the learning of the interpolated list. That is, syllables from the original list occur as erroneous intrusions during the learning of the interpolated list. These intrusions occur for the most part during the first 5 trials on the interpolated list.

(3) The following interpretations were ventured.

(a) The results are interpreted as favorable to a transfer theory of retroactive inhibition, as a consequence of the correlation between the frequency of overt intrusions and the amount of retroactive inhibition, and as a consequence of the correlation between the occurrence of an intrusion and the similarity of the serial positions or the formal identity of the intruding and displaced syllables.

(b) The theory of retroactive inhibition which attributes all the inhibition to a competition of orginal and interpolated responses during the recall and relearning of the original responses is, however, questioned. Since it can be shown that there is a large amount of retroactive inhibition on the first relearning (recall) trial after 40 interpolated trials even though there are very few overt intrusions on this trial, it is argued that some factor other than the competition of original and interpolated responses may be responsible for a portion of the retroactive inhibition.

(c) Several theories regarding the nature of this unknown factor are examined, and it is proposed that this factor is a direct 'unlearning' of the original responses at the time the interpolated list is learned. Such 'unlearning' would presumably follow from the unreënforced occurrence of the original syllables as erroneous responses during the learning of the interpolated list, this phenomenon having been demonstrated in the data of this experiment.

GEOGRAPHICAL ORIENTATION

By T. A. RYAN and M. S. RYAN, Cornell University

The topic of orientation with respect to places, directions, and positions has long been of interest to psychologists and to students of biology. The ability of animals to travel long courses over unknown territory, the differences among men in skill for maintaining direction, and those frequent illusions of place, distance, and direction which annoy and baffle the human traveler, have especially been made the subject of discussion, casual description, and occasional experimentation.

The present study concerns an aspect of the general problem of orientation in man somewhat unlike that found in most researches, where emphasis has been laid upon 'objective accuracy' and upon its conditions.[1] The 'objective' approach is of obvious value in a practical way. It commonly fails, however, to inform the reader upon the nature of the psychological processes involved in orienting, *e.g.* how the individual perceives his surroundings as related to himself and how he passes from the immediately perceived field to more remote regions. It is this passing from the immediate field—as, for instance, from this room where I now sit—to objects, scenes, and events lying beyond, including them all in one plan of orientation, which we especially consider in the present experimental study.

In accordance with our aim to investigate the nature of psychological performance involved in geographical orientation, we have undertaken experiments which are descriptive in character. Any precise measurement of 'objective' or physical accuracy of orientation we have deliberately left out of our procedure. Our problem was simply to find out what orientation in a general geographical setting is like *for the observer.* This, as it seems to the writers, is the first question to be asked in any such inquiry where an unknown performance of the organism is to be investigated.

The general plan was to ask *O* a question or to set him a problem concerning geographical relations, and then to obtain a description, as exact as possible, of

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[1] The general literature may be roughly set down under four headings; (1) theoretical discussions, with emphasis upon instinct, (2) personal accounts of illusory orientation, (3) experimental studies of homing among animals, and (4) experiments upon human accuracy in orientation.

the activities by which *O* arrived at his answer.[2] The several series to be described differed in the kind of orientation involved or in the sort of question asked.

The crux of our method lies in making the questions difficult enough or in making the orientation difficult enough to provide a clear cut performance. For in most skilled performances the products are so condensed and foreshortened as to make description extremely difficult. As we engage in everyday activities among familiar surroundings, orientation likewise becomes so stable and foreshortened that we scarcely notice that we *are* oriented, to say nothing of an ability to describe just what the means and the terms of orientation are.

In an early series of experiments we met this difficulty by disturbing the orientation of *O* in the immediate perceptive field, making it more laborious without destroying it altogether. In the later series *O* was comfortably oriented, but the questions which he had to answer were either unusual or else they forced him to go beyond the perceptive surroundings.

Series 1. *O* was seated in a rotating chair and given the following general instruction:

> This is not an experiment in rotation. You will be blindfolded and moved about very slowly. If you feel vertigo or dizziness at any time please report it at once and the experiment will be stopped.

The more specific observational instructions were as follows:

> At the signal 'ready' you will be moved slowly about. At the signal 'now,' movement will stop, and *E* will ask a question. As soon as you have answered the question, report upon your performance in terms of product. Report everything which occurs from the 'ready' signal until the time of report.

O was rotated slowly by hand, first in one direction and then in the other but in random order. As *E* rotated the chair he also moved about in the room so that his voice would not serve as a fixed point of reference, coming as it did just at the crucial time when movement ceased and *O* was interrogated.

The observations took place during the morning when accessory sounds were relatively unobtrusive. Since the *O*s were not to be completely disoriented and since we were not studying the conditions which govern spatial placing, random noises were to our advantage rather than disturbing factors. The only thing which we wished to avoid was a constant cue which would remain the same from observation to observation. This would lead to rapid habituation, which we were trying to avoid.

After *O* had reported, he was instructed:

> Remove the blindfold and report what occurs as you do so.

This latter instruction was added with special reference to those cases where the blindfold orientation was objectively disturbed. We wished to obtain a description of re-orientation when visual pattern was restored.

The tasks for *O* in this series were of two general kinds; (a) pointing toward objects within the room, and (b) pointing in the direction of invisible objects outside (usually buildings on or near the campus).

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[2] The following *O*s, all practiced in observation, served in the experiments: Dr. Cleo Chrisof (*Ch*), Dr. R. M. Lindner (*Li*), F. N. Jones (*Jo*), J. I. Lacey (*La*), and J. W. Macmillan (*Ma*).

Series 2. This series was made up of several groups of questions and problems. *O* was seated comfortably in a familiar room of the laboratory and given the following instructions:

> I will ask you a question. After you have answered it, report everything which has occurred from the time of the question until your answer.

The tasks presented were of four kinds: (i) *O* was asked to point in the direction of some city or town. (The list included cities in near-by regions and also places at greater distances.) (ii) Tasks took the form of giving the campass directions from Ithaca to some other place. (iii) The task was the same except that *O*'s home city or town was substituted for Ithaca. (iv) *O* was presented with questions in the form: "Give the compass directions from —— to ——." Pairs of cities from all parts of the world were used.

Ten tasks of each kind were set for each *O* (40 questions for each of 5 *Os*). Because of the fulness of most of the reports, only 3 to 5 reports were secured in each observational period.

The conclusions and results of this study are then based upon a total of 260 separate descriptions, secured from 5 trained *Os*.

EXPERIMENTAL RESULTS

The reports show almost without exception that the act of orienting occurs in several successive stages. Before considering the whole process, therefore, it will be well to consider the nature of the stages in the procedure, and later their integration and the transition from one to another. These momentary orientations may be considered as the *materials* which the organism makes use of in working out the task. Of these there are several main groups, as shown in Table I.

TABLE I

KINDS OF ORIENTATION

(A) Immediate perceptive orientation
(B) Relation of perceptive field to other objects outside of the field (primary directionalization)
    (1) specific objects related directly to perceptive field
    (2) primary directionalization in compass coördinates
(C) Orientation deduced from (or held in) verbal formula
(D) Directional relations between objects outside the perceptive field
    (1) maps and 'air-views'
    (2) telescoped imaginal traveling

The primary orientation within the immediate perceptive field will be taken for granted. This is not to say that perceptive orientation is not an important psychological problem, nor that it is not important in the wider kind of orientation in which our interest lies. But it is in itself such a wide topic that we must be content to refer the reader to such summaries as that of Gibson.[3] For purposes of the present discussion, we shall take orientation in the directly apprehended field as one of the 'givens' from which we set out, noting that it may take various forms. We center our interest in those moments where the orientation of the organism gets away from the structure of the immediate field to objects, places, and directions outside the perceived 'here and now.'

Primary directionalization. One of the simplest and commonest, and at the same time one of the most difficult to understand from the point of view of ordinary accounts of psychological activity, is what we shall call 'primary directionalization.' Here the relation of the 'here and now' to other places is inherent in the present apprehended scene.

The commonest mode consists in a direct *implication* by objects in the field of something which lies just-beyond. This was particularly common in our rotational experiments. For example, *O* reported that he went around in a visual place even though blindfolded. Rotation was accompanied by visual 'imagining' of the objects in the room. We cannot say that one 'imagines' the rotation, for the visualizing was really an integral part of the perceiving. The translation of the body as perceived directly *implied* corresponding changes in the visual scene.

The same sort of implication was carried still further when *O* was asked to localize objects outside the room. The south wall of the room seemed to imply directly the library which lay beyond. One of the characteristics of the south wall was that it was in the direction of the library. The question did not necessarily do anything except to bring out this characteristic of objects already present.

In our later experiments we found primary directionalization of this kind carried still farther (farther in the literal, geographical sense, not necessarily in the sense of a more complicated performance). The fact that 'New York is over that way' seems to have become an immediate characteristic of a certain portion of the visual field. When asked to point toward a distant but familiar place, *O* behaves as though it were part of his immediate surroundings or at least immediately bound up with some aspect of the present perceived scene.

We shall select a few examples from the large number found in the reports:

Vis. per (room going around faster than I was moving). [It changed in rate, but it was hard to keep it going as slow as I perceived myself as going.] (*Jo*-1-2)[4]

In this instance the visual accompaniment is, as often occurs, 'deliberately' controlled by *O* for the purpose of making his task easier. But at the same time we notice that this visual pattern has a more or less independent existence, and in many of our examples we find that the visual scene is 'just there' much as it is in ordinary perception. In the following instance this is the state of affairs:

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[3] J. J. Gibson and O. H. Mowrer, Determinants of the perceived vertical and horizontal, *Psychol. Rev.*, 45, 1938, 300-323.

[4] The letters in parentheses indicate the *O*, the number of the series, and the number of the item in the series, in that order. Common abbreviations which occur in recording the descriptions are as follows: *per* for perceiving, *imag* for imagining, *com* for comment, *rem* for remembering, *insp* for inspecting. Items in parentheses are the products of the performance, *i.e.* a description of the things perceived, imagined, etc. The sign > means "successive functional phase."

We must emphasize that descriptions like the above are excerpts and simply describe one stage of a longer process.

> Imag (room: to determine my position). [*O* questioned concerning this statement.] > per (self as sitting facing the door into the other room). [I was just as sure of it as if I had my eyes open.] (*Ch*-1-5)

The reports indicate that the process is not to be considered as somatic perception *plus* the auditory perception *plus* visual imagining, but that the whole process is organized and turns out a more or less unitary field. In fact, there seems to be an ever increasing body of evidence that space as perceived, especially bodily space, is not to be separated into compartments such as tactual space, visual space, auditory space, and so on. Even before the beginning of the century Washburn pointed out that vision plays an integral part in localization on the skin—that visual imagery is not just a by-product, but that it actually contributes to the perception.[5]

Perceived objects in space may be non-specific as far as sensory modality is concerned (Galli).[6] Successive visual and tactual stimuli led to reports of 'something moving' from one place to another, the 'something' being neither visual nor tactual.

Again, tactually perceived things may extend beyond the limits of the body, as Hoisington's experiments upon the apprehended length of lifted rods demonstrated. Here it was found that the perception of length "is based primarily upon the experience of two opposed pressures whose intensive ratio falls within certain limits."[7] Our concern here is not, however, with the essential factors underlying such perception, but with the nature of the perceived result. The fact that Hoisington has to resort to exact control of conditions in order to demonstrate that specific factor is in itself sufficient evidence that we do not ordinarily perceive *pressures* but a qualification (length) of the object.

In Hoisington's experiment, visual 'imagery' seems to play a minor rôle. Perhaps this is because the underlying factors are so specific for the perception of length that vision was unnecessary. At any rate, in the general kind of perceptive orientation here discussed the factors are not very specific. This non-specificity may explain why the perceiving of distant objects is visual rather than directly tactual, as in Hoisington's 'length.'

Putting these varied facts together, it would seem best to discount the term 'imagining' which was used by our *O*s to describe the visual aspects of their orientation with respect to near-by surroundings. The visual pattern which they describe is a closely integrated part of the perceived orientation. *O*s label their performance 'imagining' merely because visual receptors are eliminated by the blindfold, but *descriptively* the visual patterns are perceptive in character, forming an extension of the space which is directly attributable to stimulation.[8]

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[5] M. F. Washburn, Über den Einfluss von Gesichtsassociationen auf die Raumwahrnehmung der Haut, *Phil. Stud.*, 2, 1895, 190-225.

[6] P. A. Galli, Über mittelst verschiedener Sinnesreize erweckte Wahrnehmung von Scheinbewegungen, *Arch. f. d. ges. Psychol.*, 85, 1932, 137-180.

[7] L. B. Hoisington, On the non-visual perception of the length of lifted rods, this JOURNAL, 31, 1920, 146.

[8] We did not question *O* concerning functional labels, since he was instructed to emphasize the description of functional products. The important thing for us here is the recognition of the close relation between these visual patterns and other perceptual patterns in the total orientation at any moment. Whether we define

Primary directionalization with respect to more distant objects. Many of the orientations which fall here are essentially like those already illustrated. *O* is not only oriented to objects in the room, but also to near-by buildings and landmarks. Here perceiving follows the same pattern as in rotation. Consequently we shall illustrate here only those kinds of relation to distant objects which show a broader sweep of orientation. The greater geographical range may imply only greater background and previous training, with no evidence of a more complicated performance.

One difference, however, does appear. The field becomes less unitary in the sense that the objects toward which *O* is oriented are not within a single perceived area. Objects which are in the field *imply* the more distant objects. This implication is extremely difficult to describe. A characteristic of a particular street or road is that it takes you to the place in question (*e.g.* "this is the Elmira road"). This is more than a mere verbal label. Whither the road leads has become a property of the road itself, analogous to the *Aufforderungscharakter* of the objects involved in acting.

At the same time, however, more is involved than this implication-character of present objects. The distant town or place is somehow *present,* although out of sight, in much the same way that the objects of the room in the first series were present although not visible in the ordinary sense of the word.[9] The distant town is present in the sense that it exists in the vague boundaries of the field, but it does not come out of a separate act of imagining.

The following examples illustrate the orientation with respect to distant objects with which we are now concerned:

(1) Per ("Greenland") OI (determine its position) > imag and insp (Greenland is behind me) [automatic pointing]. (*Ch.*-2-ib-4)

(2) Imag (Porto Rico in front of me and to the left). (*Ch.*-2-ib-8)

(3) Per plus comp (question) >search [automatic pointing] >com ("that way") >vis imag (hazy outlines in that direction). (*Jo*-2-ia-2)

(4) Com ("Elmira over that way") >imag (inlet, bridge, hills, road in valley) > comp (at end of this road is Elmira). (*Li*-2-ia-1)

(5) Insp (own position) >imag (wall cut out so I could see Library) >insp (London lies that way and a little this way). (*Li*-2-ib-1)

(Two *Os* almost never reported primary directionalizations of this kind, substituting modes of orientation to be discussed later.)

These descriptions are almost always vague. *O* lacks words for adequate description. Yet when we put all of the instances which bear a general resemblance together, the features which we have describd come out more clearly. It may be that in our introductory remarks about this kind of directionalization we have been too explicit and concrete. But this was done in order to catch its essence. It is by its nature very difficult to set down on paper in a coherent way.

Let us summarize. We have illustrated two closely related kinds of report. In the first, the immediate perceptive field is filled out and extended to the just-beyond in space where the observer *sees* things which are not physically present. Yet he sees them in such a way that

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'imagining' in such a way as to include or exclude such patterns is a matter of convenience as far as our present problem is concerned.

[9] For an illustration of this 'non-qualitative' presence see K. Koffka, *Principles of Gestalt Psychology,* 1935, 322.

they form part of the total spatial framework in which he apprehends himself and in which he is orientated. Some would call this 'imagining,' but it is rather a form of perceiving.

In the second kind of report, objects at a relatively great distance do not perceptively become a part of the general field. Yet they are present. They are 'off there' and out of sight, and yet 'really there.' Objects in the present field imply and point toward them. The observer is still oriented with respect to these distant objects as he is oriented to the window which is behind him but which he 'knows' to be there.

Primary directionalization with respect to compass directions. For certain *Os* the present perceptive field frequently has an inherent property of 'compass' direction, for one of our *Os* the compass directions are almost always something which must be worked out in words and step by step. The first sort we consider first. Here a street is at once a way-with-trees-down-the-side and a north-and-south-way. This without verbal label or phrase 'associated with' the object.

We might say that the organism has built up a more or less permanent compass-framework into which is fitted the present scene. But the notion of framework is itself in need for further clarification. We have no evidence that the 'framework' exists for the individual in any psychological manner. The term is just a convenient abstraction which describes a characteristic inherent in many concrete perceptual situations. We may, however, use it as a convenience.[10]

Before considering our illustrations of this primary directionalization of the compass variety, let us mention one other way in which it may be treated. Some may contend that the compass orientation is the result of an implicit generalized muscular response; that, when *O* is operating under a task to find his way or to place distant objects, the old boy-scout method of finding directions by reference to the right and left hands and the like has become foreshortened to incipient pointing gestures which give a cue to direction. This is a definite possibility, and as a matter of fact many of the examples we shall cite involve incipient, as well as overt, pointing. The objection which we may raise regarding this as a solution of the problem is simply that muscular gestures are not directions, even though they may be indispensable to orientation. A description of muscular twitches cannot substitute for a description of the psychological performances and objects. The fact remains (and will be illustrated in the following descriptions) that direction *is a property of the scene* remembered, perceived, or imagined.

We turn to reports.

(1) Search plus imag (a kind of bodily posture: myself going south with Baltimore as goal). (*Ch*.-2-ia-3)

(2) Insp (I'm facing west). (*Li*-2-ia-2)

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[10] Frameworks may, to be sure, become, on occasion, separate objects as the result of more complex activities, as was suggested in the preliminary paper on this subject: T. A. Ryan, Dynamic, physiognomic, and other neglected properties of perceived objects; a new approach to comprehending, this JOURNAL, 51, 1938, 641. It is not necessary, however, to call upon such a notion in describing the sort of orientation under consideration at the moment.

The description in these cases seems to be more meager than it is in connection with any of our other categories. But the meagerness of the descriptions is itself an indication that the directionalization is inherent in the field. Otherwise *O* would have to go through a more complicated performance to arrive at it.

The following illustrations seem to indicate that even where *O* was directly oriented with regard to the distant object this compass direction was inherent in the general orientation with respect to the distant object. In other words, the field involved both direct implication of the distant object and at the same time directionalization with regard to compass directions.

(3) [Automatic pointing behind: translation into] com ("north"). (*Ch*-2-ib-6)

(4) Imag plus insp (Rome in the direction behind and to the left of me) [checked movement of pointing in that direction by reinstatement of] > FI > com ("southeast"). (*Ch*-2-ib-7)

We shall come back to compass directionalization in connection with some of our later kinds of orientation, for it is naturally intimately concerned in many sorts of performances.

Orientation deduced from verbal formula. Under this category we mean to include those instances in which *O* is unable to solve the problem by means of primary directionalization of the field, and is forced to proceed by a series of deductive steps in which verbal patterns play a dominant part.

In *O*s who rarely exhibit primary directionalization of the compass variety, the orientation is not usually a property of the perceptive field in the implicit way described above. Rather is orientation secured by means of labels of a purely verbal sort which are associated with certain familiar objects. For example, there is a hill visible from the windows of the laboratory which is named "West Hill." This often served as an explicit point of reference for deducing orientation. As a rule, bodily movements, postures, and gestures were coupled with the verbal label.

In terms of the genesis of orientation in the individual, such verbal deduction may represent an early stage out of which some *O*s have never progressed. If this is so, the problem of genesis is difficult because we have to explain how the organism progresses from verbal labels to new properties of the perceptual field itself. Again, however, we cannot carry this further because of our principal interest in description.

Examples of purely verbal deduction of orientation follow:

(1) Insp (which direction am I facing?) > imag plus insp (surroundings) > com ("lake is on right-hand side," "lake is west," "west is to the right"). (*Ch*-2-ia-1)

Note that even here there was primary directionalization of a simple kind. The lake was not visible to *O*, who 'just knew' it was to her right; but verbal deduction was needed for the compass directions.

Once orientation was thus determined, it was carried over in verbal but foreshortened form from one task to another, as the following quotation from the next report shows:

(2) Insp (where am I facing?) > rem (discovery in the last problem that I am facing south). (*Ch*-2-ia-2)

In other instances, even where *O* was completely directionalized in his surroundings, distant objects were placed in a purely verbal way. For example, Virginia is understood as a southern state without being actually related to the present perceptual field. *O* of course 'knows' this even when he is completely disoriented in the present perceptive field. Even for unfamiliar places, a few remembered verbal tags or automatized verbal associations may come into the solution of the problem. The distant place is a topic or concept completely unrelated to the surrounding scene. The following examples have been selected to illustrate this sort of understanding of a place name.

(1) Comp (Europe is north of U.S.) > comp (adjusting pointing to take account of this). (*Jo*-2-ia-9)

(2) SI (remember that Tokyo lies farther north than most people think). (*Jo*-2-ia-10)

Directional relations outside the perceived field. Some of the tasks set could be solved quite independently of orientation or lack of orientation within the present scene. In these cases, *O* resorted to imaginal devices of various kinds. Some of these are maps and 'airplane' views to be expected, others were less obvious. In spite of the obvious nature of the maps and air-views, there are certain facts of psychological import which appear in the results.

First, the maps were more than mere pictures: they were tied up in various complex ways with the orientation of *O*'s body. This is especially true when maps were used in solving problems of pointing toward distant cities, or in giving compass directions from *O* toward a distant place. Here obviously the map is of little use unless its direction schema is somehow related to the present scene. The question of how this relation comes about and what it is like is not only important for orientation but also for the problem of symbols and signs at large.

Even where the present situation is not under consideration, *O* is somehow oriented in the imaginal scene with respect to the map, with respect to compass coördinates, and the like. The map seems often to be something which includes *O* in it rather than something passively looked at.

The bird's-eye or airplane view is used in exactly the same manner as the map, and may be employed in familiar or unfamiliar territory.

The important psychological question in both cases is how *O* gets direction from the map. This is usually a highly practiced procedure difficult to divine from the reports. *O* usually speaks of "reading off the directions from the map" and can say little more about it. Apparently the directions have become just as much an inherent part of the map as they are in the perceived scene in primary directionalization. In other words, there are no 'symbols' for direction on the map: lines on the map do not *stand-for* directions. A diagonal from lower left to upper right simply *is* a north-east line, and "north-east" (in the report) simply describes this direction *on the map.*

In those cases where *O* must relate map-directions to orientation of his body in the immediate surroundings (esp. in Ser. 2-ia), we gain a little more insight into the relation of directions on the map to directions in the geographical environment.

Sometimes the imagined map is literally made a part of the immediate surroundings, as in the following instances:

(1) Imag (physical map with myself here and with the ocean and London beyond). (*Li*-2-ib-1)

(2) Imag (whole area around here—lake, Ithaca, Cortland, Elmira, scenery as seen from here and from an airplane) > comp (Elmira was on this map in that direction) > [pointing]. (*Ma*-2-ia-1)

We insert the following description to show the difference between an imagined map completely divorced from the present situation and the above descriptions where the imagined map merely extends the perceptive situation at its borders:

(3) Vis imag (surface representing a map—no particular form—, in one corner Ithaca and in other "New York" spelled out) > com ("I know the direction—New York is SE of Ithaca, but she wants me to point to it. Can it be done?") > com (go down State St., turn to left, etc.) comp (Impossible to point to it, I don't know all the turns). (*La*-2-ia-2)

In a few instances, like (1) and (2) above, the map is explicitly 'tacked on' the immediate surrounding. More frequently, however, there is little more than a very vague 'taking account of' or 'reference-to,' which, for the *O* highly practiced in reading maps, seems to be about all that can be said in the way of description. For example:

(4) Vis imag (dark brown map of U.S. as though I were standing on hill or up in the air looking down towards the Miss. river) [pointing to direction of New Orleans as it was on the map.] > insp (that way and a little that way since I'm here). (*Jo*-2-ia-4)

Note that the imaginal maps used in these pursuits are often more than conventional maps; they are condensed or schematic landscapes.

Telescoped imaginal traveling. This way of identifying places might be called 'temporal mapping,' since it is, like maps and air-views, condensed and foreshortened, but it also involves a series of created events out of which orientation or localization comes. Here *O* divines the direction to a city or the relation between two cities by means of an imaginal trip from the one place to the other. The whole temporal process is very brief and highly schematc and during the devised trip, orientation of the sort described as 'primary directionalization' is involved. Each road over which *O* travels is *directionalized* either with respect to the compass directions or with respect to the coördinates of the present field. From this point of view, then, this imaginal traveling is to be understood as of exactly the same mode as primary directionalization in the perceptive field. There are, however, other aspects to be considered.

In imaginal traveling *O* ignores details of the route, devising a single direction which sums up the whole trip. It is as though *O* were a geometer, adding up all of the angles to find a resultant direction. Actually, of course, this is not done explicitly, and *O* is usually quite unable to state how he arrived at his final result. Apparently remembering or imagining a familiar route involves a reduction to essentials and a loss of irrelevant details, without any necessity for logical deduction.

Following are some examples of this sort of orientation:

(1) Rem (driving through Trumansburg and going in that general direction) . . . insp (*this* way then *that* way from here). (*Li*-2-ib-3)

(2) Rem (road) > imag (going out that way) > imag (turn in road by stadium) > imag (Ithaca here and Dryden there, disregarding the turn) > comp (Cortland east and slightly north). (*Li*-2-ib-6)

(3) Com ("windows of Morrill Hall facing campus are to the left, go straight down College Ave. to State St. turn right and then right again") *cum* kin per (turning trunk) > comp (directly in back). (*La*-2-ia-2)

(4) Imag insp (self driving to Annapolis) > com ("north-east"). (*Ch*-2-ii-2)

Imaginal traveling of this kind appears only twice in *Jo*'s reports. *Ma* reports something like it frequently but it seems to be less explicitly an actual traveling, and more often is a sort of elaborated map with details along the road added.

The total course of performance. Having considered the temporal units which are the parts of an entire solution, the description of the total course of performance can be briefly considered.

Of the rotational experiments little need be said since the whole course was made up of a series of short primary directionalizations in the perceptive field (Table I, A and B). These two kinds of orientation succeeded each other or were variously combined.

In the later experiments, where *O* had to relate his perceptively present orientation to distant cities and places, the total course of the performance was more complex. *O* typically shifted rapidly back and forth from one to another of all of the fractional means of orientation which we have described. Very little in the way of a typical pattern, or order of progression can be made out since the mode varied with the type of problem, with *O*'s knowledge of the places involved, and with *O*'s skill in orienting performance.

It must be said, however, that the 'directional' aspect of the problem tempered everything which *O* did. Thus while the course of performance may seem haphazard and random when considered as momentary devices, this appearance is an artifact of analysis. We must remember that all of the items which we analysed out had in common directional properties which made them more or less interrelated, interchangeable, and fitted to serve similar ends.

SUMMARY AND CONCLUSIONS

When regarded from the standpoint of psychological description, the world to which the individual geographically orients himself may be shown to include much more than the physical surroundings which immediately affect the organism would imply. First, the perceived field is variously extended outward from its primary boundaries. Secondly, it commonly has incorporated with it such a system of reference as the mariners compass suggests, this system becoming part and parcel of the entire 'present' field, the two moments being as inseparable as color and shape in the object.

In the light of our descriptive survey, it would seem that a related study by one of us had exaggerated the distinction between 'personal' and 'impersonal' frames of reference. There can be no sharp general line drawn between a 'personal orientation,' in which body-objects and their positions and relations are focal, and an 'impersonal' frame from which body-objects are absent. Indeed, the processes of spatial orienting appear, when reported descriptively, so fluid that any emphasis placed therein upon a scene or object—whether body-object or other object—is, as a rule, only momentary, fragmentary, and passing.

A by-product of this study holds a certain importance for the psychology of comprehending and thinking. One might well expect to find that tasks involving the complicated relations of space and place would fundamentally demand signs, symbols, elaborative inductions, and other non-apprehensive devices commonly associated with higher mental operations. We seem to find, however, that the orienting tasks chiefly call for an extension of the basal perceptive mode, into which imagining is sometimes incorporated in a restricted way.

It is primarly to the constant and skilful exercise of these two apprehensive functions and to the relatively invariant physical conditions of the environment that the adult human organism owes its coherent and stable chart of the earth's surface, to which it actively, constantly, and dextrously orients itself from moment to moment and from year to year.

THE CHRONAXY OF COLD AND WARMTH

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Although essentially a physiological measure,[1] chronaxy may be applied in various fields of psychological research. The applications to date have, however, been relatively few in number, and the bulk of them have been in the field of sensation.[2] The chronaxy of various of the sensory end-organs has been measured: for vision,[3] taste,[4] the vestibular mechanism,[5] and the skin senses. The first experimental work upon the sensory mechanisms of the skin was done by Bourguignon, who stimulated the sensory nerves directly.[6] Stein,[7] Ruffin,[8] and Walthard and Weber[9] stimulated the

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[1] Louis Lapicque, *L'excitabilité en fonction du temps*, 1926, 1-371. Henri Fredericq, Chronaxie, *Physiol. Rev.*, 8, 1928, 501-544. M. O. Wilson, Chronaxie, *Psychol. Bull.*, 32, 1935, 4-32.

[2] The experiments of Jasper upon handedness (H. H. Jasper and Marthe Bonvallet, Rôle de l'écorce dans l'organisation asymétrique des chronaxies des nerfs symétriques chez les rats droitiers ou gauchers, *C. R. Soc. Biol.*, 116, 1934, 991-994; H. H. Jasper and E. T. Raney, The physiology of lateral cerebral dominance, *Psychol. Bull.*, 34, 1937, 151-165), of Kreezer and Bradway upon intelligence (George Kreezer and K. P. Bradway, Relation between Binet mental age and motor chronaxia, *Arch. Neurol. & Psychiat.*, 34, 1935, 1149-1171), and of Bourguignon upon emotionality (G. Bourguignon, Relations de la chronaxie vestibulaire avec l'émotivité à l'état normal et dans pseudo-mongolisme et la demence précoce, *C.R. Acad. Sci.*, 193, 1931, 250-253), may be cited as examples of experiments in other fields. In these investigations, the attempt was made to find some relation between certain chronaxy values and some trait of the subjects. Thus Jasper reported a particular pattern of extensor-flexor muscular chronaxy in the right-handed individual, which was reversed in the left-handed subject. Kreezer and Bradway found a statistically significant difference between the average chronaxy of adult feebleminded *Ss* with a mental age of 3 yrs. and that of adult normal *Ss*, for the motor point of the triceps, but not for two flexor motor points examined, for other chronaxic indexes, or for higher mental ages in the feebleminded group. Bourguignon claims that emotionality and vestibular chronaxy go hand in hand, but the reliability of his ratings of emotionality is in doubt. For this type of problem, then, chronaxy may or may not be useful, dependng entirely upon the particular question to be answered.

[3] G. Bourguignon and R. Dejean, Double chronaxie du système optique de l'homme, *C. R. Soc. Biol.*, 94, 1926, 750. S. L. Last and F. Laubenthal, Beitrag zur normalen und pathologischen Chronaxie des optischen Systems, *Dsch. Zsch. f. Nervenhlk.*, 129, 1933, 278-282.

[4] A. Chauchard and B. Chauchard, Recherches quantitatives sur l'excitabilité de l'appareil du goût chez l'homme, *C. R. Acad. Sci.*, 184, 1927, 1273.

[5] G. Bourguignon and R. Dejean, Chronaxie normale du nerf vestibulaire de l'homme, *C. R. Soc. Biol.*, 94, 1926, 750.

[6] G. Bourguignon, *La chronaxie chez l'homme*, 1923, 1-417.

[7] H. Stein, Untersuchungen über die Chronaxie der Sensibilität, *Dsch. Zsch. f. Nervenhlk.*, 100, 1927, 221-232.

end-organs in the skin. Stein and Ruffin failed to indicate the modality with which they worked. This criticism applies only in part to the work of Walthard and Weber. Working with pressure and pain, Walthard and Weber differentiated the quality of the sensation at the time the rheobases were established, but neglected to do so when the chronaxy masurements were made. Neff and Dallenbach were the first experimenters to insist upon an identity of quality for rheobase and chronaxy, and they have reported values for pain and pressure which meet that condition.[10] Subsequently Opitz[11] and Hut[12] have also reported work upon pain and pressure which follows the procedure of Neff and Dallenbach.[13]

In the case of the temperature senses, the only worker reporting a successful chronaxy measurement is Bourguignon, who gives the chronaxy of warmth as being about 10 times that of the motor nerve in a given area.[14] He states explicitly that cold cannot be aroused by electrical stimulation. Hut also reports failure in his attempt to determine the chronaxy of cold.[15] Unpublished attempts in the Columbia and Cornell laboratories to determine the chronaxies of the temperature senses have proved unsuccessful both for cold and for warmth.

In the field of the skin senses, then, the measurement of the chronaxy for cold, and the verification of Bourguignon's determinations for warmth, still remain to be done. Here, then, is the purpose of the present study. We wished to discover whether we could under any conditions measure the chronaxies of the temperature senses.

PRELIMINARY EXPERIMENTS

We anticipated, because of the previous failures and the masking effects of pain and pressure, that the chronaxy measurements of cold and warmth would be difficult.

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[8] H. Ruffin, Chronaximetrische Untersuchungen des sensiblen und optischen Apparates, *Zsch. f. d. ges. Neurol. u. Psychiat.*, 140, 1932, 641-656.

[9] K. M. Walthard and A. Weber, Zur Chronaximetrie der normalen Hautsensibilität des Menschen, *ibid.*, 67-88.

[10] W. S. Neff and K. M. Dallenbach, The chronaxy of pressure and pain, this JOURNAL, 48, 1936, 632-637.

[11] G. Opitz, Untersuchungen über die Hautsensibilität. 5. Über die Chronaxie der Schmerzreizung, *Arch. f. d. ges. Physiol.*, 239, 1938, 736-747.

[12] L. J. Hut, *De sensibele chronaxie*, 1936, 88.

[13] Cf. Emil von Skramlik, Beiträge zur Psychophysiologie der Sinnesleistungen: Psychophysiologie der Tastsinne, *Arch. f. d. ges. Psychol.*, Ergänzungsband, 4, 1937, 1-510, 408, for a discussion of the necessity for following Neff and Dallenbach's suggestion that the rheobase and chronaxy qualities be the same.

[14] G. Bourguignon, Chronaxies normales des terminaisons servitires cutanées chez l'homme, *Amer. J. Physiol.*, 90, 1929, 294; Électrophysiologie. Interprétation des sensibilités thermique et douloureuses à l'aide des chronaxies sensitives cutanées normales et de leurs variations dans la syringomyélie, *C. R. Acad. Sci.*, 197, 1933, 792-794. Cf. also Von Skramlik, *op. cit.*, 238, who reports several other references to Bourguignon which we were unable to verify.

[15] L. J. Hut, *op. cit.*

For this reason, every *O* was given an extended period of training before the actual chronaximetric determinations were undertaken.

Observers. The *Os* for the entire course of experimentation were Mr. John W. Macmillan(*Ma*), senior assistant in psychology, Mr. Oliver L. Lacey(*La*), Sage fellow in psychology, and Mr. Henry L. Sisk (*Si*), assistant in rural education, with a minor in psychology. *Ma* had had previous experience in observing in this type of experiment, but *La* and *Si* had not, although they were familiar with the general methods and results of experimentation upon the skin.

Cold: (a) Aroused by adequate stimuli. First of all, *O* was trained in the experience and report of cold. During this period he sat at a table with his right forearm held rigidly in position by means of a plaster cast that was made especially for him. The cast exposed the dorsal surface of the forearm, upon which area all of the experimental work was done. This area was shaved at the beginning of every experimental hour, and a cross-grid of 2-mm. squares was stamped upon it. The cold spots were then located by means of punctiform stimulation with a Dallenbach thermoesthesiometer through which water was circulated in order to control its temperature.[16] The water, taken directly from the University mains, varied from day to day in temperature between 12° and 19° C. *O* was required to evaluate the intensity of the sensations aroused on a five-point scale: 1 being very weak, and 5 very strong. The intensive gradations were recorded so that only the stronger spots would be selected for further experimentation. In practice, the latter experiments were limited to spots of intensity 3 or 4, with now and then one of 5. The room temperature ranged from 19° to 21° C. from day to day. In these preliminary experiments, usually two areas were mapped at every experimental period. Training in the reporting of cold was continued until the *Os* were thoroughly familiar with the quality and their intensive reports of it varied over the range of the 5-point scale.

(b) Aroused by inadequate stimuli. Since the current which was to be used in determining the chronaxy was an inadequate stimulus, the *Os* were next stimulated with three different forms of inadequate stimuli in order that they would be familiar with cold, no matter how aroused.

(1) Pressure. Pressure, the most easily applied of the inadequate stimuli, and the one least likely to arouse distracting concomitants, was tried first. A blunt gutta percha point, which aroused no sensations of temperature when brought to bear upon the skin, was pressed lightly on a cold spot, and moved to and fro with a sort of 'stirring' motion. Under these conditions, cold was usually reported. It was not, however, aroused by a stationary pressure; movement was required.

(2) Prick. The second inadequate stimulus used was the prick of a needle. The needle was prepared by sharpening under a binocular microscope until the point was long and very sharp. When the skin had been softened by rubbing with soap and a rough paper towel, the application of this needle aroused only pain, and no pressure. In stimulating the cold spots, the needle was probed slowly into the skin. Frequently a clear pain as well as cold was aroused. It was difficult always to strike a cold spot directly, so that several jabs of the needle often were necessary before cold was aroused. When aroused, it was clear and in no way obscured by the accompanying pain.

(3) Electrical. Finally, a faradic current was applied by means of an induction

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[16] Dallenbach, Some new apparatus, this JOURNAL, 34, 1923, 92 ff.

coil. The active electrode was a moderately heavy piece of copper wire, bent double so as to give a rounded point about 1 mm. in diameter. The passive electrode consisted of a sheet of copper covered with a padding of gauze on one side which was simply moistened with tap water, since this contained enough dissolved material to serve as a fairly efficient electrolyte. It was merely held in the hand opposite to the arm being stimulated. When the current was adjusted to the point of being slightly painful, this type of stimulation proved very effective in arousing cold. It is interesting to note that the intensity of the spot reported by *O* during mapping bore no observable relation to the ease of its arousal by faradic stimulation.

When these inadequate stimulations were accomplished, the attempt was made to measure the chronaxy of cold. After positive results with cold were obtained, the experiments upon warmth were undertaken.

Warmth: (a) Aroused by adequate stimuli. In the experiments with warmth, the general plan of procedure was the same as that for cold except that warm instead of cold water was circulated through the aesthesiometer. The water was warmed, by being passed over an electric coil, to a temperature of approximately 45°C. Training progressed with the adequate stimulus for warmth until the *O*s were thoroughly familiar with the 5-point intensity scale.

(b) Inadequate stimuli. Then the same three inadequate stimuli—pressure, prick, and faradic current—were administered, and proved as effective for warmth as they had for cold. At first, the *O*s had some difficulty with these observations, due to the more diffuse and indefinite nature of the sensation of warmth, but after a few days' training, they found that the difficulty had vanished. Warmth finally proved easy to observe, even in the presence of the pain and pressure which were aroused by the faradic current.

After the training in warmth, the work again was shifted to the measurement of chronaxy, this time for warmth. Thus the plan of experimentation was training in cold, chronaxy of cold, training in warmth, chronaxy of warmth. The strength-duration curves for cold and warmth, and a few measures of the chronaxy of pressure and of pain, were taken at the very end of experimentation.

MAIN EXPERIMENTS

(1) Chronaxy measurements: (a) Cold. The actual chronaximetric determinations were made by means of the thyratron-tube chronaximeter described by Kreezer, Coakley, and Dallenbach.[17] This instrument delivers rectangular pulses of current, whose durations are controlled by means of a decade condenser. The active electrode, attached to the cathode, was the same as that used in the study by Neff and Dallenbach,[18] and consisted of a ¼ in. glass tube with a platinum wire sealed through one side. The lead ended in a silver wire which lay in contact with a cotton wick about 1 mm. in diam. kept moistened with a moderately strong salt solution. The indifferent electrode was a piece of copper screening covered with cheese-cloth moistened with salt solution, which *O* held in the hand opposite to the arm being stimulated, or upon which he laid the opposite arm.

The casts mentioned in the preliminary experiments were discarded when we turned to the measurements of chronaxy because of their distracting effects. They were invariably cold, and though they could have been warmed to the physiological temperature of the arm, their use was omitted as unnecessary since there was no necessity of maintaining the arm in the same position from day to day. We subsequently, therefore, simply had *O* lay his arm upon a few towels spread on the table.

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[17] G. Kreezer, J. D. Coakley, and K. M. Dallenbach, A thyratron chronaximeter: its operation and calibration, this JOURNAL, 46, 1934, 641-647.

[18] Neff and Dallenbach, *op. cit.*, 633 f.

The procedure for the stimulation of cold was as follows. First a region on the dorsal surface of each forearm was shaved and two grids were stamped on each arm. The cold spots were then mapped by the continuous method,[19] usually until two spots of intensity 4 or 5 were found lying next to each other. In case the entire area was mapped without this occurring, the densest region of spots was selected for experimentation. Since one arm was mapped, and then the other, a period of about 10 min. usually elapsed between the mapping and the application of the electrical stimulus. One spot was always selected for stimulation from each grid. Next the rheobase for each of the selected spots was determined, by the short-circuiting of the usual control key of the chronaximeter, and the manual operation of a knife switch which yielded a pulse of rather long duration (about 0.5 to 1 sec.). The apparent crudeness of this method is rendered justifiable by the reliability of the rheobases obtained. The rheobases were always determined by an ascending series of voltages, which progressed by 1 volt steps until a sensation of cold was reported. After the rheobases for one arm had been obtained, experimentation was shifted to the other arm, so that a period of recovery intervened between this stimulation and the determination of the chronaxies. The chronaxy was obtained by doubling the rheobase voltage, and then gradually increasing the duration until cold was reported. It should be noted that these observations were extremely difficult. *The most important point of technique concerns the water used to moisten the active electrode.* If this water be at room temperature, its coldness either masks the faint cold aroused by the electrical stimulus, or it serves partially to adapt the cold receptors and thus to inhibit their response. In any case, the observation of cold under such circumstances is very difficult, if not impossible. It is necessary to warm the water well above physiological zero, and to warm the electrode wick before every stimulation by dipping it in warm water. Since the wick cools quite rapidly, due to its small mass, the result is that it quickly approaches physiological zero upon being applied to the skin, and thus eliminates both the masking effect of cold and the distracting and masking effect of a warm stimulator.

(b) Warmth. The method used in obtaining the measurements from the warm spots was identical, in most respects, to that for cold. The shaving, stamping, and mapping proceeded as before, except that on two of the experimental days three areas on each arm were used. The electrode, even in the case of stimulation of the receptors for warmth, was slightly warmed, since an extremely cold electrode, it was found, masked the warmth.

(2) Strength-duration curves. For the determination of strength-duration curves for cold and warmth a condenser-discharge chronaximeter, similar to that used by Bourguignon,[20] and diagrammed in Kreezer and Bradway,[21] was constructed. The

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[19] Dallenbach, The temperature spots and end organs, this JOURNAL, 39, 1927, 402-427.

[20] Bourguignon, *La chronaxie chez l'homme,* 1923, 1-417.

[21] G. Kreezer and K. P. Bradway, *op. cit.*

effective voltages were obtained by applying an ascending series of intensities at various durations. A curve was determined for each arm of every *O*.

(3) Chronaxy of pain and pressure. To serve as comparative measures, six determinations of the chronaxy of pain and of pressure were made upon every *O*. Three areas were used upon each forearm, and the procedure was as nearly as possible identical with that explained by Neff and Dallenbach.[22] Identity of rheobase and chronaxy sensations was insisted upon, and the observations proved very easy.

TABLE I

RHEOBASES AND CHRONAXIES FOR COLD

	Ma		*La*		*Si*	
	Rheo. (volts)	Chron. (ms.)	Rheo. (volts)	Chron. (ms.)	Rheo. (volts)	Chron. (ms.)
	50	0.876	50	0.388	50	0.596
	54	.898	50	.397	50	.610
	56	.883	51	.437	50	.621
	56	1.016	51	.403	50	.596
	53	.988	55	.448	52	.609
	58	.988	54	.437	51	.591
	51	1.030	50	.440	50	.609
	51	.988	54	.420	50	.609
	51	.883	55	.420	52	.596
	53	.977	51	.403	52	.609
	53	.911	57	.440	51	.673
	53	.988	53	.420	51	.621
	56	.988	54	.430	50	.610
	56	.898	56	.448	50	.596
	52	.988	53	.440	51	.621
	53	.988	53	.437	50	.596
Av.	53.5	0.956	52.9	0.426	50.6	0.610
σ	2.2	0.053	1.8	0.033	0.8	0.019

RESULTS

(1) Chronaxies of cold and warmth. Tables I and II show the rheobases and chronaxies obtained for every *O* for every experiment with cold and warmth. Sixteen measures of each quality were made with every *O*. The average rheobase for cold was 53.5 v. σ 2.2 for *Ma;* 52.9 v. σ 1.8 for *La;* and 50.6 v σ 0.8 for *Si*. The chronaxies were 0.956 ms. σ 0.053 for *Ma;* 0.426 ms. σ 0.033 for *La;* and 0.610 ms. σ 0.019 for *Si*. It will be observed that there is a high degree of consistency in the judgments of every *O*. The rheobases are comparable as among the *Os*, but the chronaxies are considerably different.

For warmth, *Ma* yielded an average rheobase of 49.4 v. σ 3.8 and an average chronaxy of 0.091 ms. σ 0.023, while the values for *La* were 44.8 v. σ 5.9 and 0.684 ms. σ 0.043 and for *Si* were 34.1 v. σ 15.1 and 0.662 ms. σ 0.060. The variability of the rheobases for warmth are greater than for cold, and especially is this true for *Si* whose values for warmth ranged from 10 to 52 v. The chronaxies, however, show no greater variability than in the case of cold. It should be emphasized that the *Os* found no difficulty in distinguishing cold from warmth under the conditions of the experiment. It occasionally happened that during a determination of the chronaxy of warmth, cold would be aroused, and also contrariwise, but no confusion resulted. To demonstrate the statistical differentiation of the two sets of measures, the critical ratios between the chronaxies found for cold and warmth were calculated. For *Ma* the C.R. is 3.4, for *La* 18.6, and for *Si* 3.0. These all indicate mathematical certainty that the two measures are genuinely different.

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[22] Neff and Dallenbach, *op. cit.*

TABLE II

RHEOBASES AND CHRONAXIES FOR WARMTH

	Ma		*La*		*Si*	
	Rheo. (volts)	Chron. (ms.)	Rheo. (volts)	Chron. (ms.)	Rheo. (volts)	Chron. (ms.)
	51	0.888	50	0.650	46	0.618
	49	.894	54	.618	46	.618
	48	.894	36	.659	32	.604
	50	.907	46	.683	34	.766
	51	.924	48	.665	18	.702
	52	.907	49	.683	52	.665
	52	.888	49	.697	49	.650
	48	.907	34	.711	43	.611
	48	.907	36	.611	14	.802
	50	.901	38	.691	50	.636
	52	.879	48	.683	47	.665
	51	.948	48	.691	40	.618
	51	.888	51	.702	22	.675
	52	.940	42	.808	10	.566
	36	.876	43	.697	15	.697
	50	.956	45	.702	28	.697
Av.	49.4	0.907	44.8	0.684	34.1	0.662
σ	3.8	0.023	5.9	0.043	15.1	0.060

(2) The strength-duration curves. The strength-duration curves for cold and for warmth are given in Figs. 1 and 2, respectively. The cold curve for *Si* is incomplete in the middle range, since he found that the current density in this region aroused so much pain and pressure as to mask the cold. The other *Os* give curves appearing much as the normal strength-duration curves obtained from homogeneous tissues generally. The curves

for warmth are also normal in shape, but those of *La* and *Si* show time-relations not corresponding to the chronaximetric determinations.

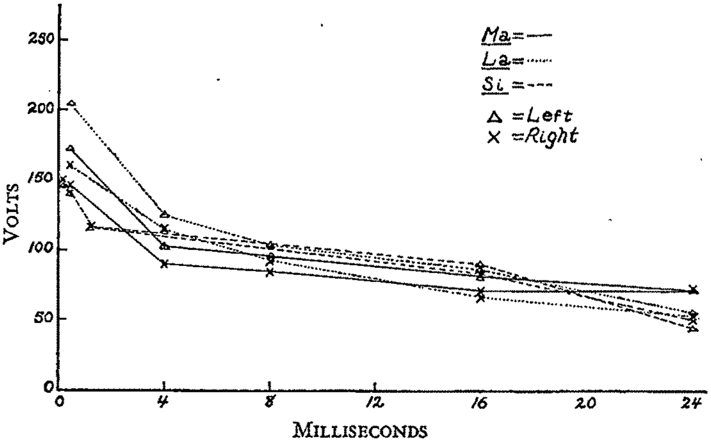

FIG. 1. STRENGTH-DURATION CURVES FOR COLD

(3) The chronaxies of the cutaneous senses compared. The fact that the chronaxies for all four of the cutaneous senses have been obtained from the same *O*s makes possible a direct comparison of their values. Table III shows the average chronaxies, in milliseconds, for every *O* for pressure, pain cold, and warmth. The average pressure values check well with those

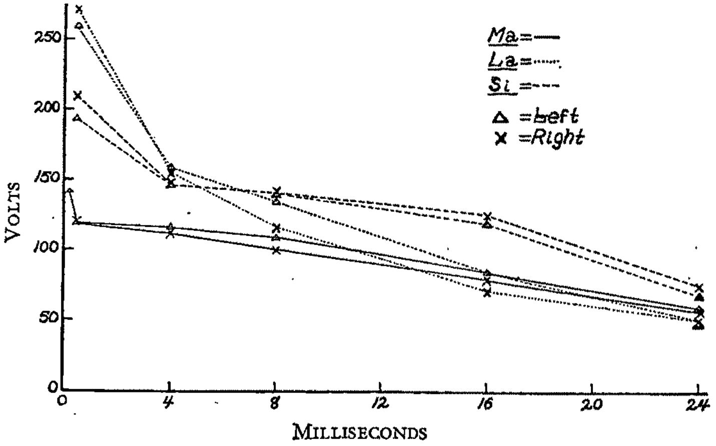

FIG. 2. STRENGTH-DURATION CURVES FOR WARMTH

obtained by Neff and Dallenbach,[23] and are the shortest in the cases of *La* and *Si*. The pain chronaxies are next in magnitude for these two *Os*, and shortest for *Ma*. It should be noted that the values for pain quoted here were derived from one component, the shorter, of what appeared to be a bimodal distribution of times. This point, it is hoped, will be the object of future research. The temperature chronaxies are the longest, but the relation between them is not consistent. Thus for *La* and *Si* warmth shows a greater magnitude, while for *Ma* it is cold which requires the greater time. The reversal of the relation between pain and pressure shown by *Ma* is paralleled by the reversal for cold and warmth.

TABLE III

AVERAGE CHRONAXIES IN MILLISECONDS OF THE FOUR SKIN SENSES WITH RELATIVE RANK ORDERS IN PARENTHESES

O	Pressure	Pain	Cold	Warmth
Ma	0.548 (2)	0.255 (1)	0.956 (4)	0.907 (3)
La	0.257 (1)	0.572 (3)	0.426 (2)	0.648 (4)
Si	0.185 (1)	0.600 (2)	0.610 (3)	0.662 (4)

DISCUSSION

Since the work of Blix,[24] Goldscheider,[25] and Donaldson,[26] which established the discreteness of pain, pressure, warmth, and cold as sense modalities, there have been several attempts to correlate specific end-organs with each of these sense departments. Donaldson,[27] and Goldscheider[28] themselves extirpated portions of the skin underlying cold and warm spots, but neither found any specialized end-organs. Häggqvist reported smooth muscle strands underlying the cold spots, but these results have never been verified.[29] When experimentation has been directed to the non-hairy regions of the body, such as the cornea,[30] or the prepuce,[31] it has been possible to demonstrate a correlation between Krause end-bulbs and cold spots. Thus Strughold and Karbe believed that they had shown the coincidence of cold sensitive spots on the cornea and Krause end-bulbs. Bazett and McGlone reported that Krause end-bulbs underlay cold spots, and Ruffini cylinders warm spots. Unfortunately, the demonstration was not direct, but involved the so-called 'population' method, in which the number of sensitive spots in a given area is correlated with the number of end-organs of various kinds found upon sectioning that area. When the hairy regions of the body have been the object of attention, no such correlation has ever been reported. Dallenbach[32] and Pendleton[33] both have failed to find specialized ending of any sort, and Dallenbach suggested that perhaps some kind of free nerve endings were responsible for the reception of temperature changes.

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[23] Neff and Dallenbach, *ibid.*

[24] Magnus Blix, Experimentelle Beiträge zur Lösung der Frage über die specifische Energie der Hautnerven, *Zsch. f. Biol.*, 20, 1884, 141-156.

[25] Alfred Goldscheider, Die specifische Energie der Temperaturnerven, *Monatshft. f. prac. Derm.*, 3, 1884 (see Ges. Abh., Bd. I, 1898, 53-76).

[26] H. H. Donaldson, On the temperature sense, *Mind*, 10, 1885, 408 f.

[27] Donaldson, *ibid.*

[28] Alfred Goldscheider, Histologische Untersuchungen über die Endigungsweise der Hautsinnesnerven beim Menschen, *Arch. f. Anat. u. Physiol., Physiol. Abtheil., Suppl. Bd.*, 1886, 191-227.

[29] Gösta Häggqvist, Histophysiologische Studien über die Temperatursinne der Haut des Menschen, *Anat. Anz.*, 44, 1913, 57 ff.

There have been several non-experimentally derived suggestions as to the probable end-organs for the temperature senses, among which the earliest seems to be that of Von Frey.[34] It was he who first proposed Krause end-bulbs as the receptors for cold, and Ruffini cylinders for warm. This seems to be the accepted correlation among physiologists.[35] Kiesow suggested that the erectores pilae muscles might be involved in the sensing of cold.[36] Head made the interesting observation that more than one type of end-organ might be responsible for each of the two senses.[37] Bourguignon proposes that Meissner's corpuscles and the free nerve endings of the skin coöperate to give rise to temperature sensations.[38] His conclusions are based, partially, upon his inability to measure the chronaxy of cold, and partially upon certain observed pathological changes in syringomyelia. One of the most interesting suggestions has come from Nafe.[39] He postulates that changes in temperature lead to contractions and relaxations of the blood vessels, notably the arterioles, of the skin, the stimulation afforded the nerve endings in the walls of these vessels by such changes in tone being perceived as cold or warmth. There are thus no specialized endings, and the patterns of contraction and relaxation are the determining factors. This hypothesis perhaps owes some debt to the suggestion of Goldscheider that the temperature sensitive spots follow the blood vessels.[40] Actually, then, nothing definite is known concerning the structure of the receptors for cold and warmth. The experimental results reported in this thesis, however, do shed a certain amount of light on this problem.

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[30] H. Strughold and M. Karbe, Die Topographie des Kältesinnes auf Cornea und Conjunctiva, *Zsch. f. Biol.*, 83, 1925, 189-200. H. Strughold, Die Schwellen des Kältesinnes am Auge, *Zsch. f. Biol.*, 83, 1925, 201-206. H. Strughold and M. Karbe, Die Dichte der Kaltpunkte im Lidspaltenbereiche des Auges, *Zsch. f. Biol.*, 83, 1925, 207-212. H. Strughold and M. Karbe, Vitale Färbung des Auges und experimentelle Untersuchungen der gefärbten nervenelemente, *Zsch. f. Biol.*, 83, 1925, 297-308..

[31] H. C. Bazett, B. McGlone, R. G. Williams, and H. M. Lufkin, Studies in sensation. I. Depth, distribution, and probable identification in the prepuce of sensory end-organs concerned in sensations of temperature and touch; thermometric conductivity, *Arch. Neurol. & Psychiat.*, 27, 1932, 489-517.

[32] Dallenbach, *op. cit.*, this JOURNAL, 39, 1927, 425-427.

[33] C. R. Pendleton, The cold receptor, this JOURNAL, 40, 1928, 353 ff.

[34] M. von Frey, Beiträge zur Sinnesphysiologie der Haut, *Ber. d. kgl. sächs. Ges. d. Wiss., math.-phys. Kl.*, 47, 1895, 181-184.

[35] S. W. Ranson, Cutaneous sensory fibres and sensory conduction, *Arch. Neurol. and Psychiat.*, 26, 1913, 1122 f.

[36] F. Kiesow, Zur Analyse der Temperaturempfindung, *Zsch. f. Psychol.*, 26, 1901, 231-246.

[37] Henry Head, *Studies in Neurology*, 1920, 63, 316.

[38] Bourguignon, Électrophysiologie, *C. R. Acad.*, 197, 1933, 792-794.

First of all, it would seem that the fact that reliably different chronaxies were obtained for cold and warmth would suggest specific, specialized end-organs subserving each modality. Bourguignon's suggestion concerning the coöperative action of Meissner's corpuscles and the free nerve endings must thus be discarded immediately. Also, Head's postulation of more than one type of ending for each sense department is disproved by the lack of a double chronaxy in either case, and by the absence of kinks or humps in the strength-duration curves. The possibility that the lack of correspondence between the strength-duration curves for warmth obtained from *La* and *Si* and the chronaxies might indicate the arousal of different receptors in the two cases may be countered by the following two considerations. First, the masking effect of the pain and pressure aroused by the current may have tended to raise the entire curve upward along the time parameter. Secondly, it may be, since the sensation arose quite late, according to the *Os*' reports, that the vaso-dilator fibers were being stimulated in this case, which would warm the tissues sufficiently to arouse the end-organs for warm. Against this latter suggestion is the possibility that the vaso-dilator fibers are iterative.[41] The question is still an open one, however, since the present curves appear similar to those obtained from visceral fibers.[42] It is further interesting to note that the values for the chronaxy of warmth reported by Bourguignon, about 5 ms. to 7 ms., correspond roughly to the time values of the curves we have determined.[43] Possibly, then, both Bourguignon's results and the present strength-duration curves refer to vaso-dilator fibers.

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[39] J. P. Nafe, The pressure, pain, and temperature senses, *Handbook of General Experimental Psychology*, 1934, 1049 ff.

[40] Alfred Goldscheider, *op. cit.*

[41] Janina Hurynowicz, Über die normale und pathologische Chronaxie der vasomotorischen Nerven, *Dsch. Zsch. f. Nervenhlk.*, 129, 1933, 273-276.

[42] G. H. Bishop and Peter Heinbecker, Differentiation of axon types in visceral nerves by means of the potential record, *Amer. J. Physiol.*, 94, 1930, 170-200; A functional analysis of the cervical sympathetic nerve supply to the eye, *ibid.*, 100, 1932, 519-532.

The chronaximetric results further suggest grave difficulties for Nafe's proposals. In the first place, it is unlikely that the same type of shock would arouse in one case cold, and in another warmth, since it would seem that the smooth muscles of the blood vessels should respond consistently in the same way to the same stimulus. Furthermore, the times obtained seem too short for smooth muscles. The possibility that the nerve endings in the blood vessels are being stimulated directly is ruled out by Nafe himself, since he postulates no specific nerve endings, and emphasizes the importance of the patterning of the tonic changes. That the vaso-motor fibers were being stimulated directly is rendered unlikely by the relatively longer times to be expected for the chronaxy of such nerves.[44] The variability of the rheobase for warmth as contrasted to that for cold is a damaging fact. It would seem that this result, coupled with the very consistent chronaxies in the two cases, would suggest that the warm receptors vary much more in depth than do the receptors for cold. Since for Nafe the essential receptor mechanism is in both cases the same, this should prove very difficult of explanation in terms of his theory.

On the positive side, the chronaximetric results can make only a few suggestions. The primary conclusion, of course, is that cold and warmth are subserved each by a single, specialized receptor mechanism. This follows from the reliable difference between their chronaxies, and from the extreme reliability of the values found. The variability of the rheobase for warmth further suggests end-organs varying greatly in depth. It might be profitable to select spots showing a very low rheobase, and thus shallow receptors, for extirpation. Of course, the chronaxies do not make possible specific identification of the end-organs. The relatively high chronaxies for the temperature senses, as compared with pain and pressure, is, however, very interesting. This fact leads to the conclusion that relatively insensitive mechanisms are involved. It thus tends to lend support to Dallenbach's conclusions that relatively non-specialized free nerve endings of some sort are responsible for the temperature sensations. The very high rheobase might also suggest either end-organs lying deeper in the skin, or end-organs surrounded by heavy layers of tissue which act as insulators.

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[43] Bourguignon, Chronaxies normales des terminaisons servitires cutanées chez l'homme, *Amer. J. Physiol.*, 90, 1929, 294.

[44] Bishop and Heinbecker, *op. cit.*

It may be concluded, then, that the results here reported show that there are specific, differentiated receptors for cold and for warmth. Although they can yield no definite suggestions as to the nature of these end-organs, they do place certain limitations upon hypothesizing in this field, as has been shown above.

THE RÔLE OF MOTIVATION IN LEARNING WITHOUT AWARENESS

By JAMES GRIER MILLER, Harvard University

The present evidence on learning to discriminate subliminal sensory stimuli is scanty, and there is none on the effect of motivation in such learning. This is a report of an experiment showing that changes in motivation affect the acceleration of the curve of the acquisition of skill in discriminating between subliminal stimuli.

ANTECEDENTS

In his presidential address in 1916,[1] Watson took pains to forestall one line of criticism of his view of the conditioned reflex by showing that it is not necessarily or essentially a 'voluntary reaction.' At that time *voluntary* and *conscious* were to some psychologists strangely synonymous. To prove his point Watson cited preliminary experiments which he had performed. He had found that it is possible to some degree to condition pupillary contraction, and he insisted that, since this contraction is mediated solely by the autonomic nervous system, conditioning can not be an essentially 'voluntary reaction.' In contradiction Hamel,[2] from introspective reports, decided that the conditioned response is always dependent upon consciousness. His evidence for this assertion was that the latent time of the conditioned reflex approaches the reaction-time of conscious discrimination, which is much longer than the time for ordinary reflexes involving striated musculature. He made no attempts, however, to develop conditioning to imperceptible stimuli.

In 1922 Cason conclusively demonstrated pupillary conditioning,[3] and reported further that some *Ss* were not aware of the conditioning stimulus, and that none was aware of the pupillary changes. Later the same year Cason reported conditioning of winking.[4] He said that the *Ss* all insisted that they were not aware of the winks until after they had occurred, and that there were no accompanying introspective 'images' or 'affective elements,' at least until after the winks.

Cason and Katcher in 1933 said: "Although it has been demonstrated that subliminal stimuli can influence bodily activities, it has not yet been shown that a connection can be formed between a subliminal stimulus and a conscious or unconscious response by the process of learning."[5] They insisted that definite findings on this problem would have important theoretical and practical value. Therefore they

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* Accepted for publication June 25, 1939.

[1] J. B. Watson, The place of the conditioned-reflex in psychology, *Psychol. Rev.*, 23, 1916, 89-116.

[2] I. A. Hamel, A study and analysis of the conditioned reflex, *Psychol. Monog.*, 27, 1919, (no. 118), 1-65.

[3] H. Cason, The conditioned pupillary reaction, *J. Exper. Psychol.*, 5, 1922, 108-146.

[4] Cason, The conditioned eyelid reaction, *ibid.*, 153-196.

[5] H. Cason and N. Katcher, An attempt to condition breathing and eyelid responses to a subliminal electric stimulus, *ibid.*, 16, 831-842.

attempted to condition breathing and eyelid responses to a subliminal electric stimulus. Although a variety of procedures was followed, they were unable to condition either response. They suggested better techniques which they thought might succeed. In the same year Hudgins by conditioning developed the pupillary reflex so that *S*'s pupil would contract if he thought the word 'contract.'[6] This was learning without awareness and beyond voluntary control, for two reasons: (1) because 10 of the *S*s were naïve and did not know that their pupillary reactions were being conditioned; and (2) because there are no afferent neurones passing from the iris.

Hamel had planned,[7] but never performed, experiments to discover whether the introspective limen and that determined by the method of conditioned response were identical. In 1933 Newhall and Sears reported experiments which seemed to indicate that finger withdrawal can be conditioned to stimuli below the statistical psychophysical limen, but they were uncertain whether this conditioning is accompanied by consciousness.[8]

Corroborating some evidence of Thorndike in 1932,[9] Thorndike and Rock devised, and in 1934 published the results of two experiments which purported to show that learning may proceed without awareness of what is being learned or intent to learn it.[10] In one of these, a free-association experiment, the *S*s were told that the task was to respond to the stimulus-words rapidly. Their answers were judged right or wrong by *E*, and the *S*s were told this judgment was made on the basis of an arbitrary list of 'right' responses for each stimulus-word. Actually words with sequential connections were called right and rewarded, and *S*s learned to give such free associations rather than other words, such as those connected with the meaning of the stimulus-word. Because the learning curves rose slowly, the *E*s assumed that the *S*s were not aware of the task, on the grounds that with insight the curves would have risen immediately to 100% correctness. *E*s also devised a skill-test on the same principles which gave much the same results. Irwin, Kauffman, Prior, and Weaver did not believe that the gradualness of the rise of the learning curve was an adequate criterion of learning without awareness,[11] and criticised Thorndike and Rock on that point. Their criticism was based on experiments which they performed using Thorndike and Rock's association-test, in which the *S*s were told the purpose of the experiment half way through it. This did not make the learning curves rise rapidly to 100%. In a reply, Thorndike and Rock admitted the invalidity of their criterion of gradualness, but said that their experiment still contained much evidence for learning without awareness.[12] They had not at any time asked the *S*s whether or not they were aware of the task, and no other test of unawareness was proposed. Irwin closed the debate with a short note reasserting his earlier request for such a criterion.[13]

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[6] C. V. Hudgins, Conditioning and the voluntary control of the pupillary light reflex, *J. Gen. Psychol.*, 8, 1933, 3-51.

[7] I. A. Hamel, *op. cit.*, 24-26.

[8] S. M. Newhall and R. R. Sears, Conditioning finger retraction to visual stimuli near the absolute threshold, *Comp. Psychol. Monog.*, 9, 1933, (no. 43), 1-25.

[9] E. L. Thorndike and others, *The Fundamentals of Learning*, 1932, 207 ff.

[10] E. L. Thorndike and R. T. Rock, Jr., Learning without awareness of what is being learned or intent to learn it, *J. Exper. Psychol.*, 17, 1934, 1-19.

[11] F. W. Irwin, K. Kauffman, G. Prior, and H. B. Weaver, On 'learning without awareness of what is being learned,' *ibid.*, 823-827.

[12] Thorndike and Rock, A further note on learning without awareness of what is being learned, *ibid.*, 18, 1935, 388-389.

Silverman and Baker[14] tried unsuccessfully in 1935 to condition to a subliminal shock an increase in breathing or in foot pressure. In 3 out of 12 *Ss*, however, increased frequency of the eyelid response was conditioned, and 2 of these were reconditioned. In former experiments the stimulus had been known and the response unknown; here the stimulus was unknown and the response possibly known. Neet claims to have corroborated part of this work.[15]

More recently Baker[16] has succeeded in conditioning the pupillary response below what he refers to as "the absolute limen of awareness of the stimulus," which is lower than the "psychophysical threshold," which was used by Newhall and Sears. Baker has experimented in detail with the nature of subliminal conditioning.

PROBLEMS

In an earlier paper,[17] the author gave experimental evidence that it is possible to discriminate between subliminal geometrical figures. without awareness either that a visual experiment is in progress or what the source of the subliminal stimuli is. The problems of this second study are:

(1) Can *Ss* learn to improve their subliminal discrimination even though they are unaware that they are taking part in an experiment in visual perception?

(2) Is subliminal learning like supraliminal learning in that the acceleration of the curve of acquisition is affected by changes in motivation?

(3) Is the hierarchy of motivations for subliminal learning like the hierarchy for supraliminal learning?

APPARATUS AND PROCEDURES

Four learning experiments were conducted. In each succeeding one the motivation was changed in a way that was thought, on the basis of the literature on learning with awareness, would accelerate the function. In the last two experiments indications of learning were obtained.

The basic apparatus used in all these experiments was described in detail by the author in the earlier article.[18] *Ss* were seated in a dimly-lighted room facing an 8 x 10 in. transparent mirror set in the opposite wall. *S*'s chair was about three yards from the mirror. Without *S*'s knowledge geometric figures were projected silently upon the back of the mirror at various intensities. The figures used were those on J. B. Rhine's ESP cards—*circle, plus, star, square, wave.* When projected on the mirror the figures were enlarged to almost the full size of the mirror.

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[13] Irwin, A rejoinder, *ibid.*, 389.

[14] A. Silverman and L. E. Baker, An attempt to condition various responses to subliminal electrical stimulation, *ibid.*, 246-254.

[15] C. C. Neet, A comparison of verbal, manual, and conditioned-response methods in the determination of auditory intensity thresholds, *ibid.*, 19, 1936, 401-416.

[16] L. E. Baker, The pupillary response conditioned to subliminal auditory stimuli, *Psychol. Monog.*, 50, 1938, (no. 223), 1-32.

[17] J. G. Miller, Discrimination without awareness, this JOURNAL, 52, 1939, 562-578.

[18] Miller, *op. cit.*, 566-567. The apparatus was purchased by Harvard University's Maria E. McMaster Fund for psychic research.

E sat behind *S* in a small room connected with *S*'s room through an open door. He sat behind a screen facing a panel from which he could control remotely the exposure of the cards and the illumination at which the cards were projected. This illumination was regulated by a variac, which gave nothing at 0 volts, and a comparatively dim, though definitely supraliminal, image at its top reading, 135 volts. By a periscope *E* could tell what figure was being presented on the mirror, even at illuminations well below the limen.

All *S*s were naïve, unaware of the projection apparatus and the purpose of the experiment.

Experiment A: Learning motivated by desire to please E. When *S* was taken into the experimental room, he was immediately led up to inspect the mirror on the front wall. *E* took ample precaution to see that *S* had sufficient evidence that it was a real mirror, being certain that he saw his reflection. This fact, however, was not emphasized so strongly that *S* had any cause to question that it was a regular mirror. With no light behind it gave reflections like an ordinary mirror.

S was then seated in his chair facing the mirror, shown the ESP cards, and told about Rhine's work. Then he was given oral directions approximately as follows:

> It is a common procedure in clairvoyance experiments for *S* to gaze into a crystal to help him find the answer he wants. This is usually done when *S* tries to picture three-dimensional objects. In this experiment, however, you are going to work with the figures on the flat surfaces of ESP cards. I am asking you, therefore, to stare into the flat surface of this mirror. All during the experiment I want you to stare into the mirror, and not to move your head or your eyes. You may, however, look at the mirror with any part of your eyes that you wish—either fovea or periphery. When your eyes tire, tell me, and we shall rest.
>
> The purpose of this experiment is to see whether, with experience, you can learn to do better in calling the ESP cards correctly than you can at present. I shall go into the room behind you and sit at the table behind the screen. Sitting there I shall expose the cards one by one. When I expose a card, I shall ring a signal bell. Please try to picture one of the five figures on the mirror as approximately 6 x 6 in. in size, that is, about one inch from either side of the mirror and two from the top and bottom. Report the first figure that you picture on the mirror. If you cannot get any cue from the mirror to help you, guess one of the figures, but say that it is a guess. As soon as you have mentioned a figure, I shall tell you whether you were right or wrong, and, if you were wrong, I shall tell you what the right one was.
>
> Please report also what kind of image you see on the mirror, if you see one; that is, whether it is a memory image, a thought image, an imaginatory image, an hallucinatory image, an after-image, or an actual physical image.
>
> Please use the following names for the cards: *plus, wave, star, circle, square.*
>
> Do not figure that out of every 25 calls you must call 5 of each figure. Though an equal number of each figure will be used, the cards are in a large pack, and it is quite conceivable that chance may turn up a long run of any figure.

As a matter of fact 200 cards were used, and each time before the pack was exhausted, the cards were reshuffled and put back into the machine, so that the pack was to all intents of indefinite size. Since *S* did not know that the cards were being projected from behind the mirror, it was natural that he should assume from the directions that *E* was exposing them behind him. This was the impression of all *Ss* until the apparatus was shown them at the end of the experiment.

In Experiment A a total of 12 series of 25 cards to call, three series a day every other day, were given to *F, G, H* and *I*. In the case of *F*, who was slow in calling the cards, only two series could be given the first day, but over the four-day period he was given 12 series.

To be sure that the *Ss* were never aware of the stimuli four precautions were taken: (1) The cards were projected at a constant illumination represented by 80 volts on the variac (about .0173 footcandles). For naïve *Ss*, like all in these experiments, it was found in earlier work with this apparatus[19] that none was ever aware of stimulus-objects presented at an illumination below 90 volts. The point of onset of the function of their awareness was about 95 volts. Sophisticated *Ss* in the earlier work, who knew all about the apparatus, however, were aware of figures at lower intensities. The point of onset of the function of their awareness was about 70 volts. Although no sophisticated *Ss* were used in the present work, the illumination used in Experiments C and D was 70 volts, in order to make as stringent a test as possible. Moreover, the data for Experiments A and B were prorated to the values they would have had if the experiments had been done at 70 volts (.0163 footcandles). This prorating was based on functions for discrimination at various voltages determined in the earlier experiment.[20] (2) A second precaution to make sure that the *Ss* were never aware of the stimuli was to make them report what kind of images they saw after each series of calls. In no case was an actual physical image reported. (3) A third precaution was to record the point at which *S* first saw an actual physical image at the end of the experiment, by raising the illumination until he reported one. This never occurred below 120 volts. Moreover, *S*'s limen was taken afterward by the method of limits. It was never below 90 volts. (4) A final precaution was to ask *S*, after showing him the apparatus, if he had suspected that images were being projected on the mirror. *Ss* always replied in the negative and exhibited surprise at the set-up of the experiment.

Because in Experiments C and D it was thought advisable to use discrimination between only two of the geometrical figures instead of all five, the results for all experiments will be given not in percentage of correct calls, but in terms of an arbitrary scale of degree of discrimination which will make them directly comparable. This scale was developed as follows: When five cards were used, 20% of the calls would be correct by chance. Discrimination would then be indicated by series of calls averaging consistently above 20%. Therefore the interval between the chance percentage and 100% (an interval of 80%), which is indicative of discrimination, was divided into 100 equal units of degree of discrimination. When only two figures were used, 50% of the calls would have been correct by chance, and results consistently between 50% and 100% would have been indicative of discrimination. In Experiments C and D using two figures, then, the interval between 50% and 100% was in like manner divided into 100 equal units of degree of discrimination, which were then comparable with the units from the experiments using five figures.

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[19] Miller, *op. cit.*, 577.
[20] Miller, *op. cit.*, 572.

In Experiment A the percentage of correct calls for a *S* for one day was determined, and —20% algebraically added to it. If the percentage of correct calls had been above 20, this would give a positive number; at 20 it would give 0; and below 20, a negative number. This was then multiplied by the factor 10/8 to convert it to the arbitrary scale. (If two figures only had been used, the conversion procedure would have been the same, except that the factor would have been 2 instead of 10/8.)

The results for Experiment A, prorated for 70 volts and reduced to the arbitrary scale of degrees of discrimination are shown in Table I.

Since the results from Experiment A gave no evidence of learning (all the data being near the chance value), an attempt was made to increase the motivation. That increased motivation was necessary was indicated by the fact that all the *S*s reported discouragement and frustration because they did not share *E*'s apparent belief in 'mind reading' and thought the task impossible, and because they were bored with the monotony of calling hundreds of cards. Counteracting this reaction of the *S*s was a continual problem in these experiments.

TABLE I

RESULTS OF EXPERIMENT A

S	Number of practice sessions			
	1	2	3	4
F	3.1	9.7	−0.3	1.5
G	−0.3	−0.3	4.9	−1.9
H	−0.3	1.5	−1.9	−6.9
I	−0.3	1.5	3.1	11.5
Av.	0.6	3.1	1.6	1.0

Experiment B: Learning motivated by apparent increasing success. J, K, and *L,* were used as *S*s. The task was made somewhat easier by raising the illumination voltage to 90 volts, only five volts below the point of onset of the function of awareness for naïve *S*s, and by interspersing figures at a higher voltage (110 volts). These cards at 110 volts were not counted in the scores, however, but were introduced merely for encouragement. In the first series of 25, all the cards were exposed at 90 volts; in the next series, two at 110 volts were interspersed, one near the beginning of the series, and one near the end; in the next series, four at 110 were interspersed; in the next, six; in the next, eight; in the next, 10; and in the next, 12. The same number of series was given each of four sessions, every other day. The cards at 110 volts were called correctly more often, but 110 volts was below the limen of awareness for naïve *S*s. The effect of interspersing these cards, therefore, was to make the *S*s think they were doing better with each succeeding trial. Otherwise the procedure was the same as in Experiment A.

The motivation was quite effective for *J*, and slightly for *L*. *K* developed a theory during the third session that the purpose of the experiment was to give cues as to which card was being exposed by the length of time of ringing the signal bell, and so took his cues from the bell rather than the mirror. His results were therefore poor for the last two sessions. Since he did not report his theory until the end of his fourth session, his data are included for the sake of completeness.

TABLE II

Results of Experiment B

S	Number of practice sessions			
	1	2	3	4
J	2.5	2.5	7.5	30.0
K	−5.0	5.0	−10.0	−10.0
L	2.5	7.5	10.0	2.5
Av.	0.0	5.0	2.5	7.5

In Table II are the results for Experiment B, prorated for 70 volts, on the arbitrary scale of degrees of discrimination. Since it appeared from the results of this experiment that even if learning did occur under its degree of motivation, it would not be very rapid, another attempt to increase motivation was made.

TABLE III

Results of Experiment C

S	Number of practice sessions			
	1	2	3	4
M	0.0	6.0	24.0	20.0
N	0.0	10.0	38.0	16.0
P	−10.0	2.0	4.0	14.0
Q	2.0	4.0	8.0	12.0
Av.	−2.0	5.5	16.0	15.5

Experiment C: Learning with monetary reward and punishment. Four naïve *S*s, *M*, *N*, *P*, and *Q*, were given four series of 25 cards each day for four consecutive days. They were bet five cents on every card: they received five cents for every card right, and paid five cents for every one wrong. Because it would simplify the betting, and because it would make the discriminatory task somewhat easier, only two of the five figures—*circle* and *plus*—were used in Experiment C. In order to be doubly certain that an adequate test was made, and that *S*s were unaware of all the cards, the illumination was lowered to 70 volts. Otherwise the procedure was as in Experiment A.

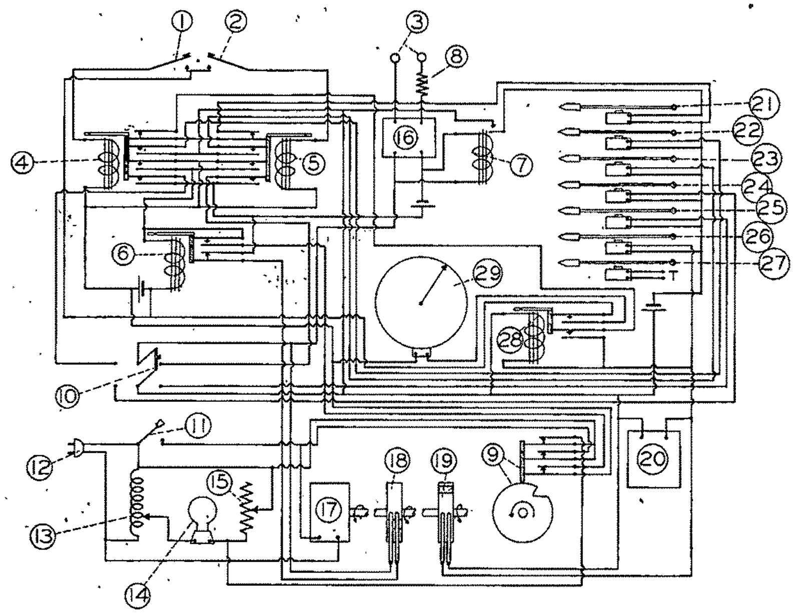

FIG. 1. WIRING DIAGRAM OF APPARATUS FOR EXPERIMENT D

1, *S*'s left key (*plus*); 2, *S*'s right key (*circle*); 3, shock electrodes; 4, 5, 6, 7, 28, relays; 8, 10,000 ohm resistor to counteract changes in *S*'s resistance; 9, motor driven 3-pole, single-throw switch; 10, double-pole, double-throw switch which *E* uses to close circuit for *circle* or *plus*, according to which is projected; 11, single-pole, single-throw main switch which closes circuit through motor of 9; 12, to 110-volt power supply; 13, variac; 14, lights furnishing illumination to project image; 15, variable dimmer resistor to control illumination before 9 shunts it out, so *E* can see card through periscope before it is exposed to *S*; 16, inductorium; 17, synchronous motor driving 9, 18, 19; 18, 19, commutator switches; 20, signal bell; 21, shock kymograph signal magnet (k.s.m.); 22, left key k.s.m.; 23, right key k.s.m.; 24, k.s.m. indicating 10 set for left key (*plus*); 25, k.s.m. indicating 10 set for right key (*circle*); 26, k.s.m. indicating illumination on; 27, k.s.m. to second-interval timer; and 29, chronoscope. Mr. Ralph Gerbrands is responsible for much of this electrical work.

The results of this experiment, which was carried on at 70 volts, on the arbitrary scale of degrees of discrimination, appear in Table III.

Finally an even stronger motivation was tried.

Experiment D: Learning with punishment by electric shock and reward by praise. Three naïve *S*s, *P, T,* and *V,* were used in this experiment.

An apparatus, diagrammed in Fig. 1, was used. *S* pressed the left key if he thought the card to be a *plus,* and the right if he thought it to be a *circle.* He had electrodes on his wrist, and, if he pressed the wrong key, he automatically received a shock immediately. If he pressed the correct key, he got no shock. If he pressed no key in three seconds, he got a shock. He was praised whenever he made a right choice. An automatic kymograph record was made, and the time from the flashing on of the lights projecting the card and the simultaneous ringing of the signal bell to the pressing of the key was recorded. This last observation was made in order to discover which was more rapid: the correct or the incorrect choice, the left or the right hand. No significant differences were found in either case.

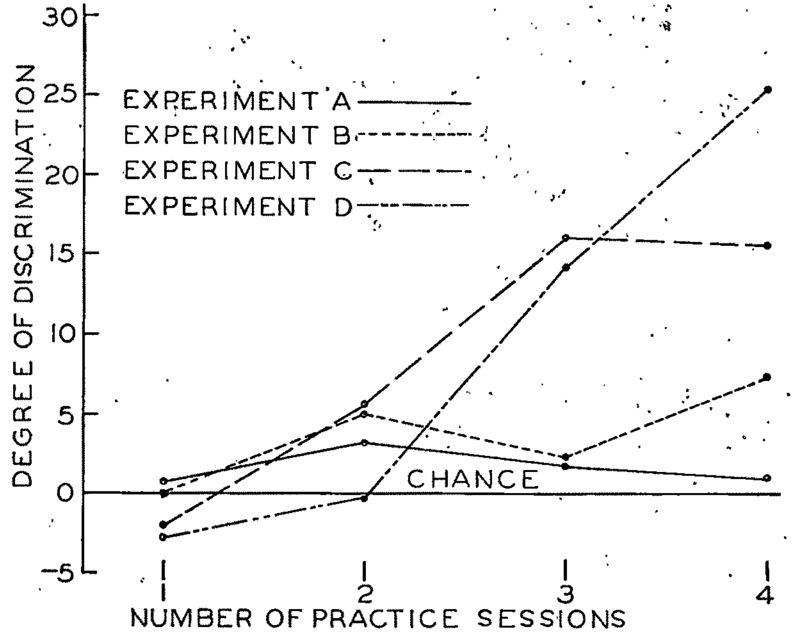

FIG. 2. AVERAGE RESULTS FOR FOUR EXPERIMENTS
Degree of discrimination is measured in arbitrary units explained in the text.

Five series of 25 were run for each *S* for four sessions, every other day. The illumination was at 70 volts. The results, on the arbitrary scale of degrees of discrimination, are shown in Table IV.

DISCUSSION

Fig. 2 summarizes graphically the average results of the four experiments. This figure definitely indicates that learning to discriminate subliminal cues is possible when *Ss* are unaware that they are taking part in an experiment in visual perception. It indicates that motivational factors determine the acceleration of the curve of acquisition, as is shown in the differences between the curves of Experiments A and B and those of C and D. It is well known in learning with awareness that monetary reward and punishment, electric shock and praise are stronger motivations than desire to please *E*, or such a desire combined with increasing success. This difference has also been shown to be true of subliminal learning, which to this extent is like supraliminal learning. Baker has said that conditioning above and below the limen are different in that it is easier to condition to a subliminal stimulus, and that the conditioning appears more suddenly.[21] His work indicates ways in which subliminal and supraliminal learning differ, while the present study shows one way in which they are similar.

TABLE IV

RESULTS OF EXPERIMENT D

S	Number of practice sessions			
	1	2	3	4
P	−8.8	−8.8	12.0	45.6
T	−2.4	12.0	23.2	16.8
V	2.4	−4.0	21.6	13.6
Av.	−2.9	−0.3	14.2	25.3

It is important to notice that in all the present experiments *S* was aware of the motivation even though he was not aware of the stimulus. This differentiation between awareness of stimulus or of response, on the one hand, and awareness of motivation, on the other, is important, and it has not often been carefully made by clinicians. Such techniques as have been employed here might well be used to determine which specific motivations are of most effect in unaware activities. Freudian theory on this point could thus be subjected to laboratory experimentation. Masochists might learn better to the tune of electric shocks than to praise. Individual differences might be determined by such techniques. The variations between *Ss* in the present results indicate that such deviations are great.

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[21] L. E. Baker, The pupillary response conditioned to subliminal auditory stimuli, *Psychol. Monog.*, 50, 1938, (no. 223), 16.

Three experimental criteria of lack of awareness in learning have been used in the literature: (1) gradualness of rise of the learning curve;[22] (2) asking *S* if he is aware of the stimulus;[23] and (3) maintaining the stimuli below some psychophysical limen. That the first criterion is inadequate Irwin, Kauffman, Prior, and Weaver have proved.[24] The second is adequate, but entails the difficulties and dangers of introspective report. The third is the most satisfactory. There has been some disagreement as to the best method of determining the psychophysical limen. Baker used what he called "the absolute limen of awareness of the stimulus,"[25] which he considers a better measure than the "psychophysical threshold," such as Newhall and Sears used. This use of the word *limen* is unjustified, because it does not fit the accepted usage. To psychologists *limen* has traditionally meant a statistically determined point, usually the point where the sensation is reported 50% of the time. Baker, however, means by *limen* the lowest point at which it is ever possible to have awareness. This is the lowest point of the psychophysical function of awareness, for which the author has suggested the term "the onset point" of the function of awareness.[26]

Aside from his terminology, however, Baker is right. If *unaware* is defined as *below the psychophysical limen,* difficulties will arise. At an intensity just below the limen *S* will be able to report almost half of the stimuli. Learning to react to these stimuli would not be learning without awareness in the ordinary sense. Therefore all the data in our experiment have been prorated for 70 volts, below the lowest possible point of onset of awareness.

SUMMARY

This experiment has shown: (1) that learning to discriminate geometric figures at subliminal intensity can occur without *S*'s awareness that he is taking part in an experiment on visual perception; (2) that the acceleration of such learning curves is affected by changes in motivation; and (3) that the relative strengths of certain motivations for learning without awareness are in general comparable to those for learning with awareness.

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[22] E. L. Thorndike and R. T. Rock, Jr., Learning without awareness of what is being learned or intent to learn it, *J. Exper. Psychol.*, 17, 1934, 1-19.

[23] *E.g.* H. Cason, The conditioned eyelid reaction, *ibid.*, 5, 1922, 153-196.

[24] F. W. Irwin, K. Kauffman, G. Prior, and H. B. Weaver, On 'learning without awareness of what is being learned,' *ibid.*, 17, 1934, 823-827.

[25] Baker, *op. cit.*, 2.

[26] J. G. Miller, Discrimination without awareness, this JOURNAL, 52, 1939, 574-577.

THE PITCH OF COMPLEX TONES

By Lloyd A. Jeffress, University of Texas

In a previous article the writer suggested that the stability of pitch of a complex tone in the absence of its fundamental might be similar in nature to the perceptual completion of fragmentary visual patterns.[1] The present paper describes a series of experiments undertaken to test this hypothesis.

Fletcher reports a number of experiments in which a complex tone with the fundamental missing, and in some cases with the fundamental and several overtones missing, is compared in pitch with "a standard, giving a fairly pure tone."[2] He employed a variety of complex tones from organ pipes, sung vowels, violin, piano and clarinet. The Bell Telephone Laboratories have issued a phonograph record (B.T.L.-5) illustrating the phenomenon. Fletcher also reports a series of similar experiments in which the source of the complex tone is a vacuum-tube generator capable of producing the 10 frequencies 100~, 200~, . . . 1000~. In most of the experiments his *Ss* reported that the incomplete complex tone had the pitch of the missing fundamental. When the tone generator was used, the pitch of the composite was the pitch of the fundamental even when only three frequencies, *e.g.* 700~, 800~, 900~, were sounded at an intensity level of about 90 db. Later, Fletcher repeated some of these experiments using a loudspeaker as the source of sound instead of the telephone receiver employed in the first series.[3] The results were similar. In almost every case in both series when the sound level was sufficiently high (usually above 45 db.), the pitch reported by the *Ss* corresponded to that of the "fundamental," *i.e.* of the common difference between the frequencies of adjacent partials. The exceptions occurred only in those cases where the common difference was not an integral quotient of the frequencies. Here, the *Ss* reported that the sound had no definite pitch, was like a noise, or was lacking in musical quality.

Fletcher explains the stability of pitch of a complex tone in terms of difference tones between adjacent partials. If two different frequencies of vibration are imposed upon a body so constituted that its displacement is not proportional to the applied force, the vibration of the body may be expressed as the sum of various simple modes having different frequencies. These frequencies will include such values as the sum of the two generator frequencies, the difference, twice the first, twice the second, etc., and the number and intensity of these extra modes of vibration will

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* Accepted for publication July 15, 1939.

[1] L. A. Jeffress, The case for, and some implications of the place theory of hearing, *Psychol. Rev.*, 46, 1939, 31-45.

[2] Harvey Fletcher, The physical criterion for determining the pitch of a musical tone, *Phys. Rev.*, 23, 1924, 427-437; *Speech and Hearing*, 1929, pt. 4, ch. 2.

[3] Fletcher, Loudness, pitch, and the timbre of musical tones and their relation to the intensity, the frequency and the overtone structure, *J. Acous. Soc. Amer.*, 6, 1934, 59-69.

depend upon the nature of the non-linearity of the body's response. Of these, the frequency resulting from the difference between the generator frequencies is the one that concerns us here. If we consider the frequencies of the partials of a complex tone, we see that the difference in frequency between any two adjacent partials is the same as the frequency of the fundamental. This means that, if a complex tone with its fundamental missing is applied to a non-linear response system, we may expect the fundamental frequency to be supplied as a difference-tone by the response system. The ear is, of course, such a non-linear system and will generate difference-tones, as many experiments have shown. Fletcher estimates the intensity of the 100~ tone produced when the six frequencies 500~, 600~, 700~, . . . 1000~ are applied to the ear, and he concludes that its amplitude may amount to as much as 75% of the amplitude of the loudest partial, when the pressure amplitudes in the outer ear canal are about 100 dynes per sq. cm.[4]

In most elastic systems such as the ear, the departure from linearity of response is a function of the amplitude of the displacing force. When the amplitude is small, the system may be essentially linear and generate no difference tone. In accordance with this fact, Fletcher found that when the intensity of the three tones, 700~, 800~, and 900~, was reduced below 45 db., the 100~ difference-tone disappeared and the three tones were heard as separate tones.

If the phenomenon of pitch stability in the absence of the fundamental could be demonstrated at intensity-levels low enough so that the ear-generated difference-tones are below the threshold of hearing, Fletcher's explanation would be inadequate, and the analogy with vision mentioned above would be somewhat strengthened.

EXPERIMENTS

(1) Preliminary. A preliminary experiment seemed to show that this might be the case. The writer and several other *S*s listened to the record demonstrating pitch recognition (B.T.L.5-B) played at a low intensity level and agreed that the pitch did not change when the fundamental, and the fundamental and second partial were eliminated from the tone of the organ pipe. They did not agree in the case of the clarinet tone. An analysis of the organ pipe tone is shown in Table I. It indicates that the fundamental was by no means eliminated from the tone as recorded, and of course, explains why the subjects agreed about the pitch of the tone. In the case of the clarinet tone, the fundamental was effectively eliminated.

(a) Apparatus. The apparatus used in the subsequent experiments was the same throughout. Various organ pipes were used as the source of sound. A ribbon microphone was placed near the opening of the swell chambers of an organ in a moderate-sized auditorium and connected to a preamplifier in another room so distant that the direct sound of the pipes could not be heard. Between the preamplifier and the main amplifier was a switch that connected either a frequency-bridge, for eliminating any desired frequency, or an attenuator. The output of the main amplifier was connected either to the telephone receivers used in some of the experiments, or to a power amplifier and loudspeaker. A wave-analyzer

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[4] Fletcher, *Phys. Rev.*, 23, 1924, 436.

connected across the amplifier provided the means for measuring the components of the sound. The telephone receivers were of the crystal type, and previous experiments had shown them to have a satisfactory frequency response over the range employed in the present experiments. The loudspeaker had a response which was substantially flat over a still larger range of frequencies. Neither produced appreciable distortion at intensities beyond any used in the experiments, and the amplifiers were free from distortion to an even greater degree.[5]

A switch in the output circuit permitted *E* to connect the loudspeaker or the telephone receivers either to the output of the amplifier, or to an oscillator capable of giving a very pure tone, or to both at once. Thus it was possible to present to the *S*s the complex tone of an organ pipe, the complex tone with any component eliminated or partially eliminated depending upon the adjustment of the frequency-bridge, the pure tone of the oscillator, or the complex tone with a pure tone of any desired frequency added to it.

(2) Tests for the presence of a 'subjective tone.' To be sure that the difference-tone generated in the ear when the applied sound is of low intensity is really below the threshold of hearing, some method of estimating its intensity is necessary. *S*'s mere inability to discover its presence in the complex of sound is not enough. The following method proved to be very sensitive.

A complex tone with the fundamental eliminated was applied to the ears by means of the telephone receivers. Its constitution was approximately as follows: fundamental, 388~, less than 8 db.; 2nd partial, 56 db.; 3rd partial, 62 db.; 4th partial, 60 db.; 5th partial, 43 db.; 6th partial, 46 db.; 7th partial, 46 db.; 8th partial, 42 db.; other partials, negligible. While this complex tone was sounding, a pure tone from the oscillator was introduced. Its frequency was about that of the fundamental of the complex tone, and its intensity was adjusted until it was barely audible. The pitch was then varied slightly up and down several times a second and the intensity still further reduced until this warble was just audible. Its intensity proved to be about 45 db. This is, of course, somewhat below the value that would have been threshold for a steady tone, since the warble is more easily noticed against the background of steady tone from the organ pipe. The value, 45 db., is a rough practical threshold for a tone of the frequency of the fundamental under the conditions obtaining, *i.e.* in the presence of a complex tone, and in a room in which the noise level was between 30 and 40 db.

The oscillator was then adjusted to a frequency a few cycles per second higher than that of the fundamental and to an intensity that permitted it to be steadily heard (about 48 db.). Then the frequency-bridge was unbalanced to permit more of the fundamental to come through. When the intensity level of the fundamental was sufficiently high, the fundamental and the oscillator tone could be heard beating. A measurement of this intensity of the fundamental showed it to be about 28 db.

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[5] The writer is grateful to Professor C. P. Boner of the Physics Department of the University of Texas for permission to use the apparatus, and for much helpful advice.

It is possible, by this technique, to detect the presence in the ear of a tone that is some 17 db. below the effective threshold. When the fundamental was eliminated from the complex tone applied to the ear, and the oscillator tone added, no beats could be heard, but if, by opening the doors to the auditorium, some of the direct sound of the pipe was admitted to the ears, the beats again became audible. This experiment seems to indicate that the method is sufficiently sensitive to detect in the ear the presence of a fundamental frequency, of whatever origin, strong enough to be of any value in determining pitch.

(3) Further preliminary experiments. It was the purpose of *E* at this point to present to the *Ss* complex tones devoid of the fundamental and sufficiently low in intensity so that the ear-generated fundamental was not detectible; then, if the pitch seemed to be that of the fundamental, to remove enough high partials from the remaining tone so that the pitch no longer seemed the same. This procedure was expected to give an indication of the way in which the pitch of the tone is dependent upon the spatial pattern the tone creates upon the basilar membrane.

These experiments showed that the pitch of such tones does not remain constant when the fundamental is removed. *Ss*, asked to decide whether the fundamental frequency or its octave best represented the pitch of the complex tone, sometimes chose one and sometimes the other, but usually reported that neither seemed a good match. One *S* with 'absolute pitch,' when asked what note he was hearing, *g* of 388~, reported "*g* and *d*," which he identified as having the frequencies of the second and third partials.

These results are, of course in agreement with the work of Fletcher discussed above, but they suggest that a test of Fletcher's theory would be to raise the intensity-level of the tone until the pitch would remain that of the fundamental, and then to test for the presence of an audible ear-generated fundamental. *E*, however, was unable to find any such intensity-level. Even with sounds having an intensity-level of 100 db., the *Ss* reported that the pitch of the tone devoid of its fundamental seemed to be higher than the pitch of the complete tone, and higher than the pitch of the oscillator when it was adjusted to have the frequency of the fundamental. In many cases the octave of the fundamental seemed a better match.

These results led to the performance of a final experiment to see to what extent a larger group of *Ss* of varied musical experience would agree with the small number of musically trained *Ss* employed in these preliminary experiments.

(4) Final experiments. Thirty-six *Ss*, mostly young adults, were used in this experiment. Their musical training varied from about eighteen years for one *S* to none at all for the majority. Only one had had any experience with the organ.

(a) Procedure. The *Ss* were seated toward the front of a moderate-sized lecture-room, near the loudspeaker that was the source of sound. The complex tone

of an organ pipe (either with or without its fundamental) was presented to them alternately with the sound of the oscillator. If the pitch of the oscillator was adjusted to be that of the fundamental of the pipe, they were told that they were listening to combination "A"; if it was adjusted to be that of the octave of the fundamental, they were told that they were listening to combination "B." In each 'trial' the two 'combinations' were presented twice, and each 'pair,' complex tone and oscillator tone, eight times, *i.e.* two 'sets' of four.

Thus 'trial' 1 was as follows: "Combination A:" complex tone (open diapason, *g* 388~, fundamental present)—oscillator, 388~—complex tone—oscillator—complex tone—oscillator—complex tone—oscillator; "Combination B:" complex tone (same as before)—oscillator, 776~—complex tone—oscillator—complex tone—oscillator—complex tone—oscillator; "Combination A" again; "Combination B" again. At the end of the 'trial,' which required about 10 sec. to present, the *S*s were asked to write "A" or "B" in the first blank on the sheets they had been given, according as the pitch resemblance in "A" or "B" seemed the closer. This was explained to them fully and several trials were given before the series was presented. No suggestion was made that either response was 'correct.' They were told that in the event their judgment was "very uncertain," they should place a question mark beside the response.

'Trial' 1 was followed by a second 'trial' with the same pipe, this time with the fundamental eliminated; and every 'pair' of 'trials' included one 'trial' in which the fundamental was present and one in which it was absent, the order depending upon chance. Ten different organ pipes were used, all of the same frequency, *g* 388~. After the ten pipes had been presented once, they were presented a second time in a different order. This made a series of forty 'trials' in all.

The sound level to be employed was measured in several parts of the room and for each tone was adjusted to be at least 70 db. The oscillator tone was made of about equal loudness, so that it was slightly higher in intensity level. After the series of presentations had been completed, an analysis of each of the twenty tones was made. The analyses are given in Table I and include the intensities of any partials that could conceivably contribute to the formation of a difference tone, *i.e.* of any having intensities as much as 10% of that of the strongest partial.

(b) Results. The *S*s' reports were arranged in order upon the basis of the number of times individual *S*s had matched the pitch of the complete complex tone with the pitch of the octave of its fundamental. *S*s who did this for more than half the tones were discarded, as well as a few *S*s who failed to follow the instructions. This selection left a group of twenty-six whose results were used.

Table II gives the results for this group. The first two columns are the percentage of *S*s indicating that the complete tone had the pitch of the octave, the first column being for the first presentation of the pipe and the second column for the second presentation. The last two columns are the percentages of *S*s indicating that the tone with its fundamental missing had the pitch of the octave, the third column being for the first presentation of the pipe and the fourth column for the second presentation.

An inspection of Table II reveals that the *S*s show a great deal of uncertainty about pitch. Even when asked to decide about the pitch of the complete tone, they often prefer the octave to the fundamental as a pitch-match. This seems to be especially true of those pipes in which the fundamental is relatively weak and which have a strong series of upper partials, such as the violin and the kinura. In the case of the tibia, the melodia, the flute, and the trumpet, which have a strong fundamental and a short series of higher partials, the *S*s preferred the pitch of the fundamental. There is, however, a great deal of uncertainty and inconsistency in the responses. For example, the open diapason phonon has a short series of partials and a strong fundamental, but in the first series its octave was preferred by 42% of the *S*s. It will be seen that when

TABLE I

Analysis of Tones Expressed as Percentage of Greatest Amplitude

Tone	Fundamental	Partials									
		1	2	3	4	5	6	7	8	9	10
B.T.L.-5B record organ pipe tone	present	22	10	32	53	100	53	13	*		
	absent	11	11	15	20	100	93	20			
	absent†	15	10	12	17	100	100	58			
Open diapason	present	37	100	12	4						
	absent	0§	100	22	11						
Violin	present	31	28	100	56	78	16	31			
	absent	0	12	83	75	100	20	66			
Tibia	present	100	30	7	3	7	2				
	absent	0	38	88	100	15	12				
Melodia	present	100	90	50	29						
	absent	0	100	62	62						
Kinura	present	73	25	45	36	91	100	50	23	27	9
	absent	0	8	23	26	85	100	47	26	32	11
Reed oboe	present	50	38	100	38	46	5				
	absent	0	22	100	47	67	13				
Flute	present	100	60	4	2						
	absent	0	100	13	11						
Open diapason phonon	present	100	64	7	15	2					
	absent	0	100	21	64	16					
Trumpet	present	100	35	16	2	9	11	3			
	absent	0	100	67	44	67	89	27			
Salicional	present	84	67	89	100	27	22	16	11		
	absent	0	27	68	100	36	34	18	16		

* Other partials less than 10%.
† 2nd also absent.
§ Less than 0.1%.

the fundamental is eliminated, a majority of the *S*s prefer the octave, except in the case of the tibia.

It seemed possible that including in the group so many *S*s who preferred the octave to the fundamental as a pitch-match for the complete tone might obscure any tendency that existed among the more musically sophisticated *S*s to prefer the fundamental frequency to the octave for the pitch of the tone with its fundamental eliminated. To investigate this possibility *E* selected the 10 *S*s who voted the fewest times (not more than four) for the octave as the pitch of the complete tone. The results for these *S*s are presented in Table III.

TABLE II

PERCENTAGE OF 26 Ss SELECTING THE OCTAVE OF THE FUNDAMENTAL

(Responses marked with "?" were counted as one-half in computing percentages.)

Pipe	Complete tone		Fundamental eliminated	
	1st pres.	2nd pres.	1st pres.	2nd pres.
Open diapason	4	16	54	54
Violin	38	58	54	69
Tibia	4	4	38	38
Melodia	20	0	58	46
Kinura	58	20	65	84
Reed oboe	12	42	73	77
Flute	8	12	54	42
Open diapason phonon	42	12	73	88
Trumpet	12	8	69	77
Salicional	31	16	80	69

Table III shows again the tendency the *S*s had to prefer the octave as the pitch-match of the complete tones in the case of those having a weak fundamental and strong upper partials, *e.g.* the violin, the kinura, and the reed oboe. These *S*s were more consistent than the whole group in their responses to the tones with strong fundamental, *i.e.* the tibia, the flute, the trumpet, and to a lesser degree the melodia and the salicional. Their response to the open diapason is not so easy to understand, in view of its weak fundamental. Possibly the fact that the series of partials is short tends to make its pitch seem low. Again, it will be seen that when the fundamental is eliminated, a majority of the *S*s preferred this octave, except again in the case of the tibia.

That musical training and *S*'s certainly about the pitch of tones might be related is conceivable. This relationship is borne out to some extent by the results. The special group of 10 *S*s contained 7 who had had musical training whereas the group of 26 *S*s contained, of course, these 7 and only 5 others who were musically trained. There seemed to be no relation between the amount or kind of training and *S*'s response, except in the case of one *S* who had had considerable experience with the organ, and who was familiar both with the nature of the experiment and with Fletcher's work. His responses were in favor of the pitch of the fundamental in every case where the fundamental was present, and in 15 of the 20 cases where it was absent.

TABLE III

PERCENTAGES OF 10 *S*s SELECTING THE OCTAVE OF THE FUNDAMENTAL

(Responses marked with "?" were counted as one-half in computing percentages.)

Pipe	Complete tone		Fundamental eliminated	
	1st pres.	2nd pres.	1st pres.	2nd pres.
Open diapason	0	0	45	60
Violin	20	50	40	75
Tibia	0	0	40	40
Melodia	15	0	65	60
Kinura	40	15	40	80
Reed oboe	10	25	70	60
Flute	0	0	55	60
Open diapason phonon	10	5	95	95
Trumpet	0	0	95	80
Salicional	15	0	50	80

DISCUSSION

That these results are at variance with Fletcher's is evident. Under conditions similar to his, the present *S*s failed to report unchanged pitch when the fundamental was eliminated from the tones of organ pipes. The nearest approach to Fletcher's results occurred in the case of the tibia, and even here approximately 40% of the *S*s preferred the octave as the pitch-match.

Several explanations of the discrepancy seem possible. One is that the *S*s in the two sets of experiments were differently trained. Most people, even the musically educated, are unfamiliar with pure tones, and when asked to compare the pitch of a pure tone with that of a complex tone, may base their judgments on timbre rather than on pitch, and so choose that frequency of the pure tone which is most like the complex tone in timbre. Something of this sort may have been happening in the present experiment when the *S*s chose the octave as the pitch-match of the complete tone of some of the pipes. This possibility was further borne out in some early experiments where the *S*s were asked to match the pitch of the complex tone devoid of its fundamental with the tone of a harmonium. Then the *S*s tended more often than in the present experiment to choose the fundamental.

On the other hand, it may be that pitch is a significant attribute of tone only when the tone has a fundamental strong enough to stand out. Ekdahl and Boring,[6] found that, when *Ss* were asked to judge the pitch of masses of sound, they chose as the pitch of the mass a frequency which was an average of the pitches of the components of the mass. It may be that the present *Ss* were making a judgment of that sort. A person listening to the complex tone of such a pipe as the kinura, especially when its fundamental is eliminated, is inclined to feel that the tone is not 'musical,' that it is more like a chord or noise in quality than like a single tone. Fletcher's *Ss* reported the same experience when the incomplete tone to which they listened was not strong enough to generate an appreciable difference-tone-fundamental, and also when the difference-tone was of a different frequency from the quotient of the partials.

A third possibility, not inconsistent with either of the preceding, is that in the present experiment the ear-generated difference-tone was not strong enough, even though in some cases an intensity of 100 db. was used, to give the tone a definite pitch. This is by no means unlikely. Let us consider the circumstances under which such difference tones arise.

Let us assume that the response of the ear is non-linear and that we may express its displacement, y, as a function of the applied force x, in the form of a power series $y = ax + bx^2 + cx^3 + \ldots$ For purposes of the present discussion we may neglect the terms of higher than the second power, although Newman, Stevens, and Davis[7] and Wever and Bray,[8] have shown that the effects of terms as high as the sixth are detectible in the response of the cat's ear. Now if we apply to the ear three tones of frequency f_1, f_2, and f_3, so that their force amplitudes are equal, and, for convenience, equal to unity, we may write $x = \sin(2\pi f_1 t) + \sin(2\pi f_2 t + \varphi_2) + \sin(2\pi f_3 t + \varphi_3)$, where φ_2 and φ_3 are the differences in phase respectively between 2 and 1, and 3 and 1. If we now substitute this value of x in the first equation and reduce all of the terms to first powers of sines and cosines, we arrive at an expression for y involving such frequencies as f_1, f_2, f_3, $(f_2 - f_1)$, $(f_3 - f_2)$, $(f_1 + f_3)$,

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[6] A. G. Ekdahl and E. G. Boring, The pitch of tonal masses, this JOURNAL, 46, 1934, 452-455.

[7] E. B. Newman, S. S. Stevens, and H. Davis, Factors in the production of aural harmonics and combination tones, *J. Acous. Soc. Amer.*, 9, 1937, 107-118.

[8] E. G. Wever and C. W. Bray, The nature of acoustic response: The relation between sound intensity and the magnitude of responses in the cochlea, *J. Exper. Psychol.*, 19, 1936, 129-143; Distortion in the ear as shown by the electrical responses of the cochlea, *J. Acous. Soc. Amer.*, 9, 1938, 227-233.

etc. If we consider only the case where f_1, f_2, and f_3 are multiples of some lower frequency f (*e.g.* $f_1 = 2f$, $f_2 = 3f$, and $f_3 = 4f$), and concern ourselves only with those terms that will yield f, we shall be interested in $(f_2 - f_1)$ and $(f_3 - f_2)$. They will appear in the final result as bCos $(2\pi (f_2 - f_1)t + \varphi_2)$ + bCos $(2\pi(f_3 - f_2)t + \varphi_3 - \varphi_2)$, and, since $f_2 - f_1 = f_3 - f_2 = f$, we may write this as 2bCos $(\varphi_2 - \frac{1}{2}\varphi_3)$ Cos $(2\pi ft + \varphi_3)$.

In this last expression the phase angles φ_2 and φ_3 enter in such a way as to modify the amplitude of the difference-tone, and if $\varphi_2 - \frac{1}{2}\varphi_3 = \pi/2$, the difference-tone due to the square-term disappears. If $\varphi_2 - \frac{1}{2}\varphi_3$ has any value other than zero, this intensity of the difference-tone will be less than maximal. Similar relations, though not so simple, exist for the difference-tones due to the higher-order terms. Thus we see that the phase relations existing among the various partials of a complex tone may play a considerable part in determining the intensity of any difference-tone that may be established.

This fact may be of considerable importance to the problem of pitch. In the tone of musical instruments, the phase relations among the partials will depend, among other things, upon the location of the hearer, or, as in the present experiments, of the microphone, with reference to the instrument. In the case of the synthetic tone employed by Fletcher in many of his experiments, it is likely that the phase angles were all zero, and consequently the difference-tone maximal in intensity.

A further, and probably still more important factor affecting the nature of the difference-tones is the frequency relations among the partials. We have so far assumed, and it seems to be rather generally accepted as fact by many writers on hearing, that the frequencies of the partials of a complex tone are integral multiples of the frequency of the fundamental. This is by no means the case in the tones of most, probably all, musical instruments.[9] In stringed instruments, unless the bridges are perfectly rigid and the string perfectly homogeneous, the partials will deviate slightly from a harmonic series, and in the case of some organ pipes the series may be far from a harmonic one. If we examine the effect of this fact upon our results above, we see that it will be to make $(f_2 - f_1)$ different from $(f_3 - f_2)$. Thus we will have two difference-tones differing slightly in frequency and beating together, with a consequent reduction in the contribution they make to the pitch-definiteness of the composite tone. Here again Fletcher's complex-tone generator is an exception. The partials it produces are integral multiples of its fundamental frequency and so would generate in the ear a difference-tone (of the order we have been considering) having only a single frequency.

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[9] See, for example, any treatise on sound, such as P. M. Morse, *Vibration and Sound,* 1936, chaps. 3 and 6.

The most plausible explanation of the difference between the results of the present series of experiments and Fletcher's seems to the writer to involve all of the above considerations; namely, that the average *S*, even the average musically trained *S*, finds difficulty in comparing the pitch of a complex tone with that of a pure one, especially if the complex tone has a weak fundamental and a series of strong higher partials; that practice *may* improve this sort of judgment; that, in the present series of experiments, the difference-tones generated in the ear were, for various physical reasons, weaker than those in many of Fletcher's experiments; and, finally, that Fletcher's tone generator (with which his most striking results were obtained) was especially apt to produce the ear-generated fundamental frequency.

CONCLUSIONS

(1) *S*s attempting to identify the pitch of organ-pipe tones lacking the fundamental component find great difficulty in doing so, and, in most cases, choose the octave of the fundamental-frequency as the pitch of the tone. Exceptions may occur in the case of tones whose partials are so related as to favor the generation of strong difference-tones in the ear.

(2) The foregoing experiments failed to provide any evidence in favor of the hypothesis that pitch is determined by the nature of the pattern of stimulation of the basilar membrane, or that the phenomenon of pitch-stability is analogous to pattern-completion in vision.

MULTIPLE FACTORIAL ANALYSIS OF FINE MOTOR SKILLS

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The experiment to be reported was undertaken as an extension and combination of numerous earlier experiments on the nature of motor abilities.[1] It has been shown that motor abilities are only slightly though positively correlated, on the average, but questions have been raised as to the adequacy of the experimental conditions under which such results have been obtained, and as to the reliabilities of the tests employed. Our first procedure was simply to improve and lengthen these tests until such difficulties were overcome. Having verified, by our zero-order correlations in Table II, the absence of any one general factor of motor ability, our major problem bcame that of locating and identifying any possible group factors among the performances.

This study brought together for the first time tests of simple reaction-time, simple tapping rate, complex tapping rate, serial high speed co-ordinations, postural steadiness, and form board and assembly tests of mechanical abilities.

Description of Tests

In the following descriptions of the tests employed we shall first describe the characteristics of each group of tests and then the distinguishing features of each specific test. To avoid undue repetition we shall present the data as briefly as possible in a standard order, giving the number and the name of the tests, the nature of the performance, the type of scoring, the length of each trial, the number of trials per cycle, and the number of cycles.

Group A. Simple reaction-times. Group A includes Tests 1–4. In all of these tests a new silent chronoscope was employed which measured reactions in units of 1/100 sec.[2] Fifty trials were given for each test on each of two cycles.

Test 1. Auditory simple reaction. *S* depresses a telegraph key 1 mm. in response to the click of a telegraph sounder.

Test 2. Visual simple reaction. Same response to flash of light.

Test 3. Auditory jump reaction. *S* moves hand 6 in. horizontally to depress key in response to click.

Test 4. Visual jump reaction. Same movement as Test 3, to light flash.

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[1] R. H. Seashore (An experimental and theoretical analysis of fine motor skills, this Journal, 53, 1940, 86-98), has reviewed the history, theoretical bases, and minor experiments on this problem.

[2] This chronoscope was devised by R. H. and S. H. Seashore.

Group B. Tapping and related actions. Group B includes Tests 5–13, all of which have been described by McCollom.[3] All of these tests, except Test 13, employed a highly accurate timing device to record the number of taps in 5-sec. intervals, as registered by a Stoelting Hollerith counter which would record up to 12 taps per sec.

Test 5. Simple vertical tapping. *S* operates a vertical telegraph key having an excursion of 1 mm. and a spring resistance of 100 gm., by grasping the key, and by using free arm movement. Five trials were given for each of 3 cycles.

Test 6. Same as Test 5, but given two weeks earlier.

Test 7. Simple horizontal tapping. Same as Tests 5 and 6 except movement is horizontal instead of vertical.

Test 8. One-plate stylus tapping. *S* taps pencil shaped stylus vertically on metal plate 2½ in. sq. Three trials each on 3 cycles for Tests 8, 9, and 10.

Test 9. Two-plate stylus tapping. *S* taps same stylus alternately on two plates; 4½ in. from center to center.

Test 10. Three-plate stylus tapping. *S* taps same stylus on three plates in triangular pattern.

Test 11. Oscillometer, two finger. *S* grasps a fixed vertical rod with thumb, 4th and 5th fingers, using 2nd and 3rd free fingers to move a horizontal lever which makes and breaks an electrical contact to the counter for each change in direction of movement. Amplitude uncontrolled. Three trials for each of 3 cycles for Tests 11 and 12.

Test 12. Oscillometer, arm. Same as Test 11, except whole arm is free to oscillate.

Test 13. Spool-peg transfer. *S* picks up spool from one row of pegs, transfers it to corresponding position in second row. Movements are designed to involve simple grasping and transport movements equivalent to Tests 11 and 9. Twelve pegs per row, averaging approximately 12 sec. per trial, 3 trials for each of 3 cycles.

Group C. Stanford motor skills unit. Group C, besides the following tests, includes Test 5.[4]

Test 14. Spool-packing, Seashore-Tinker apparatus. *S* grasps one spool in each hand, packing in pairs in a tray. The number of trays packed in 1½ min. is taken as *S*'s record. Two trials for each of 3 cycles.

Test 15. Speed drill (Miles). *S* turns Goodell-Pratt hand drill with arm at right angle to wheel—a rotary wrist flipping movement. Veeder counter measures the number of revolutions in 10 sec. Three trials in each of 3 cycles.

Test 16. Serial discrimination. Numbers 1 to 4 appear in random order at window, *S* presses corresponding one of four keys with fingers of right hand. The number of correct discriminations is indicated in high-speed escapement apparatus during 2-min. trials. Two trials per each of 3 cycles.

Test 17. Pursuit-rotor (Koerth). A brass target ½ in. in diam. is embedded near the edge of a revolving fiber disk on phonograph turntable. *S* attempts to keep a hinged (pressureless) pointer on the target moving at 60 r.p.m. Score is the number of twentieths of a revolution that *S* holds the pointer on the target, as measured by electrical commutator and counter. Twenty revolutions per trial; 10 trials per each of 3 cycles.

Group D. Postural sway. Group D includes two tests, Tests 18 and 19.

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[3] I. N. McCollom, An analysis of factors determining individual differences in speed of simple repetitive motions of the right arm (unpublished Master's thesis, Univ. of Oregon Library, 1932).

[4] R. H. Seashore, Stanford motor skills unit, *Psychol. Monog.,* 39, 1928, (no. 178), 51-66.

TABLE I

Zero-Order Correlation Among the Various Tests

No.	Test	Test (number)																			
		2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21
1.	Aud. simple reac. time	.55	.44	.51	.09	.25	.28	.05	—.01	.05	.19	.15	—.14	—.15	.23	.03	—.60	—.18	—.24	—.10	—.03
2.	Vis. simple reac. time		.49	.87	.18	.36	.44	.22	—.30	.01	.23	.27	.10	—.21	.10	.04	—.05	.06	—.02	.04	.10
3.	Aud. jump reac. time			.78	.36	.41	.36	.49	.17	.02	.18	.26	.44	.06	.24	.36	.14	—.04	.29	.10	—.12
4.	Vis. jump reac. time				.29	.38	.44	.44	.21	.18	.29	.34	.22	.12	.18	.16	.14	—.10	.18	.11	—.04
5.	Vert. tel. key tap					.89	.74	.59	.36	.25	.61	.65	.35	.02	—.33	.18	.21	.31	—.13	—.08	—.10
6.	Same (2 wk. earlier)						.93	.79	.06	.33	.71	.87	.32	—.02	.18	.23	—.25	—.15	—.03	—.14	—.54
7.	Hor. tel. key tap							.63	.40	.23	.72	.66	.31	.20	.23	.17	.30	—.18	—.17	—.21	—.30
8.	One-plate stylus tap								.49	.31	.59	.39	.44	.12	.28	.47	.03	—.15	.04	—.19	—.29
9.	Two-plate stylus tap									.79	.45	.51	.66	.17	.32	.34	.43	—.08	.06	.27	—.07
10.	Three-plate stylus tap										.44	.39	.65	.26	.15	.77	.24	.27	—.03	.22	—.21
11.	Oscillometer, 2-finger											.78	.13	.03	.10	—.10	—.01	.06	.22	—.21	—.23
12.	Oscillometer, arm												.44	—.18	.22	.20	.11	—.14	—.21	—.16	—.08
13.	Spool-peg transfer													.19	.25	.30	.23	—.28	—.16	.31	.14
14.	Spool-packing														.10	.14	.04	—.21	—.01	.32	—.21
15.	Speed drill															.09	.10	—.24	—.27	—.12	—.13
16.	Serial discrimeter																.17	—.25	.08	.16	.00
17.	Pursuit-rotor																	—.62	—.37	.42	.41
18.	Ataxiameter, stand																		.57	—.28	—.20
19.	Ataxiameter, sit.																			—.03	—.16
20.	Spatial relations																				.51
21.	Assembly																				—

Test 18. Ataxiameter,[5] standing. *S* stands erect, heels together, toes at angle of 45°, eyes closed, as still as possible. Light weight ratchet counters measure total amount of movement in millimeters in front, back, right, and left directions. Trials 2 min. each, 4 trials per cycle, one cycle before and one after Stanford motor skills unit tests of Group C.

TABLE II

INTERCORRELATIONS FOR MOTOR SKILLS TESTS OBTAINED IN THIS AND OTHER STUDIES

(Unmarked coefficients are for the present study; others obtained from studies indicated below.)

Test	Test						
	16	5 & 6	14	15	18	10	1–2
17 Pursuit-rotor	.25 (2) .17 (3) .29 (4) .17	.31 (2) .01 (3) .28 (4) .25 (5) .21 −.25	.26 (2) .03 (3) .44 (4) .32 (5) .13 (6) .04	.38 (6) .33 (2) .16 (3) .40 (4) .10	.12 (2) .62 .37	.06 (6) .25	.15 (1) .14 .13 −.05 .62
16 Serial Discrimination		.03 (3) .19 (4) .18	.32 (2) .29 (3) .34 (4) .14	.08 (2) .24 (3) .06 (4) .09	−.15 (2) .24 −.08	.77	.36 .16 .03 .04
5 Telegraph-key tapping			.36 (3) .35 (4) .40 (5) .02	.48 (3) .06 (4) −.35			.10 (1) .35 .29 .09 .18
14 Spool-packing				.34 (6) .38 (2) .29 (3) .43 (4) .10	.15 (2) .20 .01	−.38 (6)	.06 .12 −.15 −.21
15 Speed drill					.12 (2) .24 .27	.52(6) .26	.24 .18 .23 .10

(1) R. H. Seashore, Studies in motor rhythm, *Psychol. Monog.*, 36, 1926, (no. 167) 142–189 (44 college men).

(2) R. H. Seashore, Individual differences in motor skills, *J. Gen. Psychol.*, 3, 1930, 38–65 (50 university men; 35 undergrad., 15 grad.).

(3) S. H. Seashore, The aptitude hypothesis in motor skills, *J. Exper. Psychol.*, 14, 1932, 555–561 (50 knitting mill workers, ages 19–26 yr.).

(4) R. Y. Walker and R. D. Adams, Motor skills: The validity of serial motor tests for predicting typewriting proficiency, *J. Gen. Psychol.*, 11, 1934, 173–186 (70 high school boys, ages 15–18 yr.).

(5) C. E. Buxton and L. G. Humphreys, The effect of practice upon intercorrelations in motor skills, *Science*, 81, 1935, (no. 2105), 441–442 (50 college men, 4 years range in class).

(6) C. E. Buxton, The application of multiple factorial methods to the study of motor abilities, *Psychomet.*, 3, 1938, 85–93 (76 high school boys, ages 12–16).

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[5] W. R. Miles, Static equilibrium as a useful test of motor control, *J. Indus. Hyg.*, 3, 1922, 316-331.

Test 19. Ataxiameter, sitting. Same as Test 18 except *S* sits on backless stool.

Group E. Minnesota mechanical ability tests.[6] Group E includes Tests 20 and 21.

Test 20. Form Boards A, B, C (spatial relations). *S* fits diverse shaped wooden blocks into holes by standard method.

Test 21. Assembly A, B, C. *S* assembles simple mechanical gadgets in fixed times. Score is number assembled under standard method.

Fifty men, working as a part of a regular psychology laboratory course, were tested individually with standard instructions, demonstrations by the experimenter, and criticisms of their techniques as previously used by Seashore and others.

The cycle plan was used, whereby the *Ss* worked approximately 5 minutes on each test in rotation, the testing being distributed over 3 days. Scoring was done by automatic devices, with immediate knowledge of results. The *Ss'* knowledge of their scores, together with the intrinsically interesting character of the tests, provided adequate and reasonably constant motivation. This motivation and the careful control of the testing produced, in the main, highly reliable tests (see Table III). In Table I we have reproduced our own table of zero-order correlations so that later workers may use the data in different ways and compare the r of their studies with previous studies as we have done in Table II.

TABLE III

Correlations R_{pq} Between Primary Axes in the Experimental Population

(The residual plane VI was set orthogonal to the other planes in determining these correlations.)

	I	II	III	IV	V
I	1.00	.27	−.04	−.28	.30
II	.22	1.00	.21	−.02	.41
III	−.04	.21	1.00	−.04	.08
IV	−.28	−.02	−.04	1.00	.19
V	.30	.41	.08	.19	1.00

In this attempt to discover any statistically significant grouping of tests, Thurstone's center of gravity method of analysis was employed. Orthogonal axes were used originally, and rotation was done by the standard technique.[7]

Table III gives the correlations between the primary axes, but the few correlations above three P.E. of their values do not suggest any readily apparent qualitative basis. These values must be verified on larger test populations before they may be assumed to have any definite significance.

Since the battery included groups of most of the known varieties of tests of fine motor skills, the data seemed to afford a legitimate opportunity for the identification of possible group factors by the Thurstone method.

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[6] D. G. Paterson, L. D. Anderson, H. A. Toops, and E. Heidbreder, *Minnesota Mechanical Ability Tests*, 1930, 1-586.

[7] L. L. Thurstone, *The Vectors of Mind*, 1935, 1-266. The writers are greatly indebted to Mr. Harold Bechtoldt who suggested and kindly provided a revised factorial matrix table based upon the assumption of oblique rather than orthogonal axes. This change cleared up a number of minor anomalies which had arisen from the orthogonal treatment and indicated that the data did show a better fit to the assumption of oblique axes. Mr. Bechtoldt also provided Table III.

RESULTS AND CONCLUSIONS

Table IV shows that 6 factors were necessary to account for the zero-order table of correlations. For convenience in inspection of this table loadings of 0.40 or higher were arbitrarily considered as being of probable significance and are printed in boldface. Some of the loadings are starred to indicate that in this preliminary survey they disagree somewhat with our qualitative identifications.

From previous studies we should expect that a group-factor among motor tests would appear in performances involving a *similar pattern of action,* and that loadings would vary only slightly according to the particular sense field or musculature employed.

TABLE IV

ROTATED FACTORIAL MATRIX, GIVING TEST PROJECTIONS ON THE SIX OBLIQUE AXES
(Figures in boldface are probably significant.)

Test	‡$r_{\frac{1}{2}\frac{1}{2}}$	I	II	III	IV	V	VI	h^2†	h^2_{assum}
1 Aud. simple reac. time	.63	**.65**	.01	.03	−.03	−.06	−.10	.54	.60
2 Vis. simple reac. time	.90	**.64**	.15	.08	−.06	.15	.16	.82	.87
3 Aud. jump reac. time	.74	**.48**	.20	.47	−.03	−.09	−.05	.66	.78
4 Vis. jump reac. time	.86	**.75**	.32	.09	.02	−.02	.01	.95	.87
5 Vert. tel. key tap.	.92	.03	**.89**	.04	.02	−.51*	−.03	.91	.89
6 Same (2 wk. earlier)	.85	−.10	**.86**	−.10	−.08	−.05	−.39	1.09*	.93
7 Hor. tel. key tap.	.95	**.56**	**.51**	.04	**.74***	**−.67***	−.10	.93	.93
8 One-plate stylus tap.	.89	.02	.22	**.40**	.12	.38	.18	.64	.79
9 Two-plate stylus tap.	.90	−.04	.31	**.64**	.29	−.06	.11	.73	.79
10 Three-plate stylus tap.	.87	−.10	.38	**.69**	−.10	−.06	−.10	.96	.79
11 Oscillometer, 2 finger	.92	.03	**.70**	−.03	.25	−.01	.05	.68	.78
12 Oscillometer, arm	.85	.03	**.62**	.21	**.45**	−.02	.30	.83	.87
13 Spool-peg transfer	.89	.10	.37	**.43**	.32	.06	.11	.61	.66
14 Spool-packing	.90	.01	.02	−.03	.12	.13	**−.56**	.39	.32
15 Speed drill	.81	**.54***	−.03	.26	.34	−.01	.20	.44	.33
16 Serial discrimeter	.93	.07	−.04	**.69***	−.06	+.06	.00	.58	.77
17 Pursuit-rotor	.92	−.02	.09	.19	**.44***	**.44***	.23	.74	.62
18 Ataxiameter, stand	.80	−.04	.09	−.09	**.75**	.05	−.08	.75	.62
19 Ataxiameter, sit	.89	.27	−.05	−.02	**.67**	−.06	−.03	.48	.57
20 Spatial relations	.82	.02	.03	.04	.01	**.58**	−.11	.53	.51
21 Assembly	.52	.00	−.08	−.01	−.03	**.53**	.37	.52	.54

I=Speed of single reaction; II=Finger-hand speed in restricted oscillatory movements; III=Forearm and hand speed in oscillatory movements of moderate extent; IV=Steadiness; V=Manipulation of spatial relations; VI=Unidentifiable or residual factors.

* Loadings seemingly inconsistent with identifications.

† The values in h^2 and h^2 assumed are taken from the orthogonal solution.

‡ The reliability coefficients, or correlation between the last two cycles of each test, are uncorrected for length.

Thus we find that Factor I has its most significant loadings in the four simple reaction-times (Tests 1–4), whether the stimulus was visual or auditory, and for both a simple one millimeter press reaction of the finger and hand, or a much longer (6 in.) jump-and-press reaction of the hand and forearm. It is tentatively designated "speed of single reaction." The only other significant loadings in this column are for Test 7, which behaves anomalously in relation to several other axes, and the speed drill (variable 15), which is a test of ability to move the hand continuously in a circle, and, therefore, does not seem to resemble the simple reaction-time tests. Only a retest can check the significance of this latter relationship. Each of our factors shows one or more such anomalies, which may possibly be due to statistical fluctuations from our relatively small sample of 50 cases. In a number of these instances the loadings are definitely known not to be representative of the correlational findings from previous experiments with the same tests, as may be seen in Table II—a summary of intercorrelations reported in previous studies between various pairs of tests which also appear in our battery. It is hoped that such apparent inconsistencies in the present study may be cleared up in future studies planned specifically for this purpose.

In Factor II the significant loadings fall on five tests. Tests 5 and 6, tapping a telegraph key vertically (the same test was given two weeks in succession, which accounts for two entries in the factor table); Test 7, tapping a telegraph key horizontally; Test 11, the two-finger oscillometer; and Test 12, the arm and hand oscillometer. All involve short oscillating movements in one plane only. Test 8, one plate stylus-tapping, fails to receive as high loadings as would be expected in line with our tentative identification of this factor, which is: "finger, hand, and forearm speed in restricted oscillatory movement." Note that the repetitive action tests which call for *wider* movement in two planes do not have high loadings in this factor.

Factor III shows relatively significant loadings on all three stylus-tapping tests (variables 8, 9, 10), with the two- and three-plate stylus-tapping tests receiving definitely heavier loadings. The spool-peg transfer test (Test 13), designed to involve the same distance and accuracy of movements as the two- and three-plate tests, also receives a significant weight. The ability called for seems to be one for "forearm and hand speed in oscillatory movements of moderate extent." These movements are nearly all in two planes, as contrasted with the single plane in Factor II. Two anomalous loadings are found, a moderately large one for the auditory jump reaction-time (Test 3) and a higher one for serial discriminative action of the four fingers (Test 16). The possibility seems to be rather great that the serial discrimination test would, in a different battery, have its most significant loadings in some other factor, but this hypothesis must be checked by further experimentation.

Factor IV is best identified from the two high loadings on the standing and sitting postural sway tests (Tests 18 and 19) and, on the basis of previous experiments, by Seashore and Adams,[8] Buxton and Humphreys,[9] and Buxton,[10] seems to be sampling a group factor which we have found to include a number of performances emphasizing steadiness or precision. It may also be related to a relaxed condition in taking the tests. The weighting of the pursuit-test (Test 17) seems logically to be acceptable but is negated by the correlational results of two previous studies (cf. Table II). Test 7, horizontal telegraph key tapping, is again an anomaly.

In Factor V the weightings of the Minnesota spatial relations form-board and assembly tests, together with the loading of the pursuit-rotor (Tests 20, 21, and 17 respectively), suggest that this factor should be designated "skill in manipulating spatial relations." This rather complex identification seems necessary because of the possibility that future experiments will show this to be largely a space perception factor. The two negative loadings, of the vertical and horizontal telegraph key tapping tests (Tests 5 and 7) probably have to be explained again in terms of the error in our relatively small group of *Ss*, since no other tapping tests receive significant weights. (So far as we can ascertain by checks, the discrepancies between the loadings of the various telegraph-key tapping tests are not due to computational error.)

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[8] R. H. Seashore and R. D. Adams, The measurement of steadiness: A new apparatus and results on marksmanship, *Science,* 78, 1933, (no. 2022), 285.

[9] C. E. Buxton and L. G. Humphreys, The effect of practice upon intercorrelations in motor skills, *Science,* 81, 1935, (no. 2105), 441-442.

[10] C. E. Buxton, The application of multiple factorial methods to the study of motor abilities, *Psychometrika,* 3, 1938, 85-93.

The discussions contained in the papers of this series have shown that a modification of the type of interpretation given to motor factors by Buxton may be necessary. The shift probably should be one from an anatomical emphasis to a functional emphasis in the qualitative identification of factors.

Factor VI seems to be a residual for this battery of tests.

SUMMARY

The factors isolated in this study are found to correspond more to *qualitative similarities in the pattern of action,* including perceptual activity, involved in various tests rather than to anatomical units such as the dominant sense-field or even the musculature employed. In keeping with previous experiments as summarized by Seashore,[11] we find that shifting from one musculature to another makes very little relative difference in performance, while keeping the musculature constant and shifting to another pattern of action makes a great deal of difference in the correlations. With the exception of Factor IV (steadiness), our factors include such narrow groupings of performances as to make application to the prediction of complex practical skills a hazardous undertaking, a result which is corroborated by our industrial experiments to date. The more exact boundaries of the factors which we have located can only be determined by the verification of the doubtful loadings in this experiment, plus an extension of the battery of tests into those which grade off qualitatively from those now receiving significant weights.

In view of the findings of this and previous studies as to the significance of patterns of action, rather than any basic biological constants of sense field or musculatures, the next logical step in identifying these statistically located factors would seem to be the employment of the techniques of photographic motion study and simple introspection by the *Ss*, rather than the all-too-frequent second hand analysis or vicarious introspections of the experimenter.

We need not deny the existence of individual differences in biological factors, such as speed of muscle contraction, but, as might be inferred

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[11] R. H. Seashore, An experimental and theoretical analysis of fine motor skills, this JOURNAL, 53, 1940, 86-98.

from the study of Bryan and Harter on higher units in telegraphy,[12] and the similar studies of Book on typing,[13] *any one physiological limit is usually important only for a given work method and changing to another work method may either partially or entirely overcome this limitation.* Further factor analyses in the motor field will need perhaps more to be supplemented by qualitative techniques, such as motion study of these methods, than by improvement of statistical techniques.

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[12] W. L. Bryan and Noble Harter, Studies in the telegraphic language: The acquisition of a hierarchy of habits, *Psychol. Rev.,* 6, 1899, 374-385.

[13] W. F. Book, *Learning to Typewrite,* 1925, 120-122.

OCULAR PATTERNS AND THEIR PSYCHOLOGICAL IMPLICATIONS

By HERMAN F. BRANDT, Drake University

A majority of the studies of eye movement have been made in the field of reading. The reasons for this are: (1) the importance attached to the diagnosis of reading; (2) a lack of adequate laboratory equipment for other types of research; and (3) the inability to see the possibilities of studying the learning process by means of the analysis of ocular patterns.

Cameras used in photographing eye movements during reading have for the most part recorded eye movements in the horizontal plane only. While this has been satisfactory as a means of revealing certain phases of the reading process, problematic materials, advertising and pictorial copy of necessity require records of the horizontal and vertical movement at all times. In order to obtain the necessary patterns for this study a bidimensional camera was employed.[1]

Among the studies of eye movements (other than those resulting while the *S* was reading) are those made while *S* was looking at pictures,[2] studying romance languages,[3] learning algebraic and chemical formulas,[4] reading music,[5] and taking examinations.[6] In every case, new information has been revealed leading to the general conclusion that eye movements have a functional relation to the central processes of the learner.

Dallenbach in his research on position and intensity as determinants of attention, concludes that spatial position was an important factor and that other variables such as the relative intensity of light was affected by it.[7] In his experiments, *O*, at a given signal, fixated a spot of light.[8] Then two diametrically opposed areas of equal size and of equal distance from the fixation-point were simultaneously exposed. *O* reported which was the clearer. The results obtained indicated that the positions to the left and above have a significant attentional advantage.

In a later article, Burke and Dallenbach, using the same apparatus, substantiate

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* Accepted for publication May 3, 1939.

[1] H. F. Brandt, A bidimensional eye-movement camera, this JOURNAL, 49, 1937, 666-670.

[2] C. T. Buswell, *How People Look at Pictures,* 1935, 1-198.

[3] W. R. Miles and E. Shen, Photographic recording of eye movements in reading of Chinese in vertical and horizontal axes, *J. Exper. Psychol.,* 8, 1925, 344-362.

[4] M. A. Tinker, A photographic study of eye movements in reading formulas, *Genet. Psychol. Monog.,* 3, 1928, 65-182.

[5] O. Jacobson, An analysis of eye movements in reading music, Univ. Chicago, 1926 (unpublished thesis).

[6] Arden Frandsen, An eye movement study of objective examination questions, *Genet. Psychol. Monog.* 16, 1934, 85-138.

[7] K. M. Dallenbach, Position *vs.* intensity as a determinant of clearness, this JOURNAL, 34, 1923, 282-286.

[8] Dallenbach, Some new apparatus, *ibid.,* 34, 1923, 90-95.

their previous findings and add that position seems to be a matter of left and right handedness; and that the position to the left and above the center has a distinct attention value for the right handed while the position to the right and below has an attention advantage for the left handed.[9] A neurological explanation of right and left dominance was offered by Dallenbach.[10] Papez and Cohu accepted it. They state that "neural excitations aroused in the right retinas of each eye—the areas stimulated by objects occupying positions to the left of fixation—center in the right hemisphere of the brain where they, because right hand people are left brained, are comparatively free from interference and inhibition by other concomitant processes. As a consequence, they possess on the side of consciousness a high degree of attensity, *i.e.* clearness or vividness. Everything else being equal, objects arousing processes in the right brain would enjoy therefore, an attentional advantage over all others."[11]

Nixon,[12] employing the visual method for measuring the attention-value of advertisements, tested his *Ss* by having them look at a pair of advertisements for 30 sec. The direction of the first eye movements and the relative amount of time spent in the right and left position, were observed by *E* from behind a screen. A recall test was used to check the results. Nixon concludes on the basis of his experiment that there is a marked tendency for the first fixation to fall on the left hand page. This tendency, he found, falls off rapidly with the passing of time, but a superior memory value is apparent for the page receiving the greatest primary attention.

Guilford and Hackman developed another method of measuring the attention-value of stimuli.[13] Their method was more objective in that they employed a camera to record *S*'s eye movements and insisted on immediate recall as over against Nixon's delayed response. The stimulus-material for their experiment consisted of lantern slides, made up by typing letters or gluing geometric forms on radio mats. Various slides were used representing accepted conditions or factors of attention such as: position, isolation, size, and novelty. The above slides were exposed in a tachistoscopic arrangement by means of a projection lantern and recorded by an eye movement camera. The direction of the first eye movement was noted and also the time spent to the right and to the left of the fixation-point. In another series of slides, eye movements were recorded while *S* looked at the exposure-card for 10 sec. They draw the following conclusions: (1) the relative time spent in looking at the right or left is about the same; (2) the length of time spent on right or left is not a good indicator of the attention-value of the side of the field containing the factor of attention; (3) the direction of the first eye movement rather than the length of the fixation is a somewhat better indicator that there is an attention gaining factor present; and (4) a higher correlation exists between eye movements and cognition than between eye movements and attensity.

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[9] Ruth Burke and K. M. Dallenbach, Position *vs.* intensity as a determinant of attention of left handed observers, *ibid.*, 35, 1924, 267-268.

[10] Dallenbach, *op. cit.*, 285 f.

[11] H. A. Cohu and J. W. Papez, A comparative study of the visuosensory or striate area in the two hemispheres of the human brain, *Amer. J. Phys. Anthrop.*, 14, 1930, 413 f.

[12] H. K. Nixon, Attention and interest in advertising, *Arch. Psychol.*, 11, 1924, (no. 72), 1-68.

[13] J. P. Guilford and H. B. Hackman, A study of visual fixation-method of measuring attention value, *J. Appl. Psychol.*, 20, 1936, 44-59.

PURPOSE

The purpose of the present study is to analyze ocular patterns while the *S*s are observing a symmetrical field. It is an attempt to discover characteristic eye movements and their psychological implications. In seeking an answer to these objectives, four problems present themselves. (1) Has the eye a preference for the right or left side of the page? (2) Is the upper or lower half of the page preferred? (3) Does the eye prefer horizontal or vertical excursions? (4) Is the excursion distance greater in the horizontal or vertical plane?

This study also endeavors to show that these ocular patterns reveal certain physiological and psychological implications.

METHOD AND PROCEDURE

Subjects and instruction. The *S*s, 50 college students (29 men and 21 women), were chosen at random. Preceding the presentation of the exposure-material, every *S* was instructed regarding his task. He was told to observe the exposure-card,[14] to study the pattern as he would later be called upon to reproduce it.[15] He was not, however, informed of the length of the exposure-time.

Exposure-card. The exposure-material, a white card cross-ruled into 25 equal areas each 2 in. sq., is shown in Fig. 1. Every cross-ruled division contained from 1 to 9 small black squares each ¼ in. sq. These were symmetrically arranged around the center area which contained one black square. Proceeding outward from the center area the number of small black squares increased uniformly in every direction. It is evident that no matter which area is first observed, another area containing the same number and arrangement of black squares may be found in another position. There should be no pattern-preference in such a uniform field.[16]

Fixation: (a) Duration. After being developed, each film with the ocular movements for that particular person was placed on a calibrated, illuminated, opal flash glass for the purpose of identifying and coding each fixation. The duration of each fixation was determined by measuring the distance the film had traveled.[17]

(b) Location. After all fixations of both eyes had been identified and coded, the film was placed in a high-power stereopticon and projected on a field calibrated to locate each fixation on the card originally observed. The calibration in both horizontal and vertical dimensions provided an area on which every point could be designated. If, for example, the fixation on the record fell at a point 75% of the horizontal distance from the left side and 25% of the vertical distance from the base, the resultant would be a point in the lower right hand corner where the eye fixated at

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[14] Every *S* was shown the exposure-card for 20 sec.

[15] Immediately after the exposure, *S* was given a blank duplicate of the exposure-card with the instruction that he was to reproduce the original pattern.

[16] The size of the original exposure card as observed by the *S*s was 10 x 10 in.

[17] The mean duration of the fixations for all *S*s was 0.275 of a sec.

the time of observation. Consequently every fixation was designated by two coordinate numbers constituting a definite point in the area.

Excursion: (a) Distance. The points established were next plotted for the entire period and the distance between subsequent points taken as a measure for determining the distance and direction of the excursions. The excursion from the first to the second fixation was considered the first excursion.

(b) Direction. The grid for the location of eye fixations was placed under tracing paper upon which the ocular pattern was recorded. The excursion directions were designated either as horizontal or vertical. If the line formed an angle greater than 45° and less than 135° with the horizontal, the excursion-line was said to be vertical. All other excursion-lines were classified as horizontal.

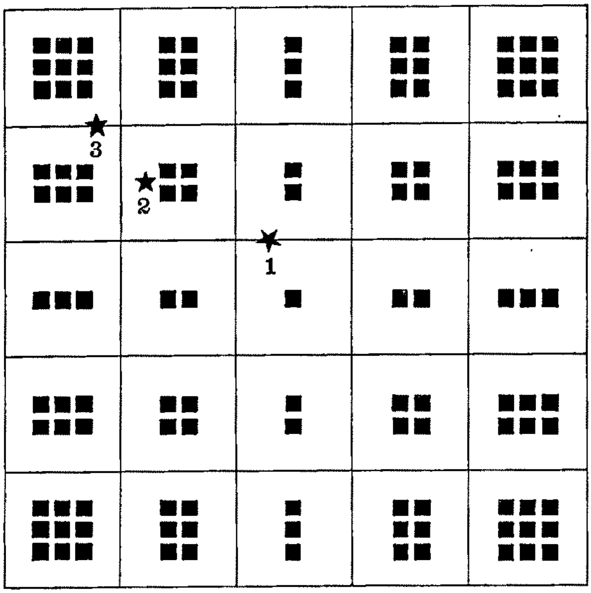

FIG. 1. MEDIAN LOCATION OF FIRST, SECOND AND THIRD FIXATION OF ALL THE *Ss* ON THE EXPOSURE-CARD

RESULTS

The median for the first fixations of all *Ss*, as indicated in Fig. 1, was located at a point above and to the left of the center of the card. This point is generally known as the optical center. The median of all the second fixations indicated that the eye had a tendency to move up and toward the left while the median of the third fixations was located still closer to the upper left hand corner.

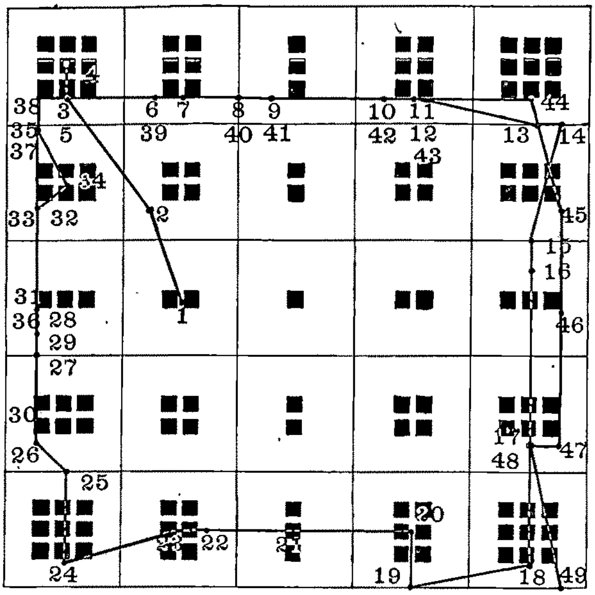

FIG. 2. OCULAR PATTERN OF *S* 14
(Achievement score 40 out of a possible 100.)

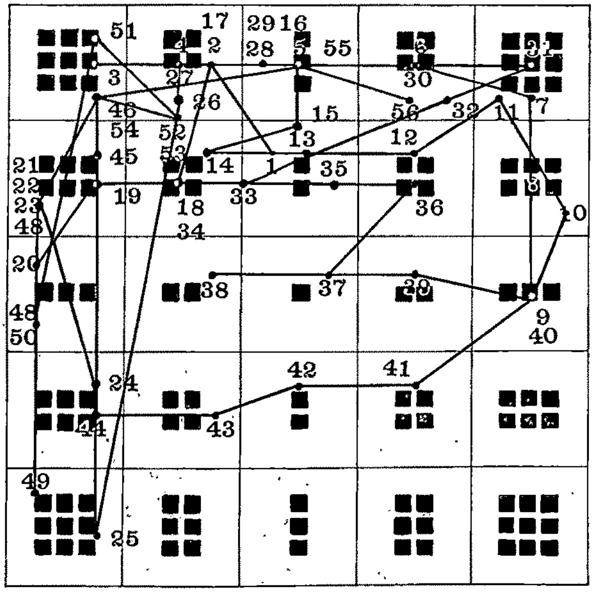

FIG. 3. OCULAR PATTERN OF *S* 15
(Achievement score 100.)

While all conclusions are based upon the performance of 50 *S*s, the results of only two *S*s are given here to illustrate the characteristic ocular patterns. The pattern given in Fig. 2 shows that *S* 14 did not see the cue relations. He returned answers only in the area where fixations were made.[18] Ten areas were correctly reproduced.

S 15 evidences in Fig. 3 an ocular pattern adequate for his purpose of reproducing the original pattern perfectly. From all indications, this *S* recognized the cue to the problem and discovered the existing relationships.

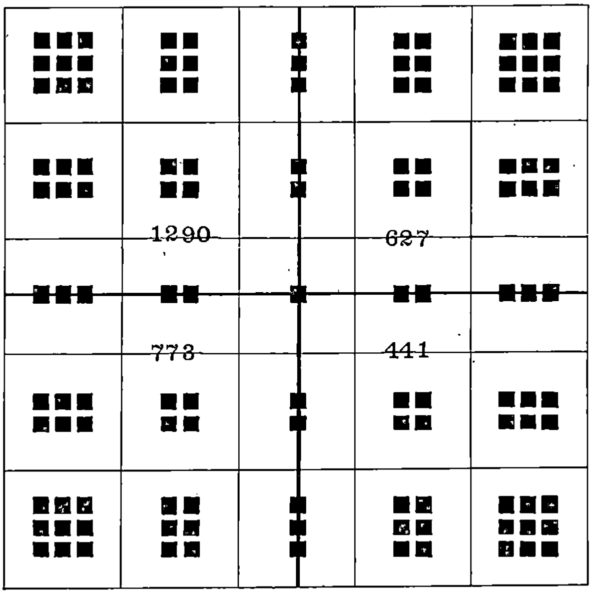

FIG. 4. RELATIVE FIXATION-FREQUENCIES IN AREAS OF A SYMMETRICAL FIELD

It appears that if the correct relationships are perceived, it is unnecessary to make complete coverage in order to reproduce the card. Evidently parts not seen may be reproduced when based on assumed relations.

The distribution of fixations as indicated by the above figure, implies a strong preference for the upper left hand area (41%) followed by the lower left (25%) and the upper right (20%) respectively. The lower right (14%) receives the least number of fixations of all four areas.

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[18] The numbers in Figs. 2 and 3 represent fixation-points as recorded by the camera. Number 1 indicates the first fixation with subsequent fixations following in numerical order. The line from fixation 1 to fixation 2 and subsequent fixations represents an excursion.

The average frequency of fixation in the different parts of the visual field and the average frequency and length of the horizontal and vertical excursions for 50 *S*s are given in Table I. These data serve as answers to the four questions raised at the beginning of this study. They reveal (1) that the left side of the field is fixated more frequently than the right; (2) that the upper half of the field is fixated more frequently than the lower; (3) that the horizontal excursion occurs more frequently than the vertical; and (4) that the sweep of the horizontal excursions greatly exceeds that of the vertical. In all of these cases, as Table I shows, the critical ratios (CR) are greater than 3.0 and the results are, therefore, statistically reliable and significant.

TABLE I

RELATIVE FREQUENCY OF FIXATION IN THE DIFFERENT SECTIONS OF THE VISUAL FIELD, AND THE RELATIVE FREQUENCY AND DISTANCE (IN CM.) OF THE HORIZONTAL AND VERTICAL EXCURSIONS

Position	Mean	S.E.$_{M}$	$M_{diff.}$	S.E.$_{M_{diff.}}$	CR
Left (freq.)	41.86	1.87	21.74	2.15	9.35
Right (freq.)	20.12	1.61			
Upper (freq.)	40.62	1.43	17.64	2.58	6.82
Lower (freq.)	22.98	1.85			
Horizontal (freq.)	35.34	1.6	14.38	.848	17.0
Vertical (freq.)	20.96	3.4			
Horizontal (dis.)	1955.98	102.1	862.93	16.31	52.8
Vertical (dis.)	1093.05	86.1			

SUMMARY AND CONCLUSIONS

The purpose of this study was to discover by means of ocular photography, characteristic eye movements and their psychological implications. Among the most outstanding characteristics of the ocular patterns were the frequency and location of fixations as well as direction and distance of excursions. Frequency and duration of fixations constitute measures of protensity, while excursion distance and direction imply extensity. Both are a type of dimension which may be measured with laboratory instruments. From the analysis of the ocular patterns it is apparent that factors other than variables in the content are responsible for certain modes of behavior.

The results of this investigation are: (1) the mean of the initial fixation is located at a point to the left and above the center of the observed field; (2) the second and third fixations are located to the left and above the first; (3) the fixations on the left half of the visual field exceed those on the right; (4) the upper half of the page is preferred to the lower; and (5) there is a strong tendency toward the facilitation of horizontal and the inhibition of vertical movements, thus affecting the excursion distance and direction.

The general conclusion to be drawn from these results; namely, that position is an important determinant of attention, is corroborated by earlier investigators. The slight variances discovered are largely due to the type of content selected for observation and the techniques employed in the respective experiments. Results are, for that reason, not always comparable.

Dallenbach and his associates agree with the general hypothesis that position is a potent factor in determining attention-value, and in addition support the conclusions that the left and top positions of the observed field have significant attentional advantage. Nixon, based upon his investigation, concludes that *Ss* observing a pair of advertisements for 30 sec. have a tendency to fixate first on the left hand page and that retention is greater on the page receiving the primary attention. Whether the left side preference as indicated when observing a single page will still hold when viewing a double page (as in periodicals) will need to be determined by further experimentation.

Implications. In studying ocular patterns of different individuals by means of photography one is impressed with the tremendous importance of the eye, not only as a sensory but also as a motor organ. When we remember that the eye fixates from 100 to 225 times per min., and that an equal number of excursions are made in the same time, we begin to realize the service rendered by this organ.

The study of ocular patterns is significant, first, because it suggests that one type of performance is preferred over another; and secondly, it raises the question of whether this phenomenon is caused by the physiological structure of the sense organ and its accessory apparatus, its neurological organization, its habitual mode of behavior or a combination thereof.

If these findings have practical application, artists, educators, advertisers, and editorial writers may attach a relative attention-value to specified areas of the respective fields of observation. In that case, these principles of behavior should play a significant rôle in the efficiency of the individual in obtaining a maximum of information or satisfaction as he views the subject matter. If, for example, the first fixations fall at a point to the left and above the middle of the page, the advertising layout which utilizes this principle will have many advantages. First, the eye will not be distracted at the beginning of the survey; secondly, the direction for the course of eye movements can be initiated here; and thirdly, the mental content or idea can have a right of way rather than to be forced by the physical stimuli of the presentation.

An adequate layout based upon this principle of observation would naturally organize all physical stimuli to facilitate the idea or purpose of the observer. The nucleus of the story would find a prominent place in the so-called optical center, in order to gain for the reader a proper orientation which would ultimately lead to a satisfactory completion.

If the eye prefers the horizontal to vertical movements, advertisements which require a preponderance of vertical movements will be read with much more difficulty or else will show only very poor coverage. Head or cutlines placed either at the top or bottom will rarely be seen. Left side preference as revealed by this study indicates that a more adequate use can be made of advertising space. If the common purpose of ocular movements is to gain for the individual such orientation as will be best for his present need, then each fixation, together with the excursion, implies inspection, selection, discrimination, and organization.

Since the eye movement technique analyzes more directly the process accompanying mental states, it may gain for teachers and supervisors a clearer understanding of the learning process, and furnish a basis for diagnostic and remedial procedures. Proper direction and instruction can only follow when the process itself is known. By means of ocular photography it should be possible to verify the assumption that achievement presupposes an efficient process.

THE EFFECT OF HYPNOSIS UPON VISUAL AND AUDITORY ACUITY

By KENNETH STERLING and JAMES G. MILLER, Harvard University

Though there is at present little disagreement that hypnotic suggestion can cause a decrease in sensory and muscular powers, and that it can cause an increase in maximal muscular performance, there is much disagreement as to whether sensory acuity can be increased under hypnosis. The only proper way to study this problem is by the use of control waking sessions similar in every important particular to the hypnotic session. In this paper is reported an attempt to increase by hypnotic suggestion visual perceptual acuity, visual sensory acuity, and auditory sensory acuity.

ANTECEDENTS

In the middle of the last century Braid[1] stated of hypnosis that: "In this process we have acquired the power of raising sensibility to the most extraordinary degree . . . and that any or all of the senses may be raised to the exalted state of sensibility referred to, almost *with the rapidity of thought.*" Henri Bergson in 1886[2] reported the case of a boy who, under hypnosis, read from the cornea of a man facing him the reflection of the 3-mm. letters of a book which the man was holding behind the boy's head. The whole image on the cornea could not have been more than 0.1 mm. high. He was also able to draw with the naked eye an object on a microscope slide 0.06 mm. high. Bergson did not ascertain whether the boy could have repeated these feats in the waking state.

In the same year Delboeuf performed experiments, cited by Bramwell,[3] on post-hypnotic recognition of time intervals. He used two young servant girls as *Ss*, who were unable even under hypnosis to tell time well or to perform the arithmetic necessary to convert such large numbers of minutes as 1600 or 1150 into hours and minutes. *M*'s ability at arithmetic was better than *J*'s, and her results were more accurate. Delboeuf suggested under hypnosis that the girls would perform certain acts a specific number of minutes later. In 14 experiments, 6 were exactly correct, and 7 others were considered by Delboeuf as partially successful. No waking control sessions were used. Bramwell himself did 55 experiments of this sort with Miss D (also referred to as Miss A) in 1895–96.[4] *S* was told in a trance that at the end of a certain large number of minutes she should place a cross on a piece of paper and mark down what time she thought it was. Bramwell seems to assume that Miss D was unable to do the arithmetic involved

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[1] J. Braid, *Neurypnology* (*Braid on Hypnotism*), ed. A. E. Waite, 1899, 86 f.

[2] H. Bergson, De la stimulation inconsciente dans l'état d'hypnotisme, *Rev. philos.*, 22, 1886, 525-531.

[3] J. M. Bramwell, *Hypnotism*, 1913, 116-119.

[4] Bramwell, *op. cit.*, 119 ff.; also, Hypnotic and post-hypnotic appreciation of time, *Brain*, 23, 1900, 161-238.

for the reasons that (1) she often had difficulty with such problems, and (2) she was normally unable to remember such complicated numbers. Out of 55 attempts, only 9 were wrong, and they were only 2 to 5 minutes off. No waking control was performed. Like results were obtained with other *Ss*.

Moll, in the fourth edition of his book on hypnotism in 1909,[5] reported controlled experiments of his own that agreed with some work of Berger's showing that two-point discrimination is increased under hypnosis; he described like results for the pressure and temperature senses, but these were not controlled by comparison with the waking state; he said that D'Abundo produced enlargement of the field of vision under hypnosis; and he reported clinical results showing improvement in vision, smell, and muscular sense. Whether or not he always used controlled technique, he was aware of its necessity and explicitly critical of those who did not use it.

Bramwell, in 1913,[6] cited evidence to show that hypnotic suggestion can markedly improve vision, hearing, smell, lifted-weight discrimination, two-point limen, and temperature discrimination. All except two of these statements he based on clinical observation. In the experiment on lifted weights there was a control session in the waking state, but there is no evidence of great insistence on attending to the discrimination in the waking state, and there was an insufficient number of trials. The two-point limen experiment was not well controlled.

In 1914 Prince[7] made mention of an experiment in which objects were placed in the field of vision so far toward the periphery that they could not be seen clearly enough to be identified. Later they were accurately described in hypnosis. In another book Prince reported a similar experiment,[8] probably the same one, in which personality C of the dissociated patient BCA was the *S*. This may well mean that the results were abnormal, especially in the light of her behavior. The objects put into the periphery of vision were a match box, a pencil, a roll of paper (all of which *S* did not recognize except under hypnosis), a watch (which *S* recognized by its tick in the waking state), and a toy cat (which *S* recognized immediately with a shriek because of a former strongly emotion experience with a cat).

Travis in 1922 reported some experiments,[9] and Morgan discussed them at length in 1924,[10] in which a lowering or raising of the auditory limen was used as a measure of hypnotizability. Travis stated that in certain *Ss* a 'hypnagogic' state was produced by staring in reverie at a crystal. Those *Ss* who showed what was called a 'schizophrenic' tendency were negativistic, and their limens rose, while those who were suggestible and easily hypnotizable (called 'psychoneurotic') showed lowering of limen. These results were supposed by Travis to be due to a change not in sensory acuity, but in some central neural set. In this work the *Ss* were not hypnotized but 'hypnagogic,' but it was reported that[11] "those who obeyed the instruction and maintained the set to respond while they abstracted were enabled to react to a stimulus below their normal threshold."

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[5] A. Moll, *Hypnotism*, trans. 4th ed., 1909, 114 ff.

[6] Bramwell, *op. cit.*, 1913, 89 ff.

[7] M. Prince, *The Unconscious*, 1914, 57-58.

[8] Prince, *Clinical Experimental Studies in Personality*, ed. A. A. Roback, 1939, 346-347.

[9] L. E. Travis, Studies in dissociation, *J. Exper. Psychol.*, 5, 1922, 338-346; Suggestibility and negativism as measured by the auditory threshold during reverie, *J. Abnorm. Psychol.*, 18, 1924, 351-368.

[10] J. J. B. Morgan, The nature of suggestibility, *Psychol. Rev.*, 31, 1924, 463-477.

[11] Travis, *op. cit.*, 345.

In 1925 Young[12] reported that he could get no increase in tactual sensitivity under hypnosis (but, rather, a slight decrease), yet the *Ss* expressed a belief that they did better, which Young explained[13] as due to a "very great subjective intensification of the consciousness in regard to a touch which is actually felt." This work was controlled by sessions in the waking state. The same experimenter in 1926[14] performed an experiment in which one stamp was studied for 5 sec. through a half-inch hole, which allowed to be visible only the face and part of the background. Then the stamp was shuffled with ten like stamps, and all were put under like holes. *S* was asked to pick out the stamp first seen. Six *Ss* did exactly as well waking as under hypnosis; for one other *S* the experiment was not completed.

In 1927 Lifschiftz reported great lowering of auditory limen under hypnosis in an experiment which was probably unrigorous.[15] Koster, in 1928, found that with 21 *Ss* he could by hypnosis bring about a diminution in muscular strength and discriminative ability in two thirds of the *Ss*, and also could obtain augmented muscular strength in two thirds, but could get increased discrimination ability in only one third.[16] In 1929 Fisher reported the case of a girl who was able under hypnosis to hear the ticking of a watch at fully twice the distance that she could in the waking state.[17] He says that many such cases of increased sensory ability under hypnosis have been observed. Of course in this case if *S* knew it was a watch ticking, she could under hypnosis have reported it even though she did not hear it, in order to please *E*.

Stalnaker and Richardson in 1930 reported a well-controlled experiment in which 9 *Ss* were tested for their ability to judge short intervals of time.[18] Altogether 1080 determinations were made, half waking, half under hypnosis. Each *S* had an equal number of trials for 1-, 2-, and 3-min. periods presented at random. None of the differences between waking and hypnotic judgments was significant, but the *Ss* thought they were doing better under hypnosis. Hull,[19] in 1933, referred to a minor experiment by Eken in which she found that the ability of persons to judge 16-min. intervals was not significantly different in hypnosis and in the waking state.

The literature reviewed is in conflict, but in the experiments which were apparently best controlled, and in the more recent work, the weight of evidence seems to indicate that hypnosis does not improve sensory acuity.

(A) Visual perception. The apparatus employed in the experiment in

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[12] P. C. Young, Experimental study of mental and physical functions in normal and hypnotic states, this JOURNAL, 36, 1925, 214-232.

[13] Young, *op. cit.*, 222.

[14] Young, An experimental study of mental and physical functions in the normal and hypnotic states, this JOURNAL, 37, 1926, 345-356.

[15] S. Lifschiftz, *Žurnal nevropatologii i psichiatrii*, 20, 1927, 317-324.

[16] S. Koster, Untersuchungen über Hypnose, *Zsch. f. d. ges. Neur. u. Psychiat.*, 109, 1928, 49-61.

[17] V. E. Fisher, *An Introduction to Abnormal Psychology*, 1929, 432 f.

[18] J. M. Stalnaker and M. W. Richardson, Time estimation in the hypnotic trance, *J. Gen. Psychol.*, 4, 1930, 362-366.

[19] C. L. Hull, *Hypnosis and Suggestibility: An Experimental Approach*, 1933, 250.

visual perception was that used by one of the authors in another experiment and described in detail in a former article in this JOURNAL.[20]

Procedure. The *Ss* were led into a dimly lighted room and seated facing an 8-in. by 12-in. transparent mirror set in the opposite wall. *S*'s chair was about three yards from the mirror. During the experiment geometric figures were projected upon this mirror from behind at various intensities. The figures used were four of those on Rhine's ESP cards[21]—circle, square, plus, star. *E* made certain that *S* knew the figures and what they looked like when projected on the screen. They were almost the full size of the screen.

E sat behind *S* in a small room connected with *S*'s room through an open door. He sat behind a screen over which he could see *S,* facing a panel from which he could control remotely the exposure of the cards and the illumination at which the cards were projected. This illumination was regulated by a variac, which gave none at zero volts, and a comparatively dim image, though definitely supraliminal, at its top reading, 135 volts. By a periscope *E* could tell what figure was being presented on the screen, even at illuminations well below the limen.

Each *S* was tested during three periods: (1) a preliminary waking period, (2) a hypnotic period, and (3) a final waking control period.

After about 10 min. for dark adaptation, the first waking tests were made in the dark room. *S* was urged to concentrate as completely as he could. The figures were projected on the screen at random. The main purpose of these tests was to give *S* practice, to adapt him to the dark, and to determine the approximate region of his limen.

S was then hypnotized, and his state of hypnosis tested by obtaining eyelid and arm catalepsy. The dim room light was turned on for the hypnosis, but was turned off immediately afterward. After time for dark adaptation, it was suggested to *S* that he concentrate fixedly upon the screen, and that he had extraordinarily sharp and accurate vision. This latter suggestion was repeated continually throughout the hypnotic visual test. The four figures were presented at random in this period, each several times, *E* beginning at zero illumination and raising the brightness until *S* reported what the figure was.

S was awakened in the dimly lighted room, and then the light was turned off and he was given the same time for dark adaptation that he had been given immediately after hypnosis was induced. Then the average intensity at which he could just see each of the four figures presented at random was determined. The whole procedure lasted about 1½ hr. As has been noted in another article on an experiment in which this apparatus was used,[22] the limens for the various figures differ, but since the figures were presented at random in equal numbers, the effects of these liminal differences are not significant for the group as a whole. This has been determined statistically. The use of four figures at random eliminated the following difficulties: (1) confusion of after images with real images, and (2) preguessing the symbol, as might occur if only one or even two figures were employed.

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[20] J. G. Miller, Discrimination without awareness, this JOURNAL, 52, 1939, 562-578.

[21] C. E. Stuart and J. G. Pratt, *A Handbook for Testing Extra-Sensory Perception,* 1937.

[22] J. G. Miller, *op. cit.,* 571-573.

Altogether in this visual experiment 9 *S*s were used who developed eyelid and arm catalepsy, and whose data are included in this report. Those *S*s from whom *E* could not obtain these symptoms of hypnosis were eliminated.

Results. The results are summarized in Table I. The CR of the paired differences between the averages for each *S* in hypnosis and in the waking state is 0.57. It will be noticed that the only *S*s giving significant differences in the two states were negativistic, showing decreased acuity on suggestion of increased acuity.

All *S*s accepted for these data gave the objective criteria of eyelid and arm catalepsy, and besides that *E* made the following judgments of degree of hypnosis: *S 1*, deep hypnosis; *2*, very light, awake by end of session; *3*, fairly deep; *4*, good light; *5*, good light; *6*, fairly deep; *7*, good light; *8*, very light; and *9*, deep.

TABLE I

VISUAL PERCEPTUAL ACUITY IN HYPNOSIS AND WAKING

Average illuminations (in volts) at which the geometrical figures were just perceptible.

S	Hypnosis					Waking				
	plus	circle	square	star	av.	plus	circle	square	star	av.
1	57.0	62.5	58.3	—	59.3	66.5	61.3	65.3	66.7	64.9
2	74.3	78.2	76.7	110.7	85.0	67.5	62.5	65.3	98.0	73.3
3	50.3	50.3	55.6	64.7	55.2	40.3	47.4	46.0	62.0	48.9
4	70.0	69.5	68.5	72.0	70.0	71.5	67.0	72.5	80.0	72.7
5	67.0	61.2	59.3	85.0	68.1	53.3	54.0	53.2	72.5	58.3
6	95.0	64.6	63.9	114.0	84.4	80.7	51.2	57.8	85.0	68.7
7	95.8	82.7	88.2	71.7	84.6	81.5	86.3	87.5	84.0	84.8
8	61.8	58.4	65.8	79.3	66.2	55.0	60.5	80.0	83.5	69.8
9	62.8	64.4	67.4	81.8	69.2	—	58.0	59.3	70.5	62.6
Av.	74.4	65.8	69.3	84.9	71.3	77.0	61.0	65.2	78.0	66.0

(B) Visual sensation. This experiment deals with the effect of hypnosis upon discriminating the absence of a light from its presence, on the mirror used in Experiment A.

Procedure. Each session was preceded by a period of dark adaptation. The limen for each *S* was determined in the waking state by the method of limits, using 3 ascending and 3 descending series. *S* was then hypnotized and eyelid and arm catalepsy elicited. After suggestion that *S* now had especially acute vision, his limen was determined as before. Then *S* was tested again for arm catalepsy, which was obtained in each case. He was then awakened and his limen was determined again in the same manner.

Results. The results are summarized in Table II. It is apparent that the differences between the acuities in the two states were exceedingly slight.

Besides the eyelid and arm catalepsy criteria of depth of hypnosis, *E* made the following subjective judgments of the degree of the *S*s' hypnosis: *S 1*, good deep; *2*, fairly light; *3*, good deep; *4*, good light; *5*, good light;

6, good light; *7,* good light; *8,* good deep; *9,* fairly deep; and *10,* good light.

(C) Auditory sensation. This work has to do with the recognition of the presence or absence of a single simple tone, rather than the perception of a more complex figure.

Procedure. S was taken into a dimly lighted soundproof room. He lay down on a couch, put on earphones, and covered the earphones and his ears with pillows. In his hands he had a signal key which, when pressed, flashed a signal light for *E.* The wires from the earphones led outside the room to an oscillator producing a pure tone of about 925~. On the output of the oscillator was an attenuator, which attenuated the constant voltage output from the oscillator from 0 to 100 db. by steps of 1.5 db. The tone was turned off by a shunt across the output of the oscillator. The shunt was cut in and out by a silent mercury switch.

TABLE II

VISUAL SENSORY ACUITY IN HYPNOSIS AND WAKING

Average illuminations (in volts) at which light was just noticeable

S	Preliminary waking period	Hypnotic period	Final waking period
1	33.8	36.0	34.8
2	37.0	36.5	36.0
3	33.1	34.3	34.3
4	35.8	35.8	34.1
5	34.3	34.5	34.3
6	37.1	35.8	35.1
7	33.7	33.8	33.8
8	32.5	32.3	32.0
9	31.1	31.3	31.3
10	39.8	39.1	38.7
Av.	34.8	34.9	34.4

E instructed *S* to press the key during whatever period he heard a tone, and then left the soundproof room and shut *S* in. The method of limits was then used. *E* began at zero and added attenuation until *S* did not press the signal key for any of 10 short tones at that intensity. The intensity next above this was taken as the descending value. *E* then further increased the attenuation until it was well below the limen, and then raised the intensity of the tone in steps of 1.5 db., giving short tones at each step. When the light went on for one or more times, that was taken as the ascending value. For each *S* under these conditions, three ascending and three descending points were determined. The mean of these values was taken as the limen for this preliminary practice period.

S was next hypnotized and tested for eyelid and arm catalepsy. When these conditions were obtained, *E* suggested to *S* that he had extraordinarily acute auditory powers. The same procedure for obtaining the limen was repeated, three ascending and three descending values being found for each *S. S* was finally brought out of hypnosis, and the whole procedure repeated a third time, and a limen found for this final waking period.

Results. The results of the auditory experiment are summarized in Table III. Since the practice effect and adaptation were negligible in this experiment, the results from the preliminary practice session were considered sufficiently significant to be included. The CR of the first waking period with the hypnotic period was 0.66, and of the second waking period with the hypnotic was 0.11. In this experiment 70% of the trials lay within 1 db. of the limen value for that *S* in that period.

All the *S*s accepted for these data gave eyelid and arm catalepsy, and *E* also made the following subjective judgments of the degree of their hypnosis: *S 1,* light hypnosis; *2,* good light; *3,* good deep; *4,* light; *5,* light; *6,* good light; *7,* deep; *8,* fairly deep; *9,* light; *10,* deep, awake at end of session; *11,* deep, and *12,* very good light.

It is interesting to note, in corroboration of Young[23] and of Stalnaker and Richardson,[24] that a majority of the *S*s in both visual and auditory experiments reported subjective certainty that they were doing better under hypnosis than in the waking state. As Hull has noted,[25] this may well have been the cause for some of the earlier reports of increased sensory ability under hypnosis.

TABLE III

Auditory Limens (Decibels of Attenuation) in Hypnosis and Waking

S	Preliminary waking period	Hypnotic period	Final waking period
1	46.3	46.2	44.1
2	42.4	43.0	42.7
3	41.3	41.6	42.4
4	35.3	35.7	35.9
5	37.1	38.5	38.3
6	41.5	45.1	42.3
7	42.2	42.2	41.3
8	39.4	40.5	39.8
9	41.0	40.7	45.0
10	34.7	37.1	35.8
11	38.7	37.5	38.9
12	35.6	38.7	38.0
Av.	39.6	40.1	40.5

CONCLUSION

The primary conclusion of these results is that it is not possible by hypnotic suggestion to lower significantly the limen for visual perception or sensation, or auditory sensation. There are at least three factors which must be considered in explanation of this result: motivation, attention, and sensory limitation. Was the limen not lowered because hypnotic suggestion does not increase motivation? Most probably not, because there is abundant evidence in the literature that hypnosis can increase motivation, as in the experiments which indicate that it can increase muscular performances beyond the usual voluntary range.[26] Is it—in opposition to Fisher's hypothesis[27] that attention can be increased under hypnosis more than it can be by insistent requests in the waking state—because attention cannot be increased by hypnosis? That is probably one reason, for certainly there is so much evidence that attention affects the correctness of sensation and perception, that it must have been constant in both waking and hypnotic sessions in order to get the practically constant results of this study. Is it because it is impossible to increase the acuity of sensory end-organs beyond a physiological limit? Probably that is the most important factor. In the waking state, with practice and dark adaptation, the senses reached their full capacity. The attention of *S* was at a maximum, and even with motivation increased under hypnosis, he was physically unable to do any better. Unlike muscular work, it seems to be possible for sensation to reach its maximum efficiency in the waking state. The earlier uncontrolled experiments underestimated the possibility of reaching this maximum without hypnosis. This experiment indicates, therefore, as Travis[28] and others have suggested, that hypnosis has a central and not a peripheral effect. Hypnotic suggestion may hinder proper central organization of the response, but it cannot enable the individual to exceed the limits of acuity set by the peripheral receptor and the lower organizing centers.

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[23] P. C. Young, Experimental study of mental and physical functions in normal and hypnotic states, *loc. cit.*

[24] J. M. Stalnaker and M. W. Richardson, *loc. cit.*

[25] C. L. Hull, *op. cit.*, 248.

Thus this experiment joins many others, such as Bass's,[29] which indicate that hypnosis is closely akin to the waking state.

What effect would hypnosis have had, if *S* had been required to guess about subliminal perceptions or sensations? It might well have increased the correctness of such guesses, but this result would have been due to the increased motivation under hypnosis, for increased motivation can counteract the boredom and feelings of frustration which develop in subliminal learning experiments.[30] Evidently correct guessing would, as in the waking state, be limited by the physiological maximum of acuity, and apparently hypnosis would effect little or no improvement in concentration over the waking state, although it might well keep attention from fluctuating for longer periods.

These results serve to add to the weight of evidence to the conclusion that hypnosis does not improve sensory acuity, and to extend the experimental findings in a quantitative manner to visual perception and sensation, and auditory sensation.

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[26] Hull, *ibid.*, 224-244. [27] Fisher, *loc. cit.* [28] Travis, *op. cit.*, 345.

[29] M. J. Bass, Differentiation of the hypnotic trance from normal sleep, *J. Exper. Psychol.*, 14, 1931, 382-399.

[30] J. G. Miller, The rôle of motivation in learning without awareness, this JOURNAL, 53, 1940, 229-239.

VARIABILITY OF JUDGMENT AS A FUNCTION OF STIMULUS-DENSITY

By JOHN VOLKMANN, Columbia University, WILLIAM A. HUNT, Wheaton College, and MILDRED MCGOURTY, University of Iowa

In studying affective judgments, Hunt and Flannery have shown that affective variability, defined in terms of the number of changes in judgment, increases with the number of stimuli to be judged.[1] In a previous article Hunt and Volkmann had shown that the anchoring effects demonstrable in judgments of visual inclination can be duplicated with affective judgments.[2] They suggest the operation of general principles of judgment, common to both affectivity and psychophysics. If some common principles do indeed apply, it should be possible to duplicate the results of Hunt and Flannery on variability in the affective judgment with conventional psychophysical material, as, for example, with lifted weights. While the present experiment is designed to test this possibility, it has certain other purposes as well.

In the first experiments of Hunt and Flannery the addition of more color-stimuli may have had either or both of two effects: an increase of the total affective range covered by the stimuli, or an increased density of stimulation within a fixed affective range. The range is defined as the difference in affective value between the most pleasant color and the least pleasant color; the density is defined as the number of affective stimuli per unit of range. It is probable that the addition of more color-stimuli increased both the affective range and the density of stimulation and, in advance of experimental information, it remains possible that variability of judgment is a function of both affective range and stimulus-density. Stimulus-density was probably, however, the major independent variable, since Hunt and Flannery attempted to keep range approximately constant by selecting stimuli which covered a wide range in each part of their experiment. In an effort to produce a parallel situation, the present psychophysical experiment holds constant the variable of stimulus-range and examines the effect of varying stimulus-density.

The stimuli were weights, arranged for lifting. In each part of the experiment the lightest weight was 45 gm. and the heaviest was 100 gm.; the stimulus-range was therefore held constant at 55 gm. The total number of stimuli varied from 3 to 12, and the intermediate stimuli were equally spaced between the values 45 gm.

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[1] W. A. Hunt and J. Flannery, Variability in the affective judgment, this JOURNAL, 51, 1938, 507-513. Note especially the trend of the results of individual *S*s in Fig. 1 and Fig. 2, p. 509.

[2] W. A. Hunt and J. Volkmann, The anchoring of an affective scale, this JOURNAL, 49, 1937, 88-92.

and 100 gm. For example, the 3-stimulus set consisted of the weights 45 gm., 72 gm., 100 gm.; the 4-stimulus set consisted of the weights 45 gm., 63 gm., 82 gm., 100 gm.; and so on. Each stimulus-set was presented 100 times to each *S*, to yield 100 psychophysical series for record. The order of presentation was haphazard within single series. An arm-rest supported *S*'s arm and a screen cut off the apparatus from her vision. The stimuli were presented at a uniform rate of 1 in 10 sec. At a verbal signal given by *E*, *S* lifted the weight with a smooth wrist motion. The instructions to *S* called for absolute judgments in 3 categories. They were as follows:

> I am going to present to you a series of weights which you are to judge. Make your judgments in the following way: call *1* for light weights, *2* for medium, and *3* for heavy. When I say 'ready,' pick up the weight under your hand and make your judgment.

Four women were used as *Ss*, but the fourth did not complete the experiment. We have defined variability, following Hunt and Flannery, as a change in the category of report. This means that the amount of variability can be ascertained by counting the number of changes in the category of report for each weight during any experimental session. Thus, if a weight were called *heavy* (*3*) the first time it was presented, *light* (*1*) the next two times, and *heavy* (*3*) the last time, there would be 2 changes in the category used and the variability-score would be 2. The sum of the variability scores for all of the weights gives the total variability or number of changes (N_{ch}).

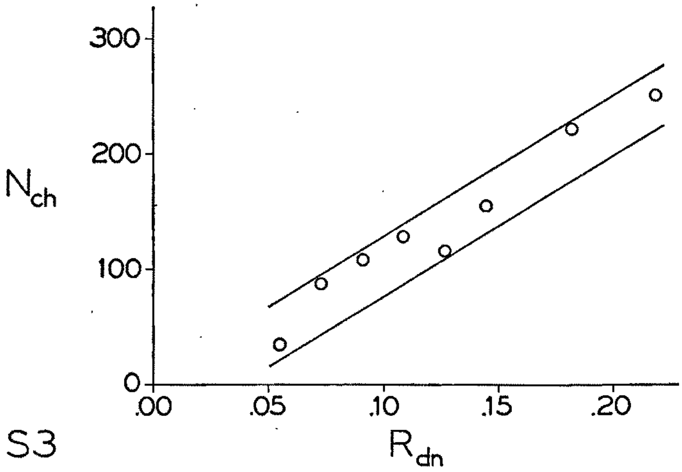

FIG. 1. THE NUMBER OF CHANGES IN JUDGEMENT AS A FUNCTION OF STIMULUS-DENSITY: DATA OF *S3*

We are also presenting a second definition and measure of variability which is more nearly consistent with usual psychophysical practice. The variability represented by a psychometric function may be expressed as *h* (the index of precision in the Urban calculation), as a standard deviation, or as the semi-interquartile range,

Q. The last-named is the measure adopted here. Since the present experiment used three categories of judgment in each of its parts, it yielded two psychometric functions, just as the method of constant stimulus-differences yields two psychometric functions from its three categories *greater, equal,* and *less.* The Q's were actually determined by graphical interpolation from the data plotted on probability paper, which transforms the sigmoid psychometric data into approximately rectilinear form. The quartiles may be read off the plot without calculation. This process does not assume the normality of the underlying frequency-distribution. The Mean Q (Q_M) which appears in Table I and Fig. 2 is the mean of the Qs of the two psychometric functions.

TABLE I

SHOWING FOR THE NUMBER OF STIMULI IN EACH STIMULUS SET (N_R) AND THE CORRESPONDING STIMULUS-DENSITIES (R_{dn}), AND FOR EACH S, THE NUMBER OF CHANGES IN JUDGMENT (N_{ch}), THE MEAN SEMI-INTERQUARTILE RANGE (Q_M), THE WIDTH OF THE MIDDLE CATEGORY (W_2), AND THE CENTER OF THE SCALE OF JUDGMENT (C)

N_R	R_{dn}	*S1*				*S2*			
		N_{ch}	Q_M	W_2	C	N_{ch}	Q_M	W_2	C
3	.055	30	5.48	27.8	71.9	20	—	—	—
4	.073	72	4.30	26.8	70.5	81	6.08	26.7	76.6
5	.091	108	4.85	26.0	73.2	95	4.70	14.7	69.4
6	.109	142	5.23	27.9	74.7	94	4.03	12.1	57.9
7	.127	139	4.05	26.4	71.0	97	4.28	14.8	55.6
8	.145	188	4.30	29.2	68.7	98	3.78	13.2	55.7
10	.182	199	4.03	27.0	68.0	124	2.83	10.5	50.5
12	.218	342	5.45	26.3	70.2	93	2.45	10.2	50.9

N_R	R_{dn}	*S3*				*S4*			
		N_{ch}	Q_M	W_2	C	N_{ch}	Q_M	W_2	C
3	.055	34	—	—	—	77	—	—	—
4	.073	87	5.88	21.7	68.5	102	7.95	28.9	77.9
5	.091	108	4.60	25.2	69.1	116	8.15	31.5	85.2
6	.109	129	6.00	26.3	75.8	157	5.75	29.0	81.8
7	.127	116	4.80	24.9	71.3	204	5.90	20.1	67.7
8	.145	155	4.80	23.0	72.5	—	—	—	—
10	.182	222	4.73	22.1	72.3	—	—	—	—
12	.218	252	4.18	21.8	72.4	—	—	—	—

Table I shows for each of the 4 *S*s and 8 stimulus-densities the N_{ch} and the Q_M. Fig. 1 shows N_{ch} as a function of stimulus-density for *S3.* A straight line is a fair first approximation to the data of this *S* and the data of *S1* and *S4.* The data of *S2,* however, are markedly curvilinear, although the slope is still positive: N_{ch} increases with stimulus-density. Fig. 2 shows for *S3* the relation between Q_M and stimulus-density. Q_M is seen to be approximately constant; the slight decrease is probably an artifact of advancing practice. The order of experimentation began with low stimulus-densities and ended with high ones; a longer period of preliminary practice or a procedure which made the order of experimentation random might have avoided this slight decrease. The results of *S1* are very similar. The results of *S4* are too scanty to permit any generalization, and the results of *S2* are atypical since Q_M shows an abrupt decrease.

An inspection of Table I will show that W_2 is constant in the data of *S1* and *S3*. W_2 is a measure of the width of the middle category *2;* it is the central interval in the terminology of the method of single stimuli,[3] and corresponds to the interval of uncertainty in the method of constant stimulus-differences. The data of *S4* are again too scanty and the data of *S2* are again atypical, since they show W_2 decreasing instead of remaining constant.

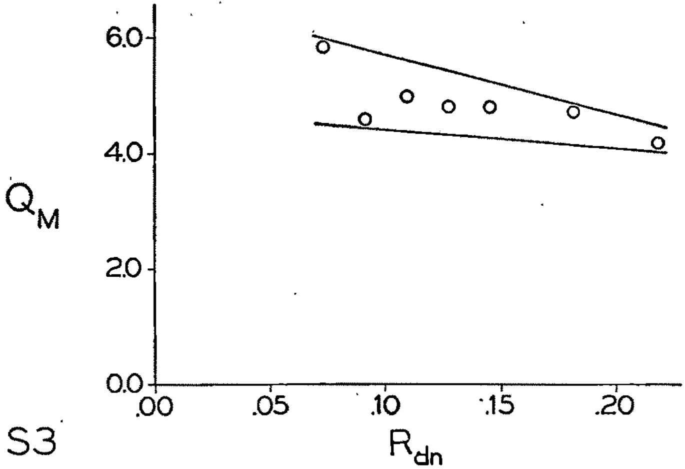

FIG. 2. THE MEAN SEMI-INTERQUARTILE RANGE AS A FUNCTION OF STIMULUS-DENSITY: DATA OF *S3*

The atypical results of *S2* deserve a separate treatment. Obviously some additional independent variable is at work; fortunately this variable is discoverable in the data themselves. As the order of experimentation progressed from low stimulus-densities to high ones, *S2* displaced her absolute scale more and more toward the lower end of the stimulus-range, so that the category *3* was used very often and categories *1* and *2* relatively seldom. This extreme displacement of the scale is shown in the decreasing values of C for this *S* in Table I. C is the center of the middle category, and *2* of the three-category scale. The other *S*s do not show any considerable displacements of C. Fig. 3 shows for *S2*, Q_M as a function of the proximity of C to the end of the stimulus-range. The measure of proximity is the difference, taken without regard to sign, between C and the end-stimulus which is nearer it, R_p, the proximal stimulus. As the scale is more and more displaced toward this proximal stimulus, the quantity $\overline{(C\text{-}R_p)}$ decreases; and it will be seen from Fig. 3 that (following the data from right to left) Q_M decreases also. The apparent decrease of Q_M with stimulus-density is probably to be explained by this displacement of the scale. The decrease in W_2 mentioned above may be accounted for in a similar way, as a function of displacement. Although this analysis of an exceptional case cannot establish a generalization, it does suggest one: displacement of an absolute scale toward either end of the stimulus-range produces a decreased variability and a decreased category-width. The attitudinal or other variables which originally produced the displacement are not known, so that the suggested generalization will be difficult to verify. As will be shown later, N_{ch} depends upon Q_M, so that if the variation of Q_M is atypical, as it was in the case of *S2*, the variation of N_{ch} would be expected to be atypical also.

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[3] The central interval is defined as the difference, taken without regard to sign, between the limen of the upper category and the limen of the lower category. Cf. J. Volkmann, The method of single stimuli, this JOURNAL, 44, 1932, 808 f.

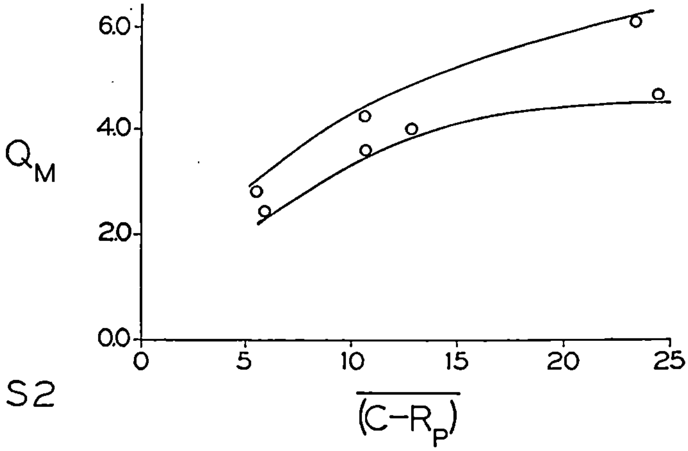

FIG. 3. THE MEAN SEMI-INTERQUARTILE RANGE AS A FUNCTION OF THE DISPLACEMENT OF THE SCALE OF JUDGMENT: DATA OF *S2*

Discussion. There is essential agreement with the finding of Hunt and Flannery that variability, as measured by the number of changes of judgment, increases as a function of stimulus-density. This agreement supports the hypothesis that in the field of judgment there are regular relations which describe the judgment of any characteristic of any material. Other lines of evidence support this hypothesis: for example, the evidence upon anchoring in affectivity and psychophysics[4] and the evidence for the round-number tendency in the judgment of a great variety of materials.[5] There is increasing indication that judgment deserves the status of a separable field of investigation, since it appears to possess its own characteristic regularities.

The width of the middle category (W_2) remains constant as stimulus-density varies. Category-width is known to vary with stimulus-range;[6] the fact that category-width does not also vary with stimulus-density implies that category-width depends upon the position of the end-stimuli and not upon any intermediate stimuli. Q_M is also constant, and likewise seems to depend solely upon the position of the end-stimuli. The theory of the absolute scale must take account of these results; the absolute scale seems to resemble a linear chain of categories whose ends are constrained by the two end-stimuli. The constancy of Q_M and W_2 is all the more noteworthy because the variation of stimulus-density involves the simultaneous variation of an entirely different variable: the total presentation-time per series. Since the stimuli were presented at a uniform rate of 1 in 10 secs., and since an increase in stimulus-density necessitated the addition of intermediate stimuli in each series, the total time needed for the presentation of a complete series increased.

The two measures of variability, N_{ch} and Q_M, seem to yield contradictory results, since N_{ch} increases with stimulus-density while Q_M remains constant, but actually there is no contradiction. The precise relation between the two measures cannot be stated at the present time because the considerations involved in it are too complicated. It is not difficult, however, to gain an approximate idea of the relation. The essential feature is that Q_M is not dependent upon the number of stimuli within a given stimulus-range (the terms in which we have defined stimulus-density), while N_{ch} is.

The reliability of the determination of a psychometric function depends upon the number of stimuli which can actually be used in determining the function, just as the reliability of the determination of any function depends in some degree upon the number of observed points. Each stimulus gives a percentage of judgments in a given category and in consequence one point which aids in the determination of the function of that category. Q is one of the constants of the psychometric function. *A priori,* then, the reliability of Q does depend upon the number of stimuli within the useful range, but the true mean value of Q does not. Empirically considered, *any* relation might obtain between Q and stimulus-density, if stimulus-density had an effect upon the discriminatory mechanisms. Apparently it does not have any effect (under the conditions of the present experiment), because Q remains substantially constant.

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[4] Hunt and Volkmann, *loc. cit.*

[5] J. E. Coover, Experiments in psychic research, etc., *Leland Stanford Junior Univ. Publ.*, Psychic Res. Monog., No. 1, 1917, 230-308.

[6] J. Volkmann, The compression of an absolute scale, *Psychol. Bull.*, 35, 1938, 676 (abstract).

The case of N_{ch} is very different, for this is a measure which is indeed dependent on the number of stimuli. If the number of stimuli is increased and if the total number of series remains constant, the total number of judgments is of course increased; it might be argued that more possibilities of changes in judgment exist, and that therefore more changes occur by reason of chance factors alone. But this argument does not do justice to the actual situation. The distribution of judgments upon the stimulus-continuum is not random but regular; the regularity is expressed by the psychometric functions. Each stimulus yields a given proportion of judgments in different categories, and this proportion is one determinant of the number of changes in judgment. If an end-stimulus yields judgments which are all in one category, for example, there will be no possibility of changes in judgment on that stimulus. Intermediate stimuli, on the other hand, are likely to yield judgments in several categories and present many possibilities of changes in judgment. The number of changes in judgment which occur at a given stimulus value depends on at least the following variables: the number of categories, the proportions of judgments in different categories, and the intraserial effects. The determination of the intraserial effects is in its turn a complicated one. Some stimuli that are otherwise affected by the foregoing variables will produce more changes in judgment, and still others will produce fewer changes. Since the changes produced by the various stimuli are added to give the measure N_{ch}, the general effect is obviously to increase N_{ch} as more stimuli are inserted to increase the stimulus-density. The original contradiction is thereby resolved. It should be added that while Q is independent of N_{ch}, N_{ch} is not independent of Q. When Q varies there is necessarily a variation in the proportions of judgments in different categories, and a consequent variation in the possibility of changes in judgment.

N_{ch} is a useful measure of variability in those cases in which there is no known relation between the psychological variable and a stimulus-continuum. This is often true, for example, in affectivity. It may be less useful just because it is not independent of the number of stimuli, and this limitation of the measure should be kept in mind. Only the use of a stimulus-correlation made it possible in the present experiment to determine empirically N_{ch} and Q, and the relation between them.

SUMMARY

Stimulus-density is defined as the number of stimuli per unit of stimulus-range. As stimulus-density increases, the number of changes in judgment increases also. Originally found in affective judgments, this relation likewise is found in judgments of intensity of lifted weight; it may be, therefore, a relation which is general in the field of judgment. Category-width does not vary with stimulus-density, and variability of judgment does not vary substantially if variability is measured in terms of the Q of the psychometric function. There is no contradiction between the statement that the number of changes in judgment varies with stimulus-density, and the statement that Q does not.

RETENTION OF CONDITIONED RESPONSES TESTED BY EXPERIMENTAL EXTINCTION

By W. J. Brogden, University of Wisconsin

The retention of several types of conditioned responses has been reported for varying durations of time, with and without a decrement in magnitude of the response.

Hilgard and Marquis found retention of the conditioned eyelid reaction in the dog for periods as long as 3 mo. with some decrement in the amplitude of response.[1] Hilgard and Campbell report the retention of conditioned eyelid responses in man at approximately full amplitude for periods of time up to 20 wk., the longest interval tested.[2] Wendt found retention of conditioned flexion in a dog 2½ yr. after the original training, with a decrement in the magnitude of response.[3] Hilgard and Humphreys state that there was good retention of conditioned discrimination in man at all intervals tested, from 4–19 mo.[4] Razran reports that a lapse of 16 wk. resulted in nearly complete abolition of conditioned salivary responses in humans to single stimuli, and in their reduction by two-thirds to patterns of stimuli.[5]

The purpose of the present study was to investigate the retention at 6 mo. of greatly over-practiced conditioned flexion-responses in dogs by means of experimental extinction, where in some cases the response had been maintained by shock and in others had been maintained by food which had been substituted for shock.

EXPERIMENTAL PROCEDURE

Within a soundproofed, light-shielded test-room, with *E* observing through a one-way glass screen, left forelimb-flexion to electric shock was conditioned to the sound of an electric doorbell. With four of the animals (1, 2, 7, 8) the

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* Accepted for publication August 1, 1939. From the Pavlovian Laboratory, Phipps Psychiatric Clinic, Johns Hopkins University School of Medicine. This investigation was carried out while the author was a Fellow of the National Research Council in the Biological Sciences, 1936-37.

[1] E. R. Hilgard and D. G. Marquis, Acquisition, extinction, and retention of conditioned lid-responses to light in dogs, *J. Comp. Psychol.*, 19, 1935, 29-58.

[2] E. R. Hilgard and A. A. Campbell, The course of acquisition and retention of conditioned eyelid responses in man, *J. Exper. Psychol.*, 19, 1936, 227-247.

[3] G. R. Wendt, Two and one-half year retention of a conditioned response, *J. Gen. Psychol.*, 17, 1937, 178-180.

[4] E. R. Hilgard and L. G. Humphreys, The retention of conditioned discrimination in man, *ibid.*, 19, 1938, 111-125.

[5] G. H. S. Razran, Studies in configural conditioning: VI. Comparative extinction and forgetting of pattern and single-stimulus conditioning, *J. Exper. Psychol.*, 24, 1939, 432-438.

duration of the bell was 10 sec.; with two others (4, 5) it was 2 sec. When a perfect score of conditioned shock-avoidance was obtained (20 shock-avoiding flexion-responses to 20 presentations of bell), one biscuit of food was thereafter given at the end of each presentation of the bell to which Animals 1, 2, 4, and 5 responded with left forelimb-flexion. No food was given if flexion failed. The shock was omitted altogether. These animals were continued on the bell-food sequence for 1000 trials after the last shock reënforcement. The conditioned salivary response of Animals 1 and 2, thus set up, was recorded. Animals 7 and 8 were maintained on bell and shock for 1000 trials after their first perfect score. The results obtained under these conditions have been reported earlier in this journal.[6] When 6 mo. had lapsed after the last test, the bell was presented alone to each animal 20 times each day, until the flexion-response was completely extinguished (zero response to 20 presentations of the bell). The extinction of the salivary response of Animals 1 and 2 was recorded along with the extinction of the flexion-response.

TABLE I

EXTINCTION OF CONDITIONED FLEXION AFTER A LAPSE OF SIX MONTHS

F represents the percentage of conditioned flexion-responses evoked by the 20 representations of the bell given each test-period. L represents the mean latency of the conditioned responses tabulated in the next column to the left. S represents the percentage of conditioned salivary responses evoked by the 20 presentations of the bell given each test period.

Animals

Tests	1				2				4	5	7		8	
	F	L	S	L	F	L	S	L	F	F	F	L	F	L
1	90	2.11	10	2.00	60	4.83	5	3.00	75	20	90	2.50	95	2.23
2	95	2.57	15	7.33	70	4.00	40	3.33	65	0	100	1.52	95	2.29
3	100	3.45	5	3.40	20	4.75	0		5		100	1.89	100	3.21
4	100	2.35	0		0		0		0		65	2.63	90	2.16
5	60	3.00	0								100	1.52	100	1.78
6	90	3.88	0								95	1.21	100	2.80
7	40	6.12	0								100	2.28	100	1.85
8	5	9.00	0								100	0.99	100	2.33
9	5	8.00	5	4.40							100	2.72	100	1.89
10	0		0								75	1.83	100	2.08
11											100	1.58	100	2.38
12											50	2.92	80	2.08
13											85	2.55	95	2.16
14											95	2.58	50	2.28
15											80	3.01	80	1.88
16											95	2.26	55	1.74
17											45	2.57	30	2.20
18											75	3.93	55	1.78
19											45	2.20	60	1.87
20											50	4.18	45	1.80
21											35	3.43	0	
22											75	4.18		
23											0			

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[6] W. J. Brogden, Unconditioned stimulus-substitution in the conditioning process, this JOURNAL, 52, 1939, 46-55.

RESULTS

All animals showed a retention of conditioned flexion to bell at the end of 6 mo. (Table I). Retention was good, the frequency of conditioned flexion being above 60% for the first 40 trials for all except Animal 5. The character of the conditioned flexion-response had not changed, its magnitude, frequency, and latency being approximately the same on the retention tests as it was 6 mo. earlier. The conditioned salivary response of Animals 1 and 2 was poorly retained. It extinguished much more rapidly than did the conditioned flexion-response. This inferiority of the conditioned salivary response to the conditioned flexion-response is in keeping with the results obtained earlier. During the 1000 trials of bell-food, conditioned flexion occurred on the average 33.3% more often and 2.48 sec. earlier than did conditioned salivation.[7] The conditioned flexion-response of Animal 1 was completely extinguished in 200 trials, that of Animal 2 in 80 trials, of Animal 4 in 80 trials, Animal 5 in 40 trials, Animal 7 in 460 trials, and Animal 8 in 420 trials. The most striking thing about the extinction scores is the much greater number of trials required by Animals 7 and 8 to reach the criterion of extinction. The mean difference in the number of trials required for extinction between Animals 1 and 2 (M=140, S.E.=60), and Animals 7 and 8 (M=440, S.E.=20) is 300 (S.E.=63.25). This difference is statistically significant. The p value, revealed by the technique devised by Fisher,[8] is 0.01. Such a difference as this might be expected to occur by chance only once in one hundred times. Therefore, conditioned flexion originally reënforced with shock will persist without reënforcement after a lapse of 6 mo. over thrice as long as will conditioned flexion originally reënforced with food. Since conditioned flexion reënforced with food (occurring 977 times in the 1000 trials; mean of three animals) is functionally equivalent to conditioned flexion reënforced with shock (occurring 988 times in the 1000 trials; mean of three animals), the large difference in the number of trials required for extinction at the end of 6 mo. cannot be due to an inferiority in the level or amount of learning. The difference then must be due to the time-lapse or to a difference in the functioning of food and shock as incentives. If extinction immediately after the completion of 1000 trials of additional training revealed conditioned flexion reënforced with food to be equal in resistance to extinction to conditioned flexion reënforced with shock, the difference in extinction-time after a lapse of 6 mo. would be due to the effect of the period of inactivity. If on the other hand, immediate extinction revealed conditioned flexion reënforced with shock to require three times as long as conditioned flexion reënforced with food, the time-lapse would not be a factor in the present results. The difference in the extinction times of the two would then be a function of the action of the incentives, food and shock. Unfortunately, no such control data is available. Four animals, extinguished immediately after a score of 100% was attained in the bell-food situation, required a mean of 270 trials (S.E. = 149.40 trials) to reach the zero response-criterion. Six animals, extinguished immediately after attaining a score of 100% in the bell-shock situation, required a mean of 113 trials (S.E. = 29.60 trials) to reach the zero response-criterion.[9] The difference of 157 trials (S.E. = 152.20 trials) in favor of flexion reenforced with food is not significant; such a difference might be expected by chance eighteen out of one hundred times. While this data does not serve as an adequate control, since the animals did not have the overtraining of 1000 additional trials, it gives some weight to the view that the significantly more rapid extinction of conditioned flexion originally reënforced with food is a function of the lapse of time.

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[7] Brogden, *op. cit.*, this JOURNAL, 52, 1939, 46-55.

[8] R. A. Fisher, Application of 'Student's' distribution, *Metron*, 5, 1925, 90-104.

SUMMARY

(1) Conditioned flexion responses in dogs showed good retention over a 6-mo. period. The magnitude, frequency, and latency of response did not differ markedly from the performance six months earlier.

(2) The conditioned salivary response of Animals 1 and 2 was poorly retained and rapidly extinguished, when compared with the good retention and relatively slower extinction of conditioned flexion in the same animals.

(3) The conditioned flexion-responses originally reënforced with shock required over three times as many trials for extinction as did the conditioned flexion responses originally reënforced with food. While no control data is available, the equality in the number of trials required in the immediate extinction of conditioned flexion responses reënforced with food and of conditioned flexion responses reënforced with shock points toward the lapse of time as the factor in the significantly longer extinction time of conditioned flexion reënforced with shock after 6 mo., rather than toward a difference in the functioning of food and shock as incentives.

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[9] Unpublished data of the author.

APPARATUS

AN INSTRUMENT FOR DETERMINING VISUAL THRESHOLDS

By R. M. SIMPSON and G. L. FREEMAN, Northwestern University

Since the early investigations of Arago, many instruments have been devised for measuring visual thresholds. The visual "adaptometers" devised by Nagel,[1] Piper,[2] and Ferree and Rand,[3] as well as the "thresholdometer" of Luckiesch and Moss,[4] have been widely used. We shall not concern ourselves here with details of these several instruments, but describe instead one of our own construction and its particular advantages. The instrument (Fig. 1) consists in essential of a hollow tube, one end of which carries a weak stimulus-light and the opposite end an eyepiece through which *S* observes variations in the intensity of illumination of a ground-glass field. The instrument is used in a dark room and *S* or *E* may vary the intensity of the stimulus-light by changing the density of an intervening optical wedge.

As shown in the figure, the barrel of the instrument is mounted upon a heavy bar of channel iron. The regulation watchmaker's eyepiece (1) is mounted at one end of the barrel with a metal plate of 1.8 mm. aperture serving as an artificial pupil. The calibrated neutral-tint optical wedge,[5] attached to a chain belt running over sprockets and enclosed in the aluminum housing (2), is placed 12 cm. from the artificial pupil. The transmission of this wedge varies in a gradient from 1 to 100%. It is accompanied by another wedge with its gradient set in the opposite direction, thereby maintaining uniform density relationships in the system.

The sprocket-wheels used to move the chain to which the wedges are attached are rigidly fixed on shafts (3) near the ends of the housing. The wedges may be drawn in either direction by turning the geared friction drive adjustment (4) which is attached to one end of the shaft bearing the sprocket. The 4-inch wheel of this gear carries a graduated scale (5) which corresponds to the values of the optical wedge and is moved by vernier adjustment. This scale is calibrated in terms of the percentage of light transmitted as the wedge is drawn across the field of vision.

A 5.5 cm. aluminum barrel (7) extends from the side of the wedge-housing toward the stimulus end of the instrument. At a distance of 74.5 cm. from the artificial pupil a ground-glass fixation screen is inserted in a holder in the tube (8). A round hole 1.2 cm. is drilled through the center of the two walls of the screen-holder to permit the light to pass through. The distance between the neutral-tint gradient wedge and the ground-glass observation screen is 62.5 cm. A 3.8 volt seasoned electric lamp (9) is mounted inside the barrel at a distance of 36.3 cm. from the ground-glass observation screen. The light is connected in circuit with a 1½ volt dry cell (10), a small ammeter (11) and a 6 ohm resistance rheostat (12). The ammeter serves to check the constancy of the current supplied to the light. The rheostat enables the experimenter to vary the intensity of the light as desired; use of the rheostat is essential in bringing stimulus-values of the threshold within the range of the optical wedge. An additional check upon the constancy of the light source is a photo-electric cell (13) located at the stimulus-end of the tube, 8.5 cm. back of the light filament. The throw switch (14), shown in the figure, enables *E* to break the connection between the photo-electric cell and the ammeter and to use the latter instrument for measuring electrodermal responses (ABC).

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[1] W. Nagel, Zwei apparate für die augenärztliche Funktionsprüfung: Adaptometer und kleines Spektralphotometer (Anomaloskop), *Zsch. f. Augenheilk*, 17, 1907, 116-118.

[2] H. Piper, Zur messenden Untersuchung und zur Theorie der Hell-Dunkeladaptation, *Klin. Monasbl. f. Augenheilk*, 44, 1907, 357.

[3] C. E. Ferree and G. Rand, A new type of instrument for testing the light and color sense, *Amer. J. Ophth.*, 14, 1931, 325-333.

[4] M. Luckiesch and F. K. Moss, A visual thresholdometer, *J. Opt. Soc. Amer.*, 24, 1935, 305-307.

[5] Eastman Kodak Company, No. JR-50.

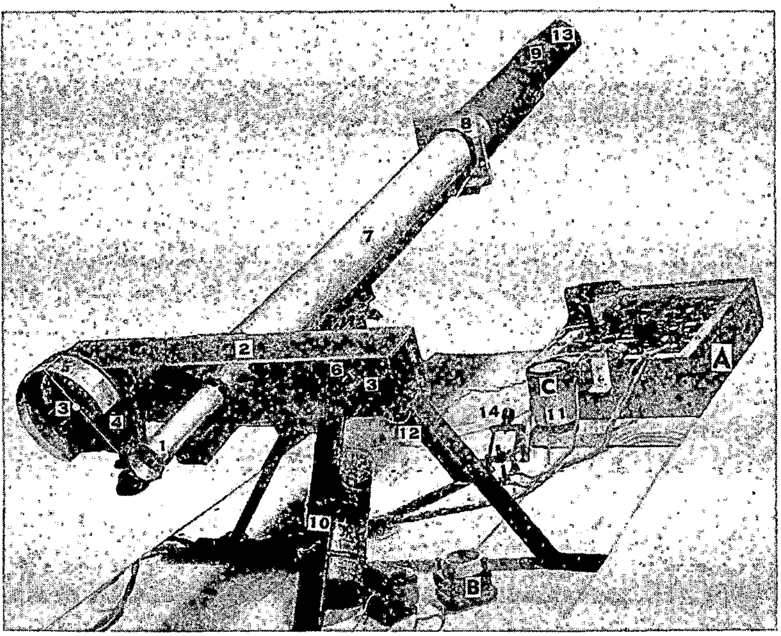

FIG. 1. THE NORTHWESTERN THRESHOLDOMETER

Stimulus-values may be easily calculated from the instrument since the intensity of the light source with 100% transmission of the wedge is 0.000,000,034 lumens and the various settings of the wedge on percentages therefrom. Among the advantages of its use may be mentioned the fact that both *S* and *E* may make the adjustments for the minimum visible, that light intensity is carefully checked and measured at the source, and that visual thresholds may be determined by various psychophysical methods.

A DEVICE FOR MEASURING DIFFERENTIAL SENSITIVITY TO LIGHT

By Carl Fisk and S. Edmund Stoddard, Blackfoot, Idaho

For four years the authors have been experimenting with a new device, a photronic cell and a sensitive galvanometer, to measure an individual's sensitivity to changes in the intensity of light. The apparatus that we have constructed and describe here is inexpensive and easily made, hence it may readily be used in secondary schools as well as in colleges and universities.

A light-proof box was constructed 88 cm. long, 22 cm. wide, and 20 cm. high. Fig. 1 shows this box placed on its side with the lid removed. A partition, D, divides the box into two longitudinal compartments. At one end of each compartment is a small circular aperture 2 cm. in diam. The distance between the centers of these openings is 5 cm. They are covered by a piece of ground glass, G, which has been removed in Fig. 1. The glass is held in place by wooden strips in such a manner that it can be withdrawn from one or both of the openings. The 7½ w. Mazda lamp, B, is stationary. The other 7½ w. lamp, A, is moved forward and backward by the rod, R. The light from the lamps is too intense, hence the lamps are covered by cylinders, C, in each of which is a round aperture 2 cm. in diam. The light from these openings falls upon the ground glass windows. The photronic cell, P, is fastened to a swivel enabling the operator to swing it easily over the window, lighted by the variable light source. As the cell is swung in place the ground glass is removed from the window.

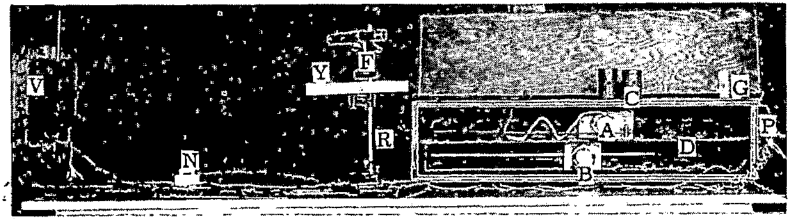

Fig. 1. Apparatus Used in Measuring Differential Sensitivity to Light
Lamp B is stationary: lamp A is variable. The light falling on the photronic cell, P, is converted to electrical energy and measured by galvanometer, V, and then translated into foot-candles of light intensity.

The photronic cell, a Weston, is non-microphonic and comes to a maximum very quickly, declining in a few minutes to its final value of approximately 1% below maximum. Changes in temperature have only slight effect on current output though a relatively large effect upon the voltage that is generated. To measure this current output we selected a Leeds and Northrup reflecting galvanometer (V) that is sensitive to amperage changes but not to changes of voltage. Its sensitivity per millimeter at one meter is 0.00075 microampere. Its period is 13 sec. The external critical damping resistance is 25,000 ohms, and the coil resistance is 1132 ohms. The deflection of the galvanometer, when knife-switch N is closed, is read on scale Y by means of the eye-piece F.

The apparatus is operated as follows. (1) The photoelectric cell is placed over the window of compartment B and the illumination of lamp B (which is 7 cm. from the window) is measured on the galvanometer and recorded. (2) The photoelectric cell is then placed over the window of compartment A, and lamp A is adjusted to give the same galvanometric reading (*e.g.* 7 cm.). (3) Then the ground glass is placed over the windows and *O* is asked to judge their relative brightnesses which is (4) altered by moving lamp A nearer to or farther away from the window. (5) The liminal setting is obtained by some one of the psychophysical methods and that value is translated into foot-candles by multiplying the setting in millimeters by the sensitivity of the galvanometer, and by dividing that product by the amount of current produced by the photoelectric cell. For example, a liminal setting of lamp A of 11.2 mm. was obtained for one *O*. This value is multiplied by 0.00075, the sensitivity in microamperes of the galvanometer one meter from the scale, and the product, 0.0084, is then divided by 1.4, the amount of current in microamperes that is produced when one foot-candle of light-intensity falls upon the photoelectric cell. The result, 0.006, is the limen in foot-candles of *O* one meter from the exposure-windows. In order that we may deal with whole numbers, this value is multiplied by 1000 and the limen expressed as 6.

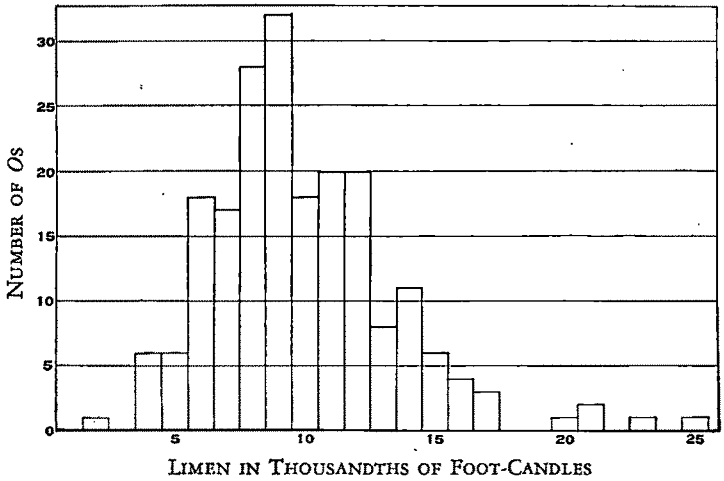

FIG. 2. BAR-GRAPH OF LIMENS OF 202 HIGH SCHOOL PUPILS

The apparatus has been used with 202 pupils of the Blackfoot High School. The results are shown in Fig. 2. The lowest limen obtained among our *O*s was 2.4 and the highest was 25.55. The median value was 9.78, with the upper quartile at 8.1 and the lower quartile at 12.28.

NOTES AND DISCUSSIONS

Size Constancy and Emmert's Law

It does not seem to have been generally stated in the literature that Emmert's law implies size constancy when certain commonly accepted assumptions are made; and conversely.[1] There may, in fact, be some confusion about the relationship, for one law tells how size varies with altered distance, and the other how it remains constant with altered distance.[2] Perhaps the experts in the field of visual space perception are all sufficiently clear about this matter, although I have found two persons, both holding that the interdependence is obvious, disputing nevertheless as to its nature. I venture, therefore, to say how the relationship appears to me, fully aware that my choice of exposition is determined by my taste in logic, and that this matter can be expounded with equal right in other ways.

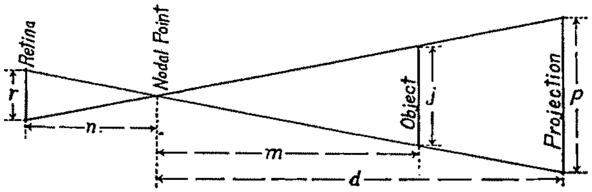

Fig. 1. The Optical Geometry of Emmert's Law

Fig. 1 represents the geometry of the physical optics that underlies Emmert's law. Its six quantities are defined as follows:

r = linear size of retinal image.
j = linear size of stimulus-object.
p = linear size of angular projection of stimulus-object on projection ground.
n = distance of retina from nodal point.
m = distance of stimulus-object from nodal point.
d = distance of projection ground from nodal point.

Now p is simply an objective size determined by the geometrical relationships of the figure, but let

$$s = \text{apparent size of } p.$$

Now Emmert's law becomes $s = k'p$

The usual form of Emmert's law is $s/d = j/m$, which expresses the geometry of Fig. 1, if $s = p$, as the law asserts. The constant of proportionality, k', reminds us merely that we are free to use any units of measurement.

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[1] On Emmert's law see E. Emmert, Grössenverhältnisse der Nachbilder, *Klin. Monatsbl. d. Augenheilk.*, 19, 1881, 443-450. It is odd that the law has received so little consideration when size constancy has received so much, but for some discussion see H. A. Carr, *An Introduction to Space Perception*, 1935, 365-368; K. Koffka, *Principles of Gestalt Psychology*, 1935, 211 f.

[2] Carr, *loc. cit.*, for instance, seems to me to be more impressed by the difference between the two hypotheses than by their mutuality.

Now it follows from Fig. 1 that

$$p/d = r/n$$

and thus by Emmert's law

$$s/k'd = r/n$$

So

$$s = (k'/n)rd$$
$$s = krd$$

The equation, $s = krd$, represents a hyperbolic surface and is the general law, since it implies both Emmert's law and the principle of size constancy. Because it is hard to visualize a surface, I have represented the surface by three sets of cross-sections in Figs. 2-4.

Fig. 2 shows how apparent size, s, varies with the distance of the field of projection, d, when the retinal size (or visual angle), r, is kept constant. Thus each line of Fig. 2 represents Emmert's law for some given retinal size; and the various lines of the figure are successive cross-sections at different r's of the surface $s = krd$. This figure does not by itself imply the general law, $s = krd$, because it states nothing about the law of variation of r.

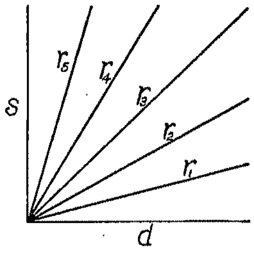

FIG. 2
EMMERT'S LAW
$s = k_1d$, r = constant

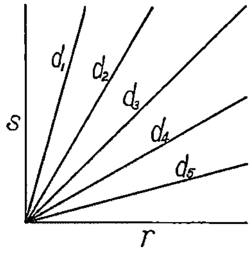

FIG. 3
DEFINITION OF APPARENT SIZE
$s = k_2r$, d = constant

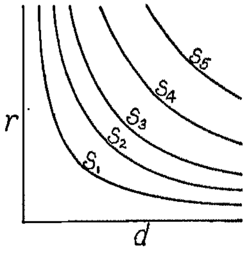

FIG. 4
SIZE CONSTANCY
$rd = k_3$, s = constant

Fig. 3 shows how apparent size, s, varies with retinal size, r, when the distance of the field, d, is constant. These relationships represent the operational definition of the magnitude of apparent size and are often overlooked, although essential to the argument. The apparent size of an object or image at various distances is measured by the physical size of a comparison object, which is kept always at a constant distance and which is equated in size to the test object or image. The lines of this figure represent successive cross-sections at different d's of the surface $s = krd$. This figure does not by itself imply the general law, $s = krd$, because it states nothing about the law of variation of d.

Taken together, however, Figs. 2 and 3 determine the surface, $s = krd$, and thus Fig. 4.

Fig. 4 shows a series of cross-sections of the surface, $s = krd$, taken at right angles to the cross-sections of Figs. 2 and 3. In other words, it shows how r varies with d if apparent size, s, is constant, and thus it is an expression of the principle of size constancy.

In general, it may be said that the three figures represent orthogonal sections of the surface, $s = krd$. Any two of them determine the surface and thus imply the third. In the foregoing discussion the principle of size constancy was derived from Emmert's law by the assumption of a definition of apparent size. Conversely Emmert's law could be derived from the principle of size constancy by the same assumption; or it could be shown that, if both Emmert's law and size constancy are true, apparent size must be proportional to retinal size when distance is constant. The mathematical derivation of size constancy (Fig. 4) from Emmert's law (Fig. 2) and the definition of apparent size (Fig. 3) is as follows:

Let s_{11} = apparent size for d_1 and r_1, where d_1 and r_1 are any d and any r; and let s_{22} = apparent size for d_2 and r_2, where d_2 and r_2 are any other d and any other r. Start with s_1, d_1 and r_1, and change d_1 to d_2, but keep r_1 constant as a parameter.

$s_{12}/s_{11} = d_2/d_1$, since r_1, the parameter, is constant. This is the function of Fig. 2, Emmert's law. Hence

$s_{12} = (d_2/d_1)\, s_{11}$

Now, keep d_2 constant as a parameter, and change r_1 to r_2. Then

$s_{22}/s_{12} = r_2/r_1$, since d_2, the parameter, is constant. This is the definition of apparent size, Fig. 3. Thus

$s_{22} = (r_2/r_1) s_{12} = (r_2 d_2/r_1 d_1) s_{11}$

$s_{22}/s_{11} = r_2 d_2/r_1 d_1$, which is the general law and may be rewritten with a constant of proportionality determining the units of apparent size:

$s = krd$

A special case of this law is

rd = constant, if s = constant, which is the function for size constancy in Fig. 4.

The other two derivations follow the same principle of treating the two variables successively as parameters, with $s = krd$ as the result. From that general law it is obvious that the three formulas of Figs. 2-4 follow at once.

The demonstration of the mutuality of Emmert's law and the principle of size constancy shows that the validation of the one hypothesis indicates the validation of the other, and conversely. It is not surprising, therefore, to find that it is both functions which fail under most conditions to be precisely correct as they are formulated here.[3] This note makes no pretense, however, of discussing the validity of either hypothesis; it seeks merely to show the nature of their relationship.

Harvard University EDWIN G. BORING

A NATIONAL ANALYSIS OF THE PSYCHOLOGICAL ARTICLES PUBLISHED IN 1939

The writer has reported several times regarding the number of titles in different languages listed in the *Psychological Index* and the *Psychological Abstracts*.[1] A further break-down of similar results would seem to be of interest in two respects. (1) The increase in the number of titles listed under the caption "All others" (*i.e.* other than English, German, French, Italian, and Russian) has considerably increased during the last few years and one may be interested in the actual distribution of languages and the number of titles in this group. (2) There is interest in the *country of origin* of the titles listed as English to determine the relative number of British and American titles and of those written by authors in other countries.

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[3] On the departure of the size-distance function from size constancy and on various factors that determine the form of the function, see B. E. Holaday, Die Grössenkonstanz der Sehdinge bei Variation der inneren und äusseren Wahrnehmungsbedingungen, *Arch. f. d. ges. Psychol.*, 88, 1933, 419-486; M. R. Sheehan, A study of individual consistency in phenomenal constancy. *Arch. Psychol.*, 31, 1938 (no. 222), 1-95. On the deviations of empirical fact from Emmert's law, see A. Noll, Versuche über Nachbilder, *Psychol. Forsch.*, 8, 1926, 3-27.

[1] S. W. Fernberger, On the number of articles of psychological interest published in the different languages, this JOURNAL, 28, 1917, 141-150; 37, 1926, 578-581; 48, 1936, 670-684.

The first question is answered by Table I. Complete results have been given for all language categories for articles listed in the 1939 volume of *Psychological Abstracts*. A total of 374 titles appear in languages other than English, German, French, Italian, and Russian. In this "All others"-group, at least 13 languages are represented and possibly more. The writer is not competent to differentiate between Swedish, Danish, and Norwegian and hence has classed all three of these languages as Scandinavian. It will be noted that there are a great number of titles in Japanese for this year—actually more titles in Japanese than either Italian or Russian for the same year.

TABLE I

BREAK-DOWN BY LANGUAGES OF TITLES APPEARING IN THE *Psychological Abstracts* IN 1939

	No.	%
Total in all languages	6557	
English	4229	64.5
German	1216	18.6
French	484	7.4
Italian	131	2.0
Russian	123	1.9
All others	374	5.7

	No.		No.
Japanese	150	Portuguese	11
Scandinavian	43	Greek	7
Roumanian	42	Czech	2
Spanish	41	Chinese	1
Polish	31	Estonian	1
Dutch	25	Finnish	1
Hungarian	19		

Table II breaks down by country of the authors the English articles. Experience would indicate that such a break-down can be done, without a tremendous amount of labor, only by a psychologist who has been familiar with the psychological literature for many years. Only such an individual can recognize a large proportion of the names of authors and also know something of their origins and present whereabouts.

The method employed in the present study was to first examine the name of the author in the hope that he could be recognized and classified as to country. If the name was not recognized, search was made in the membership lists of the American and British Psychological Associations. If still unidentified, *Who's Who*, the *International Blue Book*, *Who's Who in Education* (American) and similar biographical references were consulted. If still unidentified, one sought to find the country of origin of the author by internal evidence in the title, the abstract or the publication in which the article appeared. It would seem that if an article appeared in a publication such as the Proceedings of the Indiana Academy of Science, that one may be reasonably sure that the work was done by an American. After all of these criteria had been applied, there still remained 276 or 6.5% of the English titles which could not be identified as to the country of the author. Each of those few cases of multiple authorship by individuals of different nationality were assigned to the nationality of the senior author.

The results emphasize the volume of American work. Over 83% of all of the titles in English were done in America and certainly this number would be increased if we had been able to assign the unidentified cases. The number of British titles is relatively small—actually smaller than the number of French titles for the same year even if the Colonial and Dominion titles are added to those of the British Isles proper. Of the British titles, all but 33 were written by authors living in the British Isles.

TABLE II

Break-Down of English Titles Appearing in the *Psychological Abstracts* in 1939

				No.	%
Total listed in English				4229	100.0
Unidentified				273	6.5
American (United States)				3525	83.1
British (Territorial, Dominions, etc.)				288	6.8
	No.		No.		
British Isles	255	Australia	9		
Canada	9	South Africa	4		
India	9	British in U.S.A.	2		
Other countries				143	3.4
	No.		No.		
Germans in U.S.A.	26	Poland	5		
Germany	25	Spain	4		
Scandinavia	25	Italy	4		
Holland	14	Hungary	3		
Russia	12	Belgium	1		
China	11	France	1		
Japan	11	Czechoslovakia	1		

The largest number of English titles written by authors other than American and British is by the German group who have come to America within the last decade. In all, authors of at least 13 countries wrote articles in English which were listed in the *Abstracts* during the year 1939.

It is also interesting to note that the analysis shows that 54.6% of the titles listed for all languages have been identified as of American origin. If volume alone is considered, America is producing more titles in psychology and allied fields than all of the rest of the world put together.

University of Pennsylvania SAMUEL W. FERNBERGER

THE CORNELL SUMMER RESEARCH STATION IN PSYCHOLOGY

To meet the need, expressed by many psychologists, for a summer station at which study, research, and recreation may pleasantly and profitably be combined, the Department of Psychology of Cornell University established on trial last year the Cornell Summer Research Station in Psychology. Since the venture received encouraging support, it will be continued this coming summer for a second trial year. If the need for it is evidenced by attendance, the Research Station will be continued indefinitely.

Dr. Madison Bentley, emeritus professor of psychology and now psychological consultant on the staff of the Library of Congress, will be in attendance throughout the session. He will assist the departmental staff in placing the facilities of the University at the disposal of the members of the Research Station and in arranging seminaries on subjects selected by them.

The psychological laboratories, seminaries, libraries, and the behavior farm will be open for research and study. The laboratories, comprising 66 rooms, occupy the two upper floors of Morrill Hall. Besides the usual equipment, they contain a large soundproof room shielded for work with bio-electrical potentials as well as for acoustical studies, a constant temperature room, a wing for small animal research, another for research in psychobiology, a seminary room, individual research and study rooms, and a machine shop with the services of a skilled mechanician. The University Library, adjacent to the psychological laboratories, is adequate for historical as well as for contemporary research. Admission to the library stacks will be arranged for those requesting that privilege. The behavior farm, where most of the animal work is done, is well equipped for work on conditioning.

Ithaca, situated on Lake Cayuga in the heart of the Finger Lakes Region of New York State, provides numerous facilities for summer recreation. Swimming, tennis, golf, horseback riding, boating, and sailing are readily available. Those wishing to enjoy less strenuous activities will find many interesting walks through the numerous glens and gorges. Four State Parks (Buttermilk Falls, Taughannock Falls, Enfield Glen, Watkins Glen), the show-places of the Finger Lakes Region, are all within easy reach.

Application for admission to the Research Station should be made to the Secretary, Department of Psychology, Morrill Hall, Cornell University, Ithaca, N.Y. Those holding the doctor's degree will not be charged tuition or any other fees. They will be given the status of Resident Doctor with all the rights and privileges of University membership. Others will be subject to the usual charges of the Cornell Summer Session.

Provisions have been made for three types of work: (1) rooms and apparatus for experimental research; (2) desk-space for library research; and (3) informal study. Since rooms and desk-space are limited, early application and attendance are suggested. Assignment will be made, within the limits of the provisions available for the various types of work, as applications are received.

The Research Station will be open from June 18 to September 1. Attendance may begin and end at any time within those limits. The Cornell Summer Session runs from July 8 to August 16. Since Resident Doctors are accorded the privilege of attending the special lectures, concerts, and excursions of the Summer Session, many may find it advantageous to come during those weeks.

K. M. D.

POSTPONEMENT OF THE TWELFTH INTERNATIONAL CONGRESS OF PSYCHOLOGY

An announcement has been received from Godfrey Thomson, the General Secretary of the Organizing Committee in Edinburgh, of the postponement of the Twelfth International Congress of Psychology which was planned to be held at Edinburgh, Scotland, in July of this year. The invitation of the City and the University of Edinburgh to meet there is not withdrawn. The members of the Committee hope that the Twelfth Congress may still meet in Edinburgh when circumstances permit the continuation and completion of the preparations.

K. M. D.

THE FOURTH PHILADELPHIA MEETING OF THE SOCIETY OF EXPERIMENTAL PSYCHOLOGISTS

The thirty-sixth meeting of the Society of Experimental Psychologists was held on Monday and Tuesday, March 25 and 26, in Philadelphia at the University of Pennsylvania under the chairmanship of Professor S. W. Fernberger. It was attended by 27 members from 20 institutions, as follows: Brown (Graham, Hunter, Schlosberg); Cornell (Dallenbach, Weld); Duke (Muenzinger); Harvard (Beebe-Center, Boring); Iowa (McGeoch); Montreal Neurological Institute (Jasper); New York Psychiatric Institute (Landis); North Carolina (Dashiell); Pennsylvania (Fernberger); Princeton (Bray, Langfeld, Wever); Rochester (Culler); Rutgers (Pratt); Smith (Gibson); Stanford (Hilgard); Tufts (Carmichael); Vassar (Lanier); Virginia (Geldard); Washington (Nafe); Wesleyan (Wendt); Yale (Marquis, Miles).

Monday evening, at a banquet given to the Society by Chairman Fernberger at the Lenape Club, Professor Edwin B. Twitmyer gave an address of welcome and Dr. Ernest R. Hilgard was presented with the Howard Crosby Warren medal, the Society's award for outstanding research in experimental psychology, "for his analysis of the conditioned response and his demonstration of its integration with the verbal and volitional processes in learning and retention."

The 1941 meeting of the Society is to be held at Rutgers University under the chairmanship of Professor Carroll C. Pratt. The tentative dates are March 30 and 31.

K. M. D.

Leta Stetter Hollingworth: 1886–1939

Leta Stetter Hollingworth, Professor of Education in Teachers College, Columbia University, died on November 27, 1939, at the age of fifty-three years. She had served that institution continuously from 1916, receiving a professorship in 1930. To her colleagues in Education she will doubtless be remembered as an educator, but to her many friends and acquaintances in the field of psychology she was a psychologist. An examination of her researches and her numerous publications will clearly justify her title to a place among the eminent psychologists of her time. Even those facets of her work which were most clearly educational could rightfully be labelled clinical psychology. It is the psychologist rather than the educator to which these comments will refer.

A continuous thread of interest runs through all her scientific work, an interest in individual and group differences. An early association with the feminist movement led her to an objective attack in the laboratory upon some of the supposed sex differences held to justify the differing status of men and women in the world. Thus she studied sex differences due to functional periodicity in woman (1914) and sex differences in variability (1914). The latter problem she also attacked in an elaborate series of measurements of male and female infants at birth (1914).

Appointed in 1914 to the position of psychologist, newly created in the New York City Civil Service, she plunged actively into clinical work in hospitals, schools, and courts. Her research interest centered upon the characteristics of mental deficiency and of special mental disabilities. Her investigations and publications in these fields during the years 1914-1920 were systematically presented in *The Psy-*

chology of Subnormal Children (1920) and in *Special Talents and Defects* (1923). In the latter book there was foreshadowed her major interest of later years, the superior child.

Her critical insight in dealing with the problem children that came under her observation shortly disclosed to her the fact that poor adjustment is due not to defective intelligence alone but that emotions and attitudes play in it an important rôle. Their influence appeared to be particularly striking during the adolescent period, so that this age-group became the object of attack in her teaching and research. Courses on Mental Adjustments and Adolescence were given by her at Teachers College for many years. Ten articles on various aspects of adjustment were published, and the results of her years of study were presented in *The Psychology of Adolescence* in 1928. This is still a standard textbook in the field of mental adjustment.

The discovery of highly intelligent children among the maladjusted magnified an early interest in the superior child and led to the program of research which became her major life work. Although, out of her seventy-five published articles, some forty-five are devoted to the subject of the superior child, and one book, *Gifted Children* (1926), envisages the field as a whole, her work in it had really just begun. A survey of studies in progress and plans for the future gives some hint of the scientific tragedy of her death. To be sure, some of the material in manuscript can and will be prepared for publication, and some of the research under way will be completed, but the more ambitious parts of the program will scarcely survive the loss of her intelligent, enthusiastic and courageous direction.

A few of the uncompleted projects of particular scientific interest may be mentioned. One of these had to do with the development and future careers of gifted children. About 1920 Mrs. Hollingworth arranged with officers of the New York City school system for the segregation of a selected group of intellectually superior children less than ten years of age. They were intensively studied and their development followed over a period of twenty years and into adulthood. Such a project could never end as long as any of the original group survived. Even then their children and their grandchildren would be fruitful subjects for follow-up research.

One of her discoveries (1930), about which she was much concerned in her later work, was the fact that superior intelligence does not preclude serious maladjustments in the home, school, and later life but on the contrary carries with it peculiar hazards of its own.

An experimental school, established in February, 1936, by the New York City Board of Education and known as P.S. 500 or the Speyer School, with Dr. Leta Hollingworth as Director of Research, was a direct outgrowth of the earlier project, as well as a testimonial of its achievements. Plans were made for five years of research. The bright and dull child were both represented in this school, where unique methods and subject matters were put to the test and their effects measured. Many reports of the work of this school have already been published, and follow-up data should accumulate and be reported at intervals over many years.

Among the many studies in progress and incidental to this larger program, some educational and some psychological, were a sociometric study of gifted children in the school situation, a physiognomic study of the profiles and full-face photographs of gifted and slow-learning children, an investigation of the hands of gifted children, using objective measurements of dimensions and proportions, and a study of the age at which demonstrable race attitudes begin to appear in white and negro children of various degrees of intelligence.

What might well have been the crowning scientific achievement of a life devoted to study of the exceptional child, when viewed in the light of generations of time and in the light of magnitude and extent of its influence upon society, was a plan for the establishment of a philanthropic corporation tentatively labelled "Superior Foundation." Its intended purpose was "to achieve a substantial endowment, to be directed intelligently toward the discovery, education and conservation as natural resources of the gifted young." What foundation could have a nobler purpose, when today the world bemoans the death of intellectual leadership and faces a future in which only intellectual leadership of the highest order can solve its problems?

This bare recital of scientific aims and achievements leaves untouched the record of a kindly sympathetic and at the same time vigorous and courageous personality. Her life began under frontier conditions in the middle west in an environment of sod houses, long-horned cattle and roving Indians. She had her introduction to education in a one-room log school house, a schooling which she later came to appraise as excellent in every respect. She entered the University of Nebraska at the age of sixteen, and graduated in 1906 with Phi Beta Kappa. She taught in a Nebraska high school for two years, resigning to be married to H. L. Hollingworth. From that time her life shifted to New York City where her husband was an assistant in the Department of Psychology at Columbia University.

She began graduate work at Teachers College and received the A.M. degree in 1913. Following this she held a part-time position in mental testing at the Post-Graduate Hospital and continued her work toward the Ph.D. at Teachers College. The degree was awarded in 1916.

Her scientific work on sex differences was accompanied by an active support of the feminist movement, which led her to march in the early suffrage parades and to serve as watcher at election polls. Her interest in the measurement of mental defectives was paralleled by participation in the active controversy as to who was properly fitted to give and interpret the tests. She vigorously defended the cause of clinical psychologists and was a leading spirit in building their professional organization and in raising the standards of mental examiners in universties and in the Civil Service. She did not hesitate to appear before school boards and state legislatures to plead her cause.

In her later years she became an ardent champion of the superior child and fought for his recognition and for a training program which would at the same time make the most of his intellect and enable him to develop a well-rounded personality. She gave most generously of her time and her personal funds for the support of research when institutions delayed or refused financial support. She followed the career of every one of her superior children, entertained them in her home and aided them financially when she felt such help was needed. Only her closest associates could know that no funds, though often sought, were ever received by her in support of her research.

While her personal sacrifices were heavy, she received her reward in the respect and affection of her superior children, in the esteem of her colleagues, and in the international recognition of her achievements.

Columbia University A. T. Poffenberger

Livingston Farrand: 1867–1939

Best known for his later contributions to public health and university administration, Livingston Farrand is remembered with appreciative affection by the older psychologists. For no less than nine consecutive years, beginning in 1896, he was Secretary and Treasurer of the American Psychological Association. During this period he was instructor and professor of psychology in Columbia University. For the last twenty-five years of his life he became an administrator, serving as President of Cornell University from 1921 to 1937. He died on November 8, 1939.

Born on June 14, 1867, graduating from Princeton in 1888, Farrand then attended the Columbia medical school. During his medical course he decided not to engage in the practice of medicine but to enter the field of education. His first step was to go over to the psychological laboratory, just established by Cattell, and to devote part of his senior year at the medical school to a special course in experimental psychology. The two following years he spent at Cambridge University and the University of Berlin, preparing himself for work in physiological psychology, and in 1893 he became instructor in this subject in Columbia University. He soon added anthropology to his academic offering and later came to concentrate his scientific interests entirely in the latter subject. He taught at Columbia for twenty years, the first ten in psychology, the last ten in anthropology. His courses in physiological psychology and ethnology were much appreciated by the early graduate students of psychology at Columbia.

He collaborated with Cattell (1894 *et seq.*) in setting up at Columbia what were known as the "freshman tests," a selection of anthropometric and psychological tests made each year on a sample of the entering college class, the purpose being to accumulate data for the study of abilities and personal traits, their development and correlation. This enterprise was carried on for many years and gave rise to some of the earliest correlational studies.

Meanwhile Dr. Farrand's ability as an administrator and organizer was becoming manifest and his services were enlisted, first for part time and then for full time, in public health enterprises and later in university administration. His personal charm, his quiet humor, his tolerance and understanding, his unostentatious courage, enabled him to bring men of divergent points of view together in harmonious co-operation, and to handle delicate and serious situations both in the War and before and after. The psychological fraternity is proud to have a share in honoring his memory.

Columbia University R. S. Woodworth

Charles Augustus Strong: 1862–1940

Dr. Charles Augustus Strong was born at Haverhill, Massachusetts, on November 28, 1862. After studies at Rochester, Harvard, and Berlin, he taught at Cornell, Clark, and Chicago, and for a longer period (1896-1913) at Columbia. His courses at Columbia were on "Analytic Psychology" and the "Philosophy of Mind." His interest was in the philosophical aspects of psychology, as shown by the titles of his important books, *Why the Mind has a Body* (1903) and *The Origin of Consciousness* (1918). For many years prior to his death on January 23, 1940, Dr. Strong had resided near Florence, Italy, devoting himself to philosophy.

Columbia University R. S. Woodworth

BOOK REVIEWS

Edited by JOHN G. JENKINS, University of Maryland

The Factorial Analysis of Human Ability. By GODFREY H. THOMSON. New York, Houghton Mifflin Co., 1939. Pp. xv, 326.

All who follow the development of factor theory or methods, or who apply the factorial techniques will surely wish to read this volume. Its author is well known as an early critic of Spearman's theory and as the originator of a rival conception of mental ability known as the sampling theory. In the course of years he has also been a contributor to the underlying mathematical rationale and to the development of computational devices in the field of factor analysis. In presenting an organized exposition of extant theories and methods, the author brings to bear his own experiences and contributions.

Because computational procedures in factor analysis have been reduced to routine steps, many investigators have been enticed to employ those procedures, too often without the restraints imposed by a more thorough knowledge of the underlying mathematical theory and of its applicability. Fortunately, Thomson gives very clear accounts which can be grasped without the aid of greater mathematical sophistication than is afforded by elementary algebra, geometry, and trigonometry. A very brief mathematical appendix is provided for those who are on terms with advanced algebra.

Early and frequently the author pays deep respects to Spearman, and like most so his countrymen, holds the conviction that a *g* factor will always be found among tests of ability, no matter whatever else may or may not be found. Says Thomson, "The main idea which still, rightly or wrongly, dominates factorial analysis was enunciated by him, and practically all that has been done since has been either inspired or provoked by his writings" (p. 5). As forecasted in his earlier writings, Thomson harmonizes his own theory with the *g* hypothesis.

In early chapters the theoretical bases for the two-factor, sampling, and multiple-factor theories are laid. In every case, the assumption is made that any correlation between two tests is due to a great many small, identical elements, a conception of correlation which is at least as old as Poisson but which few writers accept explicitly. For Thomson, the psychological elements, or 'bonds' are the real constituents of mental ability and this conception dominates his theory. All other views are to be reconciled if we admit that the factors are "only statistical coefficients, possibly without any more 'reality' than an average, or an index of the cost of living, or a standard deviation, or a correlation coefficient—though on the other hand, it may be that some of them, Spearman's *g* for example, may come to have a very real existence in the sense of being both useful and influential in the lives of men" (p. 42).

His main support for the unreality of factors is given in his discussion of the effects of sampling. The factors and their loadings, he maintains, have no absolute meaning but are functions of the selected population of individuals and of tests. With this result few factorists would probably disagree. Different factors and different loadings do arise from different samplings of individuals, and in some methods, from different samplings of tests. But the conclusion is not inevitable that factors are therefore not 'real.' His elemental 'bonds' of the mind, however, he maintains are real, and are associated somehow with the "one hundred thousand million (sic) nerve cells" in each normal brain. On the basis of these atoms of ability one should expect Spearman's criterion for a single common factor to hold, particularly when a sufficiently large number of bonds are brought into play in tests. There is apparently no retreat from Thomson's early position that "the laws of probability alone will account for a tendency to zero tetrad differences among correlation coefficients" (p. 47).

The geometry of mental abilities is presented in describing the various theories. Procedures for computing factor loadings by the methods of Spearman, Thurstone, Hotelling, and Burt are described. Holzinger's work receives but slight mention. Thomson points out that Spearman's and Thurstone's methods maximize the importance of specific factors in their attempt to find as few common factors as possible, whereas the method of principal components minimizes the specifics in favor of the common factors. He warns that a really accurate solution of any factor problem is impossible because, considering all the specifics and error factors, there are always more unknowns than there are equations. He believes that Thurstone's centroid method, assuming as it does the criterion of simple structure, will fail to find a *g* factor and that the admission of oblique axes defeats the desire of the factorist to substitute a few uncorrelated variables for the original correlated test variables. He admits the validity of Stephenson's inverted analysis, in which persons are correlated instead of tests, but does not agree that the factors thus obtained will coincide with the usual ones unless we start with covariances instead of correlation coefficients.

The book is full of information about factor analysis, and in spite of rapid developments in this field will serve as a standard reference book on the subject for some time.

University of Nebraska J. P. GUILFORD

The Logic of Modern Psychology. By CARROLL C. PRATT. New York, Macmillan Co., 1939. Pp. xvi, 185.

Fundamental problems of psychology are examined thoroughly and critically in this volume. Anyone who has made a serious attempt to clarify his own concepts of the scope and methods of psycholgy will receive Pratt's considerations with interest.

In a lucid and often humorous style, some of the most troublesome psychological questions are reviewed: (1) What is the subject matter of psychology? (2) What is the relation of the various schools? (3) Can scientific psychology be coördinated with the layman's common sense observations? (4) How can satisfactory definitions of psychological concepts be arrived at? (5) In what relation does psychology stand to physiology? (6) What are the limitations and possibilities of hypothesis? (7) What are the boundaries of psychology as science? (8) To what extent may psychology be applied to practical problems?

The introduction states briefly the author's answers to these questions as the theses upon which the body of the book elaborates. Had one chapter been devoted to each problem the organization of the exposition might have been improved; as it is, however, the answers are all present.

(1) The subject matter of all sciences is the same. There is no identifying mark on the observational data of psychology which sets it apart as a study of 'mind,'

a *bete noire* which is competently disposed of. (2) Much less is there any real division between the systems which have arisen within psychology itself. A brief but pointed consideration of 'schools' since Wundt successfully maintains this position.

(3) Layman and philosopher are taken for a rough ride over fallacies in their confused concepts of self, intuition, and meaning. The author concludes that what the psychologist does with introspective data, his "manipulation of data by the instruments of logic," does indeed justify his recognition as a scientist and differentiates his work from the subjective considerations which go to make up literature or philosophy. Science is differentiated from casual observation by its careful description of the methods which produced the data and by the fact that both methods and data are public knowledge. "If scientific knowledge could be regarded as identical with the mere having of experience the task of science would be simple indeed. Every man would be his own scientist, and all of his experiences would immediately become true items in the general body of scientific information." Experiences, however, can be made public only by the use of symbols, words, and it is in communication that many all-too-familiar difficulties arise. (4) Pratt's consideration of these symbolic words, concepts, and their misunderstanding is pertinent, his analysis of discrepancies between facts and concepts is clear, his suggestion for avoiding the pitfalls of loose word-meanings follows Bridgman's scheme of operationalism, *i.e.* the equation of word-meanings to the operations and observations which led to the use of the words. Considerable space is devoted to the subject of meanings and definitions and comprises one of the most interesting and valuable sections of the book.

(5) Objections which have been raised against the tendency of psychology to move in the direction of physiology are stated and refuted as Pratt moves to the conclusion that "psychology is in a position to construct its own formal physiology without waiting for material physiology to catch up with it." His argument is well supported and convincing. (6) In elaborating this argument he clearly indicates the importance of the hypothesis in science and exemplifies the construction of logical theory on the basis of observed facts in Sherrington's description of the synapse.

(7) In defining scientific knowledge as the understanding of "conditions which underlie a thing and the quantitative influence which these conditions exert" the author simplifies the task of delineating psychology from common sense and from some disciplines which have been confused with scientific psychology. Freud, for example, is recognized as a literary psychologist, not a man of science. The value of statistical surveys and the case history method is, quite legitimately, questioned. This discussion then leads easily into the answer to the last question, (8) What applications of psychology are possible? Having pointed out "that "the important determinants of man's higher activities are inside of him . . . (hence) to apply the method of concomitant variations to them is still out of the question" it follows that interested onlookers shall be disappointed when they seek aid from the psychologist in solving problems of social adjustment, leadership, education and the like. "Treatment of maladjustments," Pratt observes, "is more of an art than a science." It is unfair and dishonest for the psychologist to make great claims for the efficacy of his science in these lines. While admitting that the ultimate justification for psychology is in its practical contributions he emphasizes that everything is to be gained by sticking to pure science for the nonce. He voices the opinion of many a

psychologist in saying that "if the general public is not fully aware of what is going on there (in the library and the laboratory), so much the better."

While Pratt's theses may have been shared by others for some time, his well thought out discussion is useful for its survey of vital problems and for crystallizing points of view. Though the internal organization of the book is not always as clear as it might have been (chapter headings and subheadings could be changed to lead the reader more easily from one main question into another) the book is on the whole coherent, stimulating, and well worth reading.

Colorado College MARTHA LOU LEMMON

The Failing Student: A Study of Academic Failure and the Implication for Education. By KENNETH L. HEATON and VIVIAN WEEDON. Chicago, University of Chicago Press, 1939. Pp. x, 286.

This study was made "under the supervision of the Coöperative Bureau for Educational Research as an aid to institutional improvement in Michigan colleges" (p. v). It concerns itself chiefly with the failing group of students in four Michigan colleges: Albion, Central State Teachers, Michigan State, and Olivet. The group selected numbered 938 persons, or about 12% of the total enrollment in the four institutions.

It was found that failing students do both good and poor work in every branch of study. As judged by the American Council aptitude test, one-third of the failing group ranked above the average. As to reading ability, though 39% ranked in the lowest quartile, these students did not necessarily have low scores in the aptitude test. Individual examination revealed many instances of imperfect study habits and skills, from which the conclusion is drawn that the college should identify individual differences at entrance and thereafter individualize the instruction.

As regards finances, vocational motivation, physical defects and general health, personal and social factors which might impair study, there appeared no notable difference between the failing group and successful students.

Women students did not rank much higher than men in the aptitude test, but the proportion of men in the failing group was much larger than that of women. From this fact the authors conclude "that men students are facing a problem of adjustment which is apparently more difficult than that faced by young women" (p. 207). No reason is given for this difference.

The most illuminating chapter deals with "College Experiences as Appraised by Students," in which reports were obtained not only from the failing group but from an equal number of successful students. From these appraisals we learn, among other things, that "many required courses seem unimportant to students" (p. 211). "Student interests are followed in planning some courses and neglected in others" (p. 213). "Students are essentially non-critical of materials and subject-matter, and seem rarely to know of materials other than those provided or recommended by the instructor" (p. 214). "There is very little evidence that students have thought of college courses in relationship to permanent interests" (p. 217). "Courses that seem of little or no value are likely to be neglected, while those that seem important are easier ones on which to work" (p. 218). "A majority of students feel that there is a lack of variety in teaching methods" (p. 221). "Most students think that discussion should be an important part of college courses" (p. 224). "Few students are familiar with what is commonly called 'self-appraisal' " (p. 229). "Dishonesty in the securing of grades is considered common" (p. 229). "Students turn more often to fellow-students than to faculty members for guidance" (p. 240).

Perhaps no clearer evidence could be had of the failure of the college to educate its students than is recorded in these comments. For those who maintain "that the college exists for intellectually superior students . . . it is disturbing that so many potentially superior individuals are falling behind in the struggle for an education" (p. 245). The authors believe "that the college does not exist solely for the superior student but that it has a contribution to make to the general education of a large body of the total population . . . and that it has at least a limited responsibility for all who may care to continue study beyond high school and also may wish, thereby, to push back the personal and social horizons of life" (p. 246).

Although the authors are hopeful that improved methods of guidance may achieve these ends, we learn little from this inquiry which is calculated to warrant such hopefulness. It does not seem to have occurred to the writers that without a genuine interest in scholarship it is impossible to pursue a higher education with profit. No matter how much we know or may be able to learn of individual differences, no matter how much we may individualize study with regard for these differences, unless the individual should awaken to his own need for knowledge and mastery of his subjects he will continue as he now does to 'get by' with a minimum of effort and understanding. Without insight one gains no understanding of anything, and without a command of linguistic, mathematical, and other tools of expression, one gains no insight which will "push back the personal and social horizons of life."

The implication of this study for education, though it is nowhere precisely stated, is that "though you may send a boy to college you cannot make him think." A further implication is that in our effort to educate everybody we are perilously near the point of educating nobody. If the failing group of students includes many persons who are potentially as able as those who succeed, the problem that confronts us cannot be solved by guidance, but only by discipline. In a process of mass-education which neglects the exacting discipline of a strict training in linguistic, mathematical, and graphic means of expression no one can be properly educated. Only with the aid of 'tools' of knowledge like reading, writing and speech, calculation, the graphic arts and the ability to construct things, is insight afforded which will provoke thought and lead onwards to the competency of the educated person.

So long as your young people are sent to school to mark time until they are old enough to find jobs in the world, the motivation essential to an education will be weak. Unless we can devise means for making the acquisition of the 'tools' of knowledge strict and disciplinary we shall continue to ruin the prospects both of those able and of those unable to profit by the well attested subjects of higher education.

Cornell University R. M. OGDEN

Mental Tests, Their History, Principles and Applications. By F. N. Freeman. Revised edition. New York, Houghton Mifflin Co., 1939. Pp. x, 460.

The reviewer, being no expert in psychometrics but, like many another, confronted with the problem of choosing a textbook suitable for a college class in measurement of intelligence and personality, proposes to evaluate the present book from that angle. This means that he will not judge it as an advanced or exhaustive treatise, which it is not, nor as a manual to be used by mental testers, which few students become. In fact, no book that demands a technical knowledge of statistics would do. He will, however, expect it to be as up to date as possible in this rapidly developing field, and to give an accurate and comprehensive survey of mental tests as they are currently used. He will expect some evaluation of the tests discussed, and some guidance in their critical use, in view of the tremendous range of difference in the validity of these various measuring instruments, and in the significance attached to them by expert opinion. Finally, he will look for an adequate theoretical discussion of the nature of intelligence and personality to give the background for interpreting the whole testing movement.

To what extent has the present text met these requirements? Knowing the author, one would expect a conservative, cautious and unbiased treatment. This expectation is, with some exceptions, borne out, especially so in Chapters I, XV, and XVI, which deal, respectively with the present status, meaning, and fields of application of mental tests; the interpretation of mental tests; and the nature of ability. No misguided layman, prejudiced either by an over- or under-confidence in psychological tests, could possibly read the sane appraisal contained in these chapters without profiting greatly by it. Both the limitations and advantages of the method are plainly presented. A good bibliography of texts in the field is appended to Chapter I.

Students often puzzle over Spearman's tetrad difference method of proving his two-factor theory of ability. This and other uses of correlation are presented in such a simple, elementary way in Chapter III as to be clear to anyone. The same can be said of Chapters X, XI, and XII, which discuss "the technique and theory of mental tests," meaning such problems as the selection of materials and organization of items, and the setting up of norms, and comparison of various types of scores. The handy table of equivalent measures of brightness on page 302 is credited to F. A. Kingsbury.

The discussion of the various types of intelligence test proceeds historically and developmentally, with clear differentiation of age and point scales, language and performance tests, etc. The chapter on "Tests for the Analysis of Mental Capacity" deals with special abilities tests, like those for mechanical, musical, and art talent. The instructor would need to supplement the brief descriptions with a much fuller analysis to make this part of the book effective.

Chapter VIII, dealing with tests of personality traits, is hardly adequate. Here, if anywhere, is needed a highly critical evaluation. The discussions of tests like the Vineland Social Maturity Test, and the Willoughby Emotional Maturity Test, are too sketchy, considering that these are prototypes along the lines of which further tests of these traits will in all likelihood develop. Granted the excuse that they are too new to be properly sized up, how about other earlier experimental attempts like the Downey Will Temperament Test? We find only casual mention of the fact that the consensus of opinion has relegated it to the discard; yet its validity is defended purely on the basis of the findings in the author's identical twin study in which four out of nineteen profiles came out nearly identical. What does this prove? One also looks in vain for a discussion of the logic of types and traits, which is practically indispensable to an understanding of the reason for the rejection by experts of the whole type theory and of the tests designed to isolate types. A good selected bibliography of tests of personality is appended. If supplemented by collateral readings, the book is quite usable.

University of Cincinnati A. G. BILLS

Psychology for Business and Industry. By HERBERT MOORE. New York, McGraw-Hill Book Co., 1939. Pp. xi, 527.

This book purports to cover the field of business and industrial psychology. Actually, of the fifteen chapters, thirteen deal with personnel (employment, testing, training, promotion, industrial relations), and the remaining two with advertising and selling.

Moore states that his purpose is to bridge the gap between laboratory findings and practices which have been found to be efficacious in business. Before the reader arches his eyebrows at that heresy, let him think of this from a standpoint of validity rather than of reliability and laboratory controls. Too much has been written by theorists who are unaware of practical limitations. A little experience in selling, advertising, or doing personnel work would have changed much that has been written.

The author made an excellent choice in opening his discussion of personnel procedures with figures on the extent and cost of turnover. Often this is one of the concluding points, yet nothing will so impress one with the necessity of proper selection as to know what a mistake will cost.

It is possible that too much space is devoted to tests, particularly since the author gives such an excellent critical evaluation of their uses, misuses, and shortcomings. Chapter VI has twenty-six pages on "Introducing and Constructing Tests." This will probably not be of much interest to the majority of readers, whether students or persons in industry, as not many have occasion to devise new tests. The same criticism might be directed to the lengthy discussion of rating scales in Chapter VIII.

Two whole chapters and parts of several others are devoted to employee relations. The 'jazz era' of the 20's was the testing decade in personnel. If the reviewer reads the signs rightly, it seems as if the trend in the 30's has been toward dealing with the worker after he has been employed. Chapter XIII, "The Problem Employee," is novel, interesting, and valuable. Not only ethically are mental hygiene services preferable over cold-blooded dismissal, but from a selfish viewpoint a concern will save by straightening out an emotionally maladjusted worker than consuming time and money breaking in his successor.

The chapter on accidents goes into the causes, or attending circumstances, very thoroughly. It is perhaps unfortunate that more space was not devoted to prevention. This shortcoming is characteristic, however, not only of this book, but of nearly all in this field.

The last two chapters, "Advertising" and "Selling," are tag-end. After thirteen chapters dealing with various aspects of personnel, two on these important topics seem too scanty. Both chapters miss the mark. In advertising, the psychologist should provide his most valuable contribution in the selection and use of appeals (motivation). The problems of type face, trade names, color, etc., seem more matters of engineering. Moore has dealt with them in the form of psychological problems, but often a second careful reading is necessary to uncover this. Discussion of testing advertisements is good and to the point, and not too lengthy. Most of the space in the final chapter is spent on the personality of the salesman and on other points not squarely at the heart of the problem. Selling, the reviewer believes, is primarily a face-to-face meeting between salesman and customer—even though this situation is less objective and less amenable to statistical treatment.

University of Wisconsin R. W. HUSBAND

A Basis of Opinion. By ADRIAN COATES. London, Macmillan Co., 1938. Pp. xvii, 457.

The central theme of this book is that people exist, and only things are real. Philosophy is defined as a reasonable point of view. Verbal affirmation is made in regard to the common-sense point of view. The author's own way of looking at philosophy is, of course, the meaning of common sense applied to philosophy. No claim is made to impartiality; but admission is made of a life long prejudice against mysticism, transcendentalism, and the claims of revealed religion and authority. The text throughout is a presentation of the personal, analytical view offered as a substitute for the common, objective-metaphysical outlook.

Things have no transcendental essence. Their essence is in their definition. Reality is given in experience. He supposes transcendental realities are merely poetical devices or pitfalls of hypostatization having only conceptual reality. Mind is the potentiality of action. The expression, "in the mind," refers only to remembering or being thought of. The real nature of a thing is in the nature of our perceptional experience as a whole. Reality and perception are co-relative terms. Reality is 'thingness.' "An object is real," Coates states, "because we all see it as the same object and the same kind of object." The idea that science is a search for reality must be abandoned. The universe cannot be an object of perception since the 'universe' cannot be distinguished as an 'entity' from the "universe or totality of our sensible experience." Thus things are resolved into events, material substance into eventuality. Eventuality, in place of reality, is the ultimate and all-inclusive term within the context of sensible experience.

Persons only can be said to exist. All the 'nows' or present moments and the activities of the selves are the context of actuality. The distinction between the "subject" and the "object thought of" is only logical. The subject is one part of the thinking self while the object thought of is another part. Even time, that is, real time, is a projection of actual time into the realm of logical thought and objectivity. Coates' distinction of the actual and the real seems to be a parallel of the psychological distinction of structure and function.

The concepts of causality, purpose, and freedom remind the reader of a dynamic view of behavior. In regard to causality, the main types of questions are: (1) Why is it? (2) Why or how did it happen? (3) Why did he do it? One's end or purpose supplies the reasons for his actions. Final causes belong only to the sphere of individual activity. To say that a man is free to say that he acts as he wills.

Facts are defined as being wholly within the realm of experience. To quote, "A fact is what is known to have happened. . . . Scientific fact is the experiment or controlled experience of a person working toward a definite end of scientific knowledge." Truth and knowledge are characterized in terms of certainty and conviction. As facts become more general, they tend to pass into generalizations. In belief, the degree of certainty is contextually emotional or volitional. All religious and political belief has the emotional and volitional basis. Belief is essentially an act of will. Intellectual certainty is the basis of all knowledge.

History is more inclusive than reality. Reality is an aspect of history. We know and contemplate the past; but what we belong to is the present. The transcendental basis of history is to be found in the existence and activities of other selves. We cannot see what kind of a world we are building.

The system presented in the book is a philosophy which repudiates most of the philosophy and religion of the past. Religion and morality are regarded as being unrelated. Pragmatic value is attributed to religious concepts. Training of character is considered not as a matter of creed but of personal example.

In the main, the author is faithful to the dynamic structure-functional psychological standpoint. The manner of writing obscures somewhat the behavior situations which evidently are the background of the analysis. Throughout, the argument is consistent with the central theme. To the psychologist, the book is a sound philosophy which does little violence to the major current psychological concepts.

University of Utah M. C. BARLOW

Psicologia del Comportamento Sociale. By A. MIOTTO. Florence, Vallechi, 1939. Pp. 151.

Psychologists and sociologists have long tended to disagree in their analyses and evaluations of the relations between the individual and the social group. In extreme cases, the psychologist has denied the reality of the group; the sociologist, the reality of the individual; or, in any event, there has been a tendency to subordinate the one to the other by declaring that the individual is merely a function or product of the community or on the other hand that society is but a sum or aggregate of individuals. The evolution of this difference in point of view, says Dr. Miotto, is very interesting and the disagreement has assumed a precise form as a result of recent studies of animal sociology. Compromises between extreme doctrinal positions are impossible, and the purpose of Dr. Miotto's work is an unprejudiced examination of social behavior as a function of both individual and environment, and an attempt to define more precisely the nature of these terms.

The introductory chapter, treating instinct and affective life, aims to demonstrate the equal and reciprocal influence of individual and environmental factors at every level of biological and sociological complexity. In short, individual and environment, apart from one another, are mere abstractions. The concrete reality is social behavior.

For the purposes of analysis, instinct and reflex are regarded as belonging to the same category and are closely connected with emotion as the concluding phase of a process of excitation followed by tension and distension. Emotion in its primitive form is the emotion of static equilibrium and the biological process is in essence reaction or disturbance of the organism characterized by reflexes and instincts, followed by return to equilibrium experienced and exhibited as emotion. Such a view disposes of vitalistic and finalistic theories of animal behavior and places equal emphasis on the two poles of behavior, individual and milieu. In short, the primary activity of the organism is largely adaptation to itself—adaptation to its own structure and system of functions, in a word, the maintenance of equilibrium. But it is not solely this. At the level of psychological process a similar tendency toward equilibrium appears. The author calls it the statics of the mental life and cites

Spinoza, Freud, and Cannon in defence of the conception. The existence of mental functions extends and complicates the environment, finally creating a true social environment which becomes indispensable to the very actualizing of psychological functions. Thus individuals and milieu are equally real and necessary conditions of behavior.

Such facts lead the author to a discussion of the importance of the environment. It is clear that the continually increasing complexity of human behavior cannot depend exclusively on the individual as a biological unity since all of his activity is in essence a return to equilibrium. But from what does he return? From the environment? A vague term which needs to be very precisely defined! Indeed it is here that Dr. Miotto feels that the sociologists have erred and in failing to define and classify environment, have invited the attacks of psychologists who encounter a misuse of the term and may themselves be led astray by such misuse.

Sociology has gone on false paths through failure to analyze the notion of environment and define its types, by an obsolete conception of the process of adaptation, by the substitution of teleological speculations for observation and experiment, by abuse of the method of the biological sciences, and the reduction of sociology to a species of social physiology. The principal errors, however, are inadequate notions of the environment and adaptation and these occupy the author for the remainder of the book, but chiefly the former. With a remark that biological adaptation can no longer be defined in Lamarckian terms, Dr. Miotto turns to a classification of environments and a discussion of their inner relations and significance to social behavior. He classifies environments as cosmic, organic, social, and psychological and in chapters three and four, he offers a condensed but well documented account of their respective rôles.

Dr. Miotto next discusses the so-called social tendency of man and rejects various efforts such as that of Freud to reduce it to a single primary tendency or drive. Inter-attraction, imitation, and a sentiment of participation all contribute to impose the particular forms assumed by the forces tending to establish equilibrium between the individual and his environments.

The final chapter is devoted to the reality of the socialized individual. The general conclusion based on analyses of what seem the most individual experiences, such as dreams, mystic states and the inventive process, is that the individual is inevitably socialized. Society is woven into his essence and he could not abstract himself from it if he would. In conclusion it may be said that Dr. Miotto's work is logical, remarkably condensed, and embodies criticism equally valuable to psychologists and sociologists.

University of Cincinnati CHARLES M. DISERENS

Life: A Psychological Survey. By SIDNEY L. PRESSEY, J. ELLIOTT JANNEY, and RAYMOND G. KUHLEN. New York, Harper & Bros., 1939. Pp. 654.

The authors consider that "psychology has a primary obligation humbly to contribute whatever little it can toward human welfare" and have developed their book around the theme of this objective. They offer a comprehensive survey of sociological, economic, and biological facts as the setting for human life of today. They relate these facts to the developmental cycle which forms the common denominator of all individual life stories. Finally, they provide numerous suggestions whereby each person may turn to his own best account the outer and inner characteristics of his life.

Part I deals with "The Conditions and Circumstances of Life." Here the cultural as well as the material environment is analyzed swiftly but carefully and recent changes are described in the social and economic condition of women, in the application of humanitarian concepts, in recreational life, in manners and in science and its point of view.

Part II traces in succession the life span developmental progressions of an individual through physical, intellectual, recreational, social, vocational, and ideological experience. This section closes with a diversified collection of case stories of people who typify the different kinds of life patterns made possible by modern living conditions.

Part III deals in detail with psychological mechanisms of adjustment. Fatigue, incentive, and emotional strain are fully discussed. This section of the book is rich in illustrative material and descriptions of concrete devices by which adjustment may be improved when it is being hampered by deficiencies of personality or environment. Special attention is given to vocational and recreational problems. The authors apparently consider these the chief problems in the field of social adjustment. One who deals with human beings as individuals rather than as bits of masses of society would have preferred to see the excellent treatment given to the vocational and recreational field supplemented by as careful a treatment of family life. There is the arena for the impact of personalities which sets the pattern with which each individual approaches the larger social world outside his home.

The most valuable phase of the book is its recurrent emphasis on the problems of elderly people and the need for greater study of their social needs and adjustment. The aged are a gift to society of the medical arts and sciences. The prolongation of life is changing the proportion of elderly people within the total population from a sprinkling of individuals to a social group. This group comes into the human race as a stranger and newcomer. Its name is on the relief rolls and it is revising the pension policies of industries. It complicates the problems of employment, increases demand for recreational facilities, and changes the complexion of family life. The middle aged have had to adjust to bearing the burden of a constantly prolonged period of childhood. If they are to be asked to carry responsibility for those who enjoy a prolonged old age, they must have help. Careful study of the needs and social assets of elderly people is imperative if a readjustment to this new group within the social order is to be made without an intervening period of chaos. The authors of this book present this field for study together with constructive suggestions for solution of some of the problems involved.

Wichita, Kansas EDWINA A. COWAN

The Troubled Mind. By HARRY ROBERTS and MARGARET N. JACKSON. New York, E. P. Dutton & Co., 1939. Pp. 284.

This book is described as a "general account of the human mind and its disorders and their remedies." There are chapters devoted to the unconscious, complexes, emotions, habits, psychoanalysis, rationalization, sanity, neuroses, psychoses. The writers state: "This book is not intended as a textbook of psychology or of psychiatry. Rather it should be regarded as an attempt to explain, in simple language, some of the outstanding conclusions of contemporary psychologists, with such criticism as the experience of the authors suggests. One of us has been engaged in general medical practice for several decades; the other has been for many years a medical officer at a hospital devoted to the care of mental abnormals. Such constitutes whatever authority we have."

The first 50 pages of the book do not constitute a fair sample of its worth. The historical perspective is limited. A definition of mind is attempted, the controversies of psychologists are pointed out, the structuralists are hung out on a line to dry, introspection is taken to task for its faults, and instincts are listed. The arguments are discursive and many fundamental issues are briefly, dogmatically discussed. The writers have but a speaking acquaintance with contemporary, experimental psychology.

The book improves with the turning of the pages. The chapters entitled "The Ages of Life," "What Is Sanity?," and "The Neuroses," are admirably done and suited to the needs of the laymen. An excellent chapter is the one on Freud and psychoanalysis. Although the authors state that Freud is largely responsible for the revolution in psychology and that "the debt which science must owe to him is no less than it must ever owe to Charles Darwin," yet they humorously question some of his terms, critically evaluate his more important findings, and point out the absurdities in some of the double-edged concepts. As an instance of their treatment in this connection: "To claim one never dreams at all, is not, as one might suppose, the sign of a tranquil subconscious; it merely implies that one's dreams are too scandalous for recollection. Nightmares and anxiety dreams on the other hand are symbols of a repressed sexual wish. So the psychoanalyst wins either way. One cannot help being reminded of the revivalist who finds a sin for you in the very place you didn't expect." They also point out, "that one may well feel sceptically inquisitive as to the true nature of the operative psychotherapeutic agent in a course of treatment that involves hundreds of hours of intimate personal contact with another individual."

The reader gains the clear conviction that the authors' criticisms of psychoanalysis are based on actual case experiences. Their clinical orientation combined with judicious exposition and interpretation on matters psychoanalytic make these sections worthwhile reading, to the psychologist as well as to the layman. It is to be hoped that some day the writers will find time to incorporate their exposition, interpretations, and criticisms of Freudian psychoanalysis into a separate article for more general distribution.

Rochester, New York ANTHONY J. MITRANO

La Psychologie Expérimentale en Italie. École de Milan. By A. MANOIL. Paris, Félix Alcan, 1938. Pp. viii, 489, 16 pl.

This book is an extensive summary of the work of Gemelli and his associates, research which came in large part from the laboratory of the Catholic University at Milan. Before the foundation of the Catholic University, Gemelli's studies had been concerned with neural and other bodily correlates of emotion, and with certain psychophysical problems of cutaneous space perception. From the University, whose laboratory of psychology was founded by Gemelli, came studies upon varied topics: perception (form, contour, movement, illusions of shape and size, temporal stages in perceiving); electro-acoustical analysis of language; applied psychology (selection of aviators, industrial problems, physical exercise); and comparative psychology. Altogether, Manoil has summarized a considerable body of research.

Manoil's task has been adequately carried out, and the result may be useful to those psychologists who lack facility in Italian. Descriptions of techniques and results are full and detailed, and the documentation includes references not only to the Milan group but also to many other writers in each field. A complete classified list of the references is included at the close of the book.

However adequate Manoil's summaries may be, the reviewer is struck by the question why Manoil should have undertaken such a task in the first place. The usual reason for summarizing the researches of a single psychologist or group of collaborators is either that these researches constitute integral parts of a unitary program, or that they all bear the mark of some special point of view, 'school,' or theory. A glance over the list of topics mentioned above shows that the Milan group investigated a number of widely divergent and unrelated problems. On the other hand, the common thread of point of view running through these investigations is scarcely far-reaching enough to unify the presentation.

There seem to be two main aspects of Gemelli's general point of view, and these are probably attributable, as Manoil suggests, to Gemelli's neo-scholastic philosophy. The first is an emphasis upon psychological activity as organized and directed toward a 'goal.' This is scarcely a unique position. The other point is the notion that there are 'psychic' or 'intellectual' factors determining activity, these factors being independent of physiological events. When, however, we ask what this means in terms of a concrete experimental situation, we find that it implies no more than that an activity like perception is determined by past experience, memory, 'set,' and the like, as well as by the stimulus-conditions of the moment. The assumption that such factors are 'purely psychic' is also scarcely new, and it is not an assumption which makes much difference in the interpretation of concrete experimental results at the present stage of our physiological knowledge.

This discussion is not meant to imply that Gemelli's individual researches and experimental contributions are of little value. In fact, there is a great deal of sound and careful work summarized here. But we do not need to immerse ourselves in almost five hundred pages of description in order to become familiar with Gemelli's point of view. Also, the researcher in any of the fields covered would probably find it necessary to go back to the original papers, no matter how careful Manoil's summaries are. Therefore the value of this book to psychology is scarcely commensurate with the labor which Manoil must have expended upon it.

Cornell University T. A. RYAN

Educational Psychology. By CHARLES H. JUDD. Boston, Houghton Mifflin Co., 1939. Pp. xx, 566.

To Judd, educational psychology is a study of the impact of civilizing institutions upon the maturing individual. The result of this impact is an interaction; for the individual, being adaptive by nature, is forced to conform in large measure but also being resistive insists on changing these institutions to some extent. At the heart of the educative process are language and number. The institution of language, growing as it does out of the rich experiences of the race, influences the child from the very beginning. His reactions to others, his means of influencing others are interwoven with his language development. While his personality is growing to intellectual maturity as evidenced by his ability to narrow his attention, to abstract common elements out of phenomena widely differing in external appearances, and finally to arrive at a generalization, language helps all the way. For these reasons, the development of vocabulary, reading readiness, methods of learning to read, and procedures which stimulate reading for understanding in the upper grades receive much attention.

In like manner, the learning of number is developed. The development of the institution of number is traced from primitive counting through Roman and Arabic numerals to the higher reaches of the mathematics. This is followed by a study of the psychology of the individual's learning to count, to add, and to master the number system. Along with the procedures used in the acquisition of these number concepts appear the processes of abstraction and generalization which are nicely illustrated in those aspects of mathematics commonly taught in the upper levels of the elementary school and throughout the high school. Handwriting, the social sciences, the natural sciences, and the fine arts are treated in the same general manner with minor changes when necessary.

The greatest difficulties in learning arise out of the immature individual's attempts to learn the more generalized concepts of these institutions without having had the concrete experience necessary to understand them. Money, for example, is a generalized concept. It implies that value has been reduced to a common denominator. Only slowly did the race after various experiments with metals of various kinds, with the barter system, etc., arrive at the concept of money. The pedagogical danger is that the growing child on meeting this apparently simple but really very complex idea will learn to manipulate it properly but will not understand its nature.

The last two divisions of the book on personality and the solution of educational problems seem commonplace and uninspired after the climax reached in the treatment of language and number and through them the psychological processes of abstraction and generalization.

The book appears to the reviewer as partial and one-sided. So close has the author been to one school of thought largely inspired by himself that other work equally valuable is treated in a cavalier manner. For example, the word "motivation" does not appear in the index. The broad field of interest is dismissed in a few pages and the problems of growth and maturation are inadequately considered. It is clear that to this author education is for the most part an intellectual process by means of which the maturing individual learns to understand the civilization which encompasses him.

This text is undoubtedly a significant contribution to the field of educational psychology. Its style is learned and intellectual; its arrangement logical and compelling; and its illustrations full of point.

University of North Carolina A. M. JORDAN

Lehrbuch der Psychologie. By TH. ELSENHANS. Third completely revised edition by F. Giese with the collaboration of H. W. Gruhle and F. Dorsch. Tubingen, J. C. B. Mohr (Paul Siebeck), 1937-1939. Pp. xii, 588.

This latest edition of the Elsenhans systematic treatise, issued serially in five paper covered brochures over a period of several years, was indeed a labor of love. First undertaken by Giese until he was stricken with an ocular disability, then finished by the two other collaborators named above, the work provides in one volume an unusual synthesis of psychology as a scientific whole from a somewhat unbiased and non-partisan point of view and in the tradition of Wundt. The backdrop for the entire presentation is the field of natural science. The revision brings together in an uncommonly successful manner widely divergent points of view. The introspective methodology is accepted with due consideration for its difficulties side by side with the behavioristic techniques and the moot question of mind-body relationship is laid aside in terms of the biology of the personality, or, in other words, with regard for the *life* of the individual.

The chapter headings convey the extensive nature of the treatise: psychology as science, perception, thought, feelings, will, bio-functions, development of the mental life, personal integration and mental community. The book is illustrated for the most part with new diagrams and figures. It contains extensive bibliographies at the close of each section and the usual subject-index at the end of the entire volume.

In a treatment of psychological data and theory which appears comprehensive in point of view, we regret that with very few exceptions the references are limited to German sources. It is as if contributions to psychological science came from only one country. Certainly in equally broad treatments of the subject in America citations commonly transgress the national boundaries because science aside from unfortunate linguistic handicaps does not recognize geographic or political limitations. Some terminological transformations are doubtless necessary as in the case of 'decibel' becoming 'phon,' but certainly Troland's contributions to perceptual functions might call for a cross-reference somewhere, as might the work of Thorndike, Carr, Lashley and McGeoch on learning. Such expectations are all the more warranted when as many as four psychologists have had a part in the completion of the treatise.

In the recognized Leipzig tradition the book exceeds the bounds of many similar systematic works of more recent date in consideration of subjects that are not strictly scientific. The book ends with a section on the psychology of the German people which analyzes the peculiar mental traits and psychic products of these nationals as contributions to the present sociological level of civilization. Even the characteristic topographical features of the general landscape, contrasted, for example, with that of the Scandinavian peoples, are discussed as a potent influence on the socio-economic and the mental life of the Germanic population. Similarly geographic conditions, such as being surrounded by neighboring lands as distinguished from an insular position, provide much thought and discussion in this direction.

Scholarship, clarity of diction and attractive presentation of factual materials run hand in hand throughout the book. A 'still' hunt for the obscure and the obtruse would fail to bag much intellectual game of that species. At the same time theoretical attitudes on controversial subjects seem for the most part well balanced with no apparent desire to proselyte the reader unduly. It is truly a valuable reference work which brings the German systematic facts and fields of psychology into perspective from the present day angle and which gives us in addition an accumulation of interesting experimental facts not usually so accessible to the American scholar in psychology.

Chicago, Illinois CHRISTIAN A. RUCKMICK

Facts and Theories of Psychoanalysis. By IVES HENDRICK. 2nd ed. New York, Alfred A. Knopf, 1939. Pp. xiv, 369.

Dr. Hendrick's book is in major part a précis of Freud's teachings, and in itself is hardly subject to criticism save as a fair and reasonably full account of these.

To the best of the reviewer's belief it is so. In style clear and temperate, it can be well recommended to psychological readers sharing a skepticism long current, at least outside clinical areas. This applies most to Parts I, III, and IV, rather less to Part II, which is much absorbed in technicalities and abstractions, with chief interest to the special student. Disagreements there will be at best; a possibly underestimated source may be noted here.

Many years ago the reviewer observed in effect to the author of this book, that one got a better impression of psychoanalysis from the analysts' conversation than one did from their literature; and despite such accounts in English as Hendrick's and Martin Peck's, this has not ceased to be the case. It seems to originate in a very easy but not too obvious confusion of Freudian psychology and its rhetoric, for which the former comes out much the worse. It was and is bad rhetoric to speak of psychoanalysis as a science, or to use the terms sex and wish in the highly abstracted meanings given them. This does not make the observations bad psychology, but critics are very pardonable who ask themselves how far those who are so indifferent with the instruments of understanding, are likely to have good understanding of what they observe. It is for the analysts themselves to resolve these matters, and the worth of their message will not be duly recognized until this is done.

Among the least happy of these usages is that involving the transitive sense of "psychoanalyze." The implication is, that the analyst performs some esoteric process, by which the person analyzed is benefited. The teaching, clearly embodied in Hendrick's book, is that the patient or student analyzes himself, with the help of the analyst, who in the Freudian sense, performs rather the functions of a catalyst. This explains Hendrick's observation, that those in a position to get the most from the analytic experience are often the honest skeptics. The conversational remark of a similarly brilliant colleague of Hendrick's, "distrust anyone who is *enthusiastic* about psychoanalysis," was meant for protagonists but applies not less to the clientele. It is enthusiasm for the analyst's doing the real work. *Sensu stricto* one cannot 'be psychoanalyzed' any more than one can 'be' confessed. We need here the middle voice of the Greeks; one makes his confession, and makes his psychoanalysis; in either case with someone capable of facilitating the process, but having a far from sufficient rôle in accomplishing it. A deeper understanding of this would greatly help the attitudes toward psychoanalysis "of them which are without."

Any book of this kind needs a glossary, but the present one is not among its best-considered achievements, at least for non-analytic terms (*e.g.* pathological = abnormal). In this sophisticated age it verges on impertinence to explain for the putative reader such terms as therapy, gastric, gratification, heredity, not to say anus, coitus, defloration, menopause. On the other hand, knowledge of the vernacular would suggest finding some more univalent term for lay analysis, even though the local profession be "against it."

Harvard University F. L. WELLS

Biology and Christian Belief. W. OSBORNE GREENWOOD. With a foreword by Theodore Savory, Malvern College, New York, Macmillan Co., 1939. Pp. 192.

It is doubtful whether this book would merit the critical attention of a scientific journal were it not for the fact that it contains a pattern of ideas which is characteristic of an increasing number of cults, movements, pronouncements, and books. In these the findings of modern natural and physical sciences are made to appear meaningless without the postulation of a Master Mind. Although the level of intellectual sophistication varies from the crudest forms of evangelical religion to the comparatively erudite formulations of such philosopher-scientists as Eddington and Jeans, the theme is the same: In the study of Nature and Man an *impasse* has been reached; the only hope in the blooming, buzzing confusion is a return to a faith in transcendental, mystical principles. And, so the argument frequently runs, since the scientist has been forced to resort to the supernatural, so must the man-on-the-street seek supernatural direction in the solution of his economic and political problems. At which point, of course, a whole political and social doctrine is implied.

Whether these manifestations are the final convulsive spasms which precede the death of formal religion as we have known it for the last 100 years, or whether they are among the premonitory signs which mark the attempt to abandon, or at least discredit, rationality as a basis for human action, it is perhaps too early to say.

The present volume covers everything from the nature of electrons (which turn out to be essentially spiritual in principle), to the nature of human personality (which turns out to be the one characteristic of man that "has all the qualifications for establishing and maintaining contact with a personal God"). There are chapters on Purpose, Glands, the Nervous System at Work, Survival of Death, and, of course, Psychical Research. The author apologizes in the Preface for the omission of a discussion of Anthropology and Miracles.

Sir Oliver Lodge, Jeans, General Smuts, Wm. McDougall, Eddington, Chas. S. Myers, and Charles Richet are among those who furnish comforting validations, while Sherrington, Darwin, Bertrand Russell, and Morton Prince seem to have made statements which, at least, do not contradict the major thesis of the book.

This thesis may be summarized in two propositions. (1) The evidence of modern science clearly points to the existence of a "directive Mind," an "architect who has thought out the whole scheme before ever a line, so to speak, was put on paper." (2) Physical, biological, and social phenomena may only be understood as inseparably linked with Guidance and Control exerted by a personal God. The use of the terms "guidance" and "control" bears a striking resemblance to that of certain contemporary religious movements—notably the so-called Oxford Group.

From the point of view of contemporary physiological psychology, the chapters on the nervous system are almost incredibly naïve. A strawman is created in the form of a "mechanistic biologist" who has the strange belief that the brain "secretes mind." On the contrary, says our author, the "seat" of the mind is in the frontal lobes which contain merely the machinery through which mind manifests itself. The "mental faculties" are not localized in brain tissue, for the reason that "mind cannot be caged within the limits of a mechanical contrivance," but it is able to manifest itself only in this particular part of the brain. All this can be understood by accepting "the great Creative God Mind—the one element in the whole cosmos which confers reasonableness on a world which otherwise too often seems irrational."

It is the judgment of the present reviewer that this book is not only an example of distorted science and bad logic, but insofar as it is representative of those attempts to discredit man's competence to understand his universe except by the introduction of the supernatural, it conveys a threat to any social order which is erected on a rational basis—a threat, which at the present stage of the world's history, it may not be safe for the responsible scientist to ignore.

University of California (Los Angeles) FRANKLIN FEARING

Our Sex Life. By FRITZ KAHN. Translated from the German by George Rosen. New York, Alfred A. Knopf, 1939. Pp. xxxviii, 459, 41 figures.

The author of *Our Sex Life* is a physician, a practicing specialist for more than twenty-five years. The book is intended to supply practical information on sex. An extensive range of topics is presented including chapters on hygiene, fertility, contraception, and impotence. The style is felicitous and easily comprehensible. The many illustrations are well done. Although Kahn does not hesitate to stress pathological consequences, morbidity is avoided. Occasionally the author indulges too freely in poetical and romantic phraseology, but, for the most part, the attitude is objective and matter-of-fact. Sex is treated as a specifically human function without reference to its rôle in evolution or its place in biology.

The emphasis is psychological and pedagogical as well as medical. The influence of psychoanalytic contributions is evident but the categorical explanations of psychoanalysis are not adopted. The significance of psychological factors is stressed throughout the book, particularly in the chapter on impotence.

The most noticeable shortcoming is the extreme tendency to exaggeration. For example, Kahn states that "thirty per cent of all mentally inferior children who are provided for in institutions are syphilitic"—a statement in agreement, perhaps, with popular thought but contrary to statistical fact. The exaggerations are abundant in other connections and the reader is constantly compelled to question critically the evidence.

Nevertheless, the book serves its purpose well. It is distinctly better than the usual book on the subject. The cost of the book is, however, too great to permit distribution to the general reader for whom it was intended. This is unfortunate, in a way, since the book's usefulness will have been outlived within a few years when other writers will want to incorporate some of the important psychological findings of recent investigations such as may be found in Terman's *Psychological Factors in Marital Happiness.*

Rochester, New York ANTHONY J. MITRANO

Man Against Himself. By KARL A. MENNINGER. New York, Harcourt, Brace & Co., 1938. Pp. xii, 485.

Menninger presents his theme, that "each man kills himself in his own selected way" (vii), in six parts, under the titles: destruction; suicide; chronic suicide; focal suicide; organic suicide; and reconstruction.

The first part deals with a great paradox. Man has his hands full fighting Nature, but not satisfied with this struggle alone, he fights his fellow men, and not infrequently turns against himself. Thus life is a continuous battle between the life instinct and the death instinct.

In the United States alone, about 22,000 people take their own lives every year, while in some European countries suicide is more than twice as frequent. Yet, very little is done about the problem outside of the medical profession. Menninger shows how difficult it is for physicians to overcome public lethargy and save the lives of would-be suicides.

The question that naturally arises is why there should be so many suicides when the urge for self-preservation is supposed to be one of the fundamental laws of life. According to popular opinion, "suicide is an escape from an intolerable life situation" (p. 17). According to Menninger, however, much of the motivation of suicide is not external, but is found in the unconscious. It consists of the wish to kill, the wish to be killed, and the wish to die.

Besides the suicide that causes death all at once, there are others that bring about death by degrees. Menninger calls such suicides chronic. It is a "suicide by inches as it were" (p. 87). Asceticism, martyrdom, neurotic invalidism, alcoholic addiction, anti-social behavior, and psychosis are examples of chronic suicides.

The third type of suicide is focal suicide. This type limits itself to specific parts of the body, and by bringing about the destruction of these often causes the destruction of the entire organism. The phenomena that belong here are self-mutilation, malingering, compulsive polysurgery, certain unconscious purposive accidents, and sexual impotence.

Finally, the fourth type of self-destruction is organic suicide. As repeatedly indicated by the psychoanalysts, man is in constant need of love. If this love is not forthcoming, the individual concerned may come to hate the person from whom he expected love. In such instances the individual who seeks to express his hate, but cannot, may turn this hate upon himself, and as a result invite organic suicide. The hate thus achieves expression in the guise of bodily diseases which may eventually cause death.

Menninger suggests that man should try to sublimate tendencies which are not compatible with the conditions by which he is bound. Man is free to avail himself of the clinical and social techniques in this matter, and need only half try to fight the death instinct in himself to be successful. "Aggression," says Menninger, "should be deflected to harmless targets" (p. 442).

Menninger has dipped freely into his rich experience as a psychiatrist to illustrate the many points in his exposition, and despite occasional overstatement, the book presents a well reasoned case. It focalizes man's tendency towards self-destruction and shows the way to combat it.

University of Manitoba PETER HAMPTON

Public Opinion. By WILLIAM ALBIG. New York, McGraw-Hill Book Co., 1939. Pp. xiii, 486.

The era of the War of Nerves assuredly calls for a calm, dispassionate, and scholarly review of the broad field of public opinion in the light of controlled investigation. Even a reviewer who was chary with his superlatives would be forced to grant that Albig has accomplished this in a volume which is not likely to pass from sight within the next few years.

A study of communication supplies the common thread of the exposition. After defining 'public opinion' and 'communication' the author proceeds with a discussion of the various influences which serve to mold public reaction. This carries him through such varied fields as the study of myths, the analysis of leadership, the measurement of opinions and attitudes, and the investigation of changes of opinion. He deals with propaganda and with the interest groups which employ special pleading. He introduces the reader to basic data concerning the status and the known effects of the radio, the moving picture, and the verbal and graphic contributions of the press. He does all this with a minimum of special pleading on his own part, introducing instead empirical data and thoughtful analyses. Even when he deals with the topic of the effect of violence upon public opinion, he resists the temptation to become violent himself and continues to write as a democratic liberal.

It is possible to cover within 400 pages as wide a field as he does only because he writes in a highly compressed style which verges occasionally on the staccato but which is always fluent and readable. Albig has read widely and critically; his ability to digest what he has read and to relate it to the news of the day is most impressive. As must be the case, there are spots in which the exposition falls short of the level of excellence maintained through most of the volume. The sections on propaganda, for example, lack the breadth and thoroughness of many of the other chapters. Such shortcomings are easily forgiven, however, when one realizes that the author never plays the pedant, never moralizes, and never uses his illustrations as anything more than illustrations. Nor do they detract seriously from the importance of the contribution that Albig, himself a sociologist, has made to social psychology.

University of Maryland JOHN G. JENKINS

Der Arzt in der Dichtung der Gegenwart. By BRUNO WACHSMUTH. Enke, Stuttgart, 1939. Pp. 241.

A large part, but not all of the book, consists of epitomes of novels in which a physician appears as the principal personality and of novels written by physicians. It is therefore in a way a contribution to the history of medicine. The author, however, is a historian of general literature by profession. Around 1890 medicine was a kind of lighthouse for all intellectual activity at least in Germany, if not in the world. The romanticism of the beginning of the century was now looked upon as a kind of medieval darkness. For example, Strindberg wanted to be a physician before he found his vocation in literature, and Ibsen prefers a physician to any other professional man as a spokesman for his ideas. At the end of the nineteenth century the physician was the ideal man. The author points to these facts by numerous quotations from and analyses of literary works. It is impossible in a review to give a correct idea of the wealth of the material thus presented.

With the beginning of the twentieth century realistic criticism applied to the practice of medicine comes to the front. The specialist is pictured in literature as a suspicious, questionable character. "Naturopathy" is praised. On the other hand the physician is also often pictured as the *Führer* of the population. Conflict of the physician with governmental authority occurred occasionally. The author mentions the case of the President of the 1904 meeting of the German Society of Physicians and Naturalists, who was severely censured by the Prussian government for having permitted the reading of a paper which denied a personal god and an immortal soul and asserted that Science would lead to universal tolerance and international peace. Later the censured president said in his autobiography that the troublesome paper had unnecessarily spoken of what was everybody's common knowledge. The world war produced a great change, which the author expresses in the words that "it was Science which lost the war." In 1929 one could hear official medicine give utterance to the notion that "he who does not believe in the psyche can not be a physician."

The final chapters of the book contain numerous abstracts from literature in which the often tragic conflicts of the physician's professional ideals with his sentiments as the father of his own family are described, especially in central Europe during the decade following the world war.

University of Miami MAX F. MEYER

Mensch und Volk der Grossstadt. By WILLY HELLPACH. Enke, Stuttgart, 1939. Pp. 139.

The social problems created as well as solved by the increasing tendency of populations to live in large cities are here very ably discussed. Older evaluations of the civilizing character of a metropolis have often been negative. But the objective studies of more recent social science have caused such negative estimations to become more and more rare. The author, a prominent psychiatrist and anthropologist, is unusually well qualified to present both the advantages and the disadvantages for the individual as well as for society of the crowding together of hundreds of thousands of human beings in one spot. Right in the beginning he quotes a famous parliamentary speech of the feudal landlord Bismarck, who could not understand why one of his "very decent" hired men, after military service in Berlin, refused to return just because, as he said, in Berlin he could on a Sunday drink a glass while sitting in the air of a beergarden and listening to music with a thousand other citizens—as if the country did not offer him "the open air" every hour every day all the year around. But the city gives, combined with an unenforced sociality, a freedom which is denied to him who lives in the country. Hellpach discusses the city man from every point of view in physical anthropology and finds that he is far from being mentally or physically inferior to his country brother.

The second subdivision of the book shows in detail how the city in a psychophysical sense makes a different being out of him who left the country. Climate, different nature of sunlight, the very ground on which the city stands, food, clothing, bedding, a variety of poisons, rapidity of maturing are factors which make the city population different from that of the country.

The third part gives the social psychology and characterology of the city man. In many different ways the city makes the man free. Marriage is delayed. The number of children per family is reduced. If these and other facts are regrettable, the author insists that they can not be modified by moral indignation and condemnation (just as syphilis could not be abolished by such means) but only by a frank scientific study of the causes and measures for amelioration based upon such studies. Any national language is largely the result of the influence of one or a few large cities. Without large cities that unifying element would be absent and the dialects alone existing could hardly be mutually comprehensible.

That there is much in a large city which is unnatural, abnormal, the author not only admits but emphasizes; and for that very reason he emphasizes the need of a thorough scientific study not only of a few, but of all the phases of city life.

University of Miami MAX F. MEYER

Aesthetic Motive. By ELISABETH SCHNEIDER. New York, Macmillan Co., 1939. Pp. 136.

Miss Schneider takes "the unity-in-variety theory, being as old as any in the world" (p. 129), and finds that such unity gives man pleasure because "one of his most disturbing problems . . . is his desire for unity when faced with the multiplicity and otherness of the world" (p. 38). Thus she hopes to find the key to esthetic experience in an understanding of human motivation and need. That the esthetic problem, which has so long resisted solution, might be profitably attacked by way of a study of the esthetic motive is a view that will appeal to the modern psychologist, but Miss Schneider produces little evidence to support her particular thesis. She rejects both the experimental method of the psychologist and the empirical method of the philosopher, and holds to what I take to be the hit-or-miss method of the artist—that is to say, she paints the truth as she sees it about esthetics, frames it in covers, hangs it in your library, and asks whether it does not please you by introducing unity into the variety of your thoughts about esthetics.

It is natural that the esthetician should use the artist's method of appeal to conviction, and also that the scientist should resent that procedure, asking for a weighing of evidence. Miss Schneider does not say how that conflict of attitudes toward knowledge could ever be resolved, and I, believing in evidence and mistrusting conviction, find her many dogmatisms disturbing. For instance: "The esthetic impulse appears to be exclusively the possession of man" (p. 12; cf. also pp. 38, 129 f.). If my conscience could free me for equally dogmatic expression of conviction, I could oppose to Miss Schneider's anthropocentric view: "Man has never been shown to possess a general psychic capacity that is not shared by lower animal forms, and it is doubtful if most human mental capacities are well understood until their animal forms have been described." I could even marshal evidence for that view—evidence from the volition, thought, reasoning and language of animals. In fact, the world needs an animal esthetics, but it will not be Miss Schneider who gives it to us.

Harvard University EDWIN G. BORING

Allied Propaganda and the Collapse of the German Empire in 1918. By G. G. BRUNTZ. Stanford University, Stanford University Press, 1938. Pp. xiii, 246.

"When, therefore, all allowances have been made and all extravagant estimates pared to the bone, 'the fact remains, propaganda is one of the most powerful instrumentalities in the modern world.' " Thus Bruntz quotes Lasswell in concluding his survey of the effects of allied propaganda upon the Germany of 1914-1918. In a meticulously documented and scrupulously unprejudiced review of many hundreds of specific examples of propaganda, the author leaves small doubt as to the truthfulness of his quotation.

After a review of organizations and general tactics involved in the barrage of war-time propaganda, Bruntz proceeds with his analysis of the types of materials employed. He discusses propaganda of enlightenment, of despair, of hope, and of a particularist (*i.e.* sectionally-aimed) sort. Revolutionary propaganda is sufficiently important to warrant a separate chapter. As a primary contributory condition the internal situation in Germany is subjected to careful scrutiny. A chapter on measuring the effect of propaganda proposes and discusses a half-dozen 'yardsticks' by means of which the influence of materials and methods may be assayed. The volume closes with an extensive bibliography.

The volume is characterized by the presentation on almost every page of original documents and citations from primary sources. Although the translations of some of these remind one of the efforts of a non-linguistic sophomore, and although the punctuation of them is distinctly impressionistic, such minor faults cannot detract from the generally careful workmanship and praiseworthy coherence of the volume as a whole. Bruntz's work is definitely on the *must* list for social psychologists and there are few psychologists of any sort who will fail to obtain a clearer insight into the present European situation by reading it.

University of Maryland JOHN G. JENKINS

Educational Psychology. By JAMES L. MURSELL. New York, W. W. Norton & Co., 1939. Pp. x, 324.

Mursell's thesis is "that mental life must be understood as a dynamic process of interaction between the individual and his environment." He strongly emphasizes the concept of mental growth.

In the early part of the book, he places educational psychology in its relation to general psychology and to education. Following this the usual topics are considered: heredity vs. environment; individual differences and adjustment; mental growth and development; the nature of learning; types of learning; transfer of training; the social setting of mental development; and the dynamics of learning and growth. In his treatment of the last topic the author summarizes his views and attempts to develop a philosophy of life in his readers.

The book is easy to read. It is interestingly written and is richly supplied with illustrative examples. A selected list of references for further reading is appended to every chapter. Few technical terms are used and then in a way that makes them understandable to the beginning student of education.

The author does not stress any of the so-called theories of learning enough to bias a reader. He does not take sides in many matters and has left out much material that is considered in most texts, because he does not think it is of enough importance to undergraduate students or beginning teachers, for whom the book is written.

Baltimore, Md. DONALD R. SMITH

The Schizophreniform States: A Katamnestic Study Based on Individual Reexamination. By GRABIEL LANGFELDT. London, Humphrey Milford, Oxford University Press, 1939. Pp. 134.

Through a personal reëxamination of 100 discharged patients who were admitted to the Oslo Psychiatric Clinic as doubtful schizophrenics, Dr. Langfeldt attempted to show that a finer diagnostic sifting of this material is necessary for its successful treatment. He sought, in the present monograph, to establish (1) the pre-psychotic factors which characterize this group, (2) the ways in which the psychoses developed, (3) the duration of the psychoses before admission to the institution, (4) the relation between the duration before admission and the future course of the disorder, (5) the atyptical conditions, and (6) their influence on future development. These facts are presented in numerous tables and are illustrated by aptly chosen case histories.

Dr. Langfeldt's study discloses that the typical schizophrenias and the schizophreniform states (atypical) are fundamentally different in their prognosis. About two-thirds of the latter are, he finds, cured spontaneously within the first year after discharge from the clinic. They, moreover, are permanently cured.

Dr. Langfeldt believes that shock therapy must be judged according to whether the cases are typical or atypical. He holds that the bulk of his cured cases present a symptomatology of a schizophreniform character similar in phenomenology to the type of case which reacts favorably to cardiazol and insulin treatment.

For the students trained in America, such statements as "he appeared to be somewhat imbecile . . . IQ 75" (p. 43) will seem strange. A stilted translation provokes, moreover, much re-reading. Investigators using the method of shock-treatment will, however, find much of interest in this monograph.

Lewisburg, Pa. ROBERT M. LINDNER

Principles of Psychology. By FRANCIS L. HARMON. Milwaukee, Bruce Publishing Co., 1938. Pp. xiv, 609.

Professor Harmon's book is intended as a text in general psychology for students in Catholic colleges, and is one of the Science and Culture Series edited by Joseph Husslein, S.J.

The author's religious affiliations and convictions presumably account for his definition of psychology as "that division of philosophy which deals with the human soul" (p. 3), and for his unremitting efforts to persuade his readers that human beings have a soul, that the soul reacts upon the body, that the soul is immortal, that animals lack "rational powers," and that the will is free. Adversaries of these Scholastic doctrines are vigorously and repeatedly attacked throughout the book, though Dr. Husslein may err when he states in the preface that a "refutation" of their views has been achieved (p. ix). The materialistic hypothesis is asserted to be responsible for the "low esteem in which contemporary psychology is held by many leaders in other fields of science and philosophy" (p. 72), and for the fact that the "man in the street" is often much more successful than the psychologist is in understanding and controlling others (p. 89).

As Professor Harmon frequently admits, most psychologists do not accept his philosophical premises. Nevertheless, from the viewpoint of the somewhat limited audience which he is addressing, the text deserves commendation. There are numerous references to contemporary experimental literature. There is an abundance of original and therefore unstereotyped diagrams and drawings. In particular, the clarity and vigor of the style testify to the possession of dialectal and literary skills to a degree none too common among psychologists. Perhaps, however, these stylistic merits derive in part from the simplicity of the author's dualistic system, and from the ardor with which he expounds and defends it.

New York University LELAND W. CRAFTS

Dreams and Dreaming. By H. TASMAN LOVELL. Sydney, Dymock's Book Arcade, 1938. Pp. viii, 128.

This small volume is a new edition of Monograph No. 2 of the Australasian Association of Psychology and Philosophy. The first part is an exposition of Freud's theory of dreams and dream mechanisms. Most of the illustrative material is in the form of quotations from psychoanalytical literature, particularly the better known writings of Freud and his disciples. Although the author might be classified as a Freudian, he does criticize certain conceptions of the "master." He attempts to reconcile traditional psychology and psychoanalysis, principally in terms of the psychology of McDougall. He agrees with Jung's and Maeder's stress upon the significance of the manifest content of the dream. He agrees with Rivers in believing that other emotional states than wishes are basic in dreams and River's criticisms of the importance of sex in the Freudian interpretation.

Unfortunately the author's attempted reconciliation of traditional psychology and Freudian psychoanalysis does not consider those few experimental studies which have been conducted. For instance, in discussing the influence of sensory stimulation upon dream content, he cites anecdotes and does not even mention the experimental work done by Horton and Cubberly.

This volume will serve as an introduction to a very slightly modified Freudian theory of dreams. For the professional psychologist there will be little of value in the work except the author's attempt to reinterpret dream mechanisms in less personalistic terms than Freud does. The appendix on dream prophecy probably will not add to the admiration which psychologists will hold for the volume.

University of Newark FREDERICK J. GAUDET

Collection and Presentation of Statistical Data in Psychology and Education. By MARTIN F. FRITZ. New York, Prentice-Hall, 1939. Pp. vi, 58.

This little manual is intended to supplement textbooks on statistical method and on tests and measurements by describing in a very simple way some of the procedures to be followed in the collection, tabulation, and graphic treatment of data. The discussions of the common sense of method and the interpretation of graphs are well done. The usefulness of the material to the beginning student will depend largely upon his lack of familiarity with statistics and, perhaps, upon his inability to comprehend the mathematical concepts presented in the usual textbooks.

The manual has been carefully prepared and the author renders service by frequently pointing out sources of error in interpretation, as, for instance, in the use of area and solid diagrams and in the uncritical use of percentages. While this material is scarcely new to graduate students in psychology and education, the manual does provide additional illustrations which will doubtlessly facilitate understanding.

As a final word of advice, the author suggests that students who are unable to solve their problems write to a statistician in some university. He adds, pertinently, that these people are usually busy. It is to be hoped that students will remember this admonition when following the author's advice.

Columbia University HENRY E. GARRETT

Superstition and Society. By ROGER MONEY-KYRLE. London, Hogarth Press, 1939. Pp. ix, 163.

The purpose of this book is to propose and initiate a liaison between psychoanalysis and anthropology in the study of society and social issues. Comparative studies of racially similar tribes show that children who are sexually suppressed and disciplined are more fearful and anxious than those from tribes in which the libido is unbridled. As adults they are less coöperative and democratic, but more cruel, intolerant, and ambitious than petted children.

The origin of the childish anxiety motive is explained in typical Freudian fashion. The babe's fear of his father compels suppression of his incestuous urge until this inhibition is fixated in the personality as a Super Ego. Much anthropological significance is attached to the indirect resolution of this childish conflict in myth and ritual, and in the neuroses. Also in religion and the modern obsessional drive to work and succeed. The irrationality of modern economic and international conflict is interpreted as a struggle against various symbols of the tyrant father.

The interpretation is as valid as the orthodox Freudianism which it espouses. In the writer's opinion the great gift of psychoanalysis to psychology is its personalistic dynamics. This gift is enhanced by the fact that the agents of action wear masks.

University of Colorado THOMAS H. HOWELLS

Representative Psychologists. By ANTHONY C. WESTERHOF. Union Bridge, Maryland, Pilot Publishing Co., 1938. Pp. vi, 119.

The psychologists selected for consideration are Stern, Koffka, Köhler, Lewin, Terman, Tolman, Freud, Jung, Adler, Pauly, and McDougall. Pauly, whose name may be unfamiliar, is a German Lamarckian biologist. These men are said to represent "the present leading schools:" the Personalistic, the Gestalt, the Psychoanalytic, and the Hormic. The Purposive Behaviorism of Tolman and Terman's position are not definitely classified.

Dr. Westerhof does not seriously undertake to report experimental or clinical observation. He is interested almost exclusively in philosophical views, stated or implied. More specifically, he is interested in establishing the positions of the representative psychologists in relation to an assumed controversy between mechanists and teleologists. His own views are avowedly teleological and this causes him to favor the interactionist conception of the mind-body relation and the belief that consciousness is a determiner of behavior. He approves Terman's insistence on the reality of purposive striving, Pauly's pan-psychism, and most of McDougall's system of theory. The main issue is not met by the various Gestalt writers, he feels.

Dr. Westerhof's literary style is extremely attractive and his characterization of the various philosophical views is penetrating and, at times, humorous.

University of North Carolina ENGLISH BAGBY

A Manual of Psychology. By G. F. STOUT. Revised, in collaboration with the author, by C. A. MACE, with an appendix by R. H. Thouless. London, University Tutorial Press, Ltd., 1938. Pp. xix, 708.

This is the fifth edition of Stout's well-known *Manual.* It is a reprint of the fourth edition which was revised by C. A. Mace. The changes from earlier editions, according to Mace, are chiefly four: (1) a restatement of Stout's general theory of the relation of thought to sentience; (2) certain amplifications dealing with the relation of psychology to philosophy; (3) indications of agreement between Stout's theory and Gestalt psychology; and (4) the relation of Stout's doctrine of conation to that of McDougall.

The fifth edition differs only in the addition of an appendix by Thouless giving a brief, though adequate, statement of Gestalt theory, followed by a supplementary note written by the author in which he states his dissent from Gestalt psychologists on two points: "(1) their almost complete denial of the part played by past experience in perceptual process; (2) their neglect of the difference between what I call sensible appearance and perceptual appearance."

Cornell University R. M. OGDEN

Hegel über Offenbarung, Kirche und Philosophie. By GUSTAV E. MUELLER. Munich, Ernst Reinhardt, 1939. Pp. 61.

Mueller, professor of philosophy in the University of Oklahoma, attempts here to study the religious contents of Hegel's thinking. Students interested in the psychology of religion will find here an objective presentation of Hegel's personal problems. How objective idealism and religious belief can be expressed in one personality, Mueller clearly explains.

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THE RELATION OF PITCH TO FREQUENCY: A REVISED SCALE

By S. S. STEVENS, Harvard University, and J. VOLKMANN, Columbia University

That *pitch* is not the same as *frequency* has already been amply demonstrated.[1] That pitch is a *function* of frequency is a fact too obvious to need argument. How pitch is related to frequency, however, has long been a vexatious problem disputed by musicians and psychologists alike. The task here undertaken is that of investigating the dependence of pitch upon frequency for the purpose of erecting a scale by which pitch, as a perceived aspect of tones, can be measured.

Pitch is one of the psychological aspects, or attributes, of tone. It is one of the dimensions in terms of which we are able to distinguish and classify auditory sensations. Pitch differs from frequency in that pitch is determined by the direct response of a human listener to a sound-stimulus, whereas frequency is measured with the help of instruments. A *pitch-scale* is what we obtain when we assign numerals to tones in such a way that the numerals stand in some definite relation to the pitch exhibited by the tones. For the assignment of numerals to tones many schemes are possible, and we shall begin with a look at some of these alternatives.

SCALES IN GENERAL

To any aspect of an object or event that exhibits itself in discriminable degrees we can assign numerals. Human beings can tell one numeral from another and most of them know the relative positions of numerals in the numeral-series. Consequently, when different degrees of the same aspect can be discriminated, these manifestations of the aspect can be

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[*] Accepted for publication August 21, 1939.

[1] S. S. Stevens and H. Davis, *Hearing: Its Psychology and Physiology*, 1938, 70.

paired with different numerals, and a kind of scale is at once achieved. *Numerals* and not numbers are what we assign when we make a scale, for number is itself an aspect of things, like weight; and for both number and weight we pair numerals with things in order to make scales.[2] But the assignment of numerals to aspects is a trivial enterprise unless some sort of definite rule is followed. In actual practice we use many different rules, and, because of the differences among the rules, we have different kinds of scales and different kinds of measurement.

We shall here consider three kinds of scales, gotten by three different rules for the assignment of numerals. The numerals, if we may recall some obvious points, are signs which we commonly make on paper, for example: 1, 2, 3, 4, etc. From the point of view of the scale-maker, the important thing about the numerals is not their shape or their history—it is their position in the numeral-series. In fact the usefulness of numerals in measurement and in mathematics derives directly from the fact that each numeral has a definite position in a series and hence bears definite 'spatial' relations to the other numerals. These 'spatial' relations among the numerals figure, in various ways, in the construction of the following types of scales.

(1) Ordinal scale. Given several objects or events exhibiting various degrees of a particular aspect, we can pick out the least and the greatest degree, and rank-order the other cases between these extremes. Then, having thus ordinalized the objects, we can assign them numerals in such a way that the order ('spatial' order) of the numerals reflects the rank-order of the objects. To the rank-ordered objects we could assign the numerals 1 2 3 4 . . . or 2 4 6 8 . . . or 12 17 32 37 . . . or any other set of numerals whose *order* is determined. We could use the letters of the alphabet too if we liked, for an ordinal scale tells us when one thing is greater than, equal to or less than another as regards some particular aspect, but it does not tell us how much greater it is. In other words, the property of order within the numeral series is the only property utilized in an ordinal scale.

(2) Intensive scale. Whenever, in addition to knowing how to order a set of items, we are able to devise a rule telling us when to assign the numeral *adjacent* to the one just previously assigned, we possess the makings of what we might call an intensive scale. Thus, in erecting a psychological scale for a series of grays, we should have an intensive

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[2] For a discussion of the relation of the numeral-series to scales, see S. S. Stevens, On the problem of scales for the measurement of psychological magnitudes, *J. Unified Science* (in press).

scale provided we assigned adjacent numerals to grays separated each from the next by a just perceptible difference. Gray number 17 would be just noticeably lighter than gray number 16, and just noticeably darker than gray number 18.

It might appear that we have here a scale with a *unit*. Fechner thought so and many have agreed with him. It must be pointed out, however, that a rule for assigning adjacent numerals to objects or events does not necessarily create a unit of measurement. For one thing a unit of measurement must remain invariant. The subjective size of the jnd for loudness has been shown to vary tremendously,[3] but, as we shall see later, the jnd for pitch appears to remain subjectively constant at all frequencies. Hence, many scales of the sort Fechner proposed would seem to be intensive scales only, possessing a definite order and a rule for the assignment of adjacent numerals.

The scale of musical notes *do, re, mi, fa,* . . . can also be considered an intensive scale: the notes themselves are ordered, as are numerals, and adjacent notes are assigned to frequencies bearing a certain *ratio* to one another. The musical note, however, is not a *unit* in the strictest sense.

(3) Extensive scale. The extensive scale utilizes more of the properties of the numeral-series than either of the two previous scales. In addition to the factors of order and adjacency, we here encounter the property of 'distance' or extent. The numeral 8 is as far from 4 as it is from 12, and it is half as far along the numeral series as is 16. Incorporating these properties in a scale, we assign numerals in such a way that the relations among them reflect relations among the objects. Thus we assign the numeral 16 to a length that is twice as long as that to which is given the numeral 8, and so on. Similarly, in scaling an attribute of sensation, if we can devise rules so that the numeral 8 is assigned to the case exhibiting half the magnitude of that to which 16 is assigned, we can erect an extensive scale wherein the 'spatial' relations among the numerals correspond to perceived relations among sensations. This type of scale allows us to define and utilize a *unit* and to employ the rules of arithmetic in manipulating the scale-numerals.

Extensive scales are generally the most useful scales for science, and the generation of an extensive scale for the attribute of tonal pitch is the aim of the present experiment.[4]

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[3] S. S. Stevens, A scale for the measurement of a psychological magnitude: Loudness, *Psychol. Rev.*, 43, 1936, 405-416.

[4] What is here called an *extensive* scale is the same as what has sometimes been termed a *numerical* scale. The present terminology seems preferable, however, since almost all scales are numerical scales in some sense of the word.

THE PREVIOUS EXPERIMENT

In the summer of 1936 Stevens, Volkmann, and Newman presented a group of *Os* with the task of adjusting the frequency of one of two alternately presented tones until it sounded half as high in pitch as the other tone.[5] The *O*s found the adjustment easier to make than might be supposed, and from their results it was possible to construct an extensive scale of pitch. The unit chosen for the scale was called a *mel* and the pitch, in mels, of each audible frequency was represented by a numeral in such a way that the 'spatial' relations among the numerals were reflected in the perceived relations among the pitches.

Although this earlier experiment demonstrated the feasibility of erecting a subjective scale for pitch and revealed certain interesting relations between the form of such a scale and functions pertaining to differential sensitivity and cochlear mechanics, several problems remained unsolved. First, there was the question as to whether a scale obtained by the method of fractionation (setting one tone to half the pitch of another) could predict, as it should, the results of an experiment on the bisection of tonal intervals or the judgment of equal sense-distances. In other words, could the same scale be got by different experimental approaches?

Secondly, we need to know more about the reliability of the judgments upon which a pitch-scale must be based. How does the variability among successive judgments by a single *O* (intra-observer variability) compare to the variability among the averages obtained from different *Os* (inter-observer variability)? The answer to such questions requires the services of more *Os* than coöperated in the earlier experiment.

Thirdly, given better experimental apparatus, what changes would appear in the results? The earlier apparatus was quite adequate for all except the very low tones. Below a frequency of about 120 ~, however, the intensity of the tones fell off so rapidly with decreasing frequency, that the behavior of pitch in the region of the lowest tones could not be adequately explored. We suspected that the pitch-scale as previously published needed correction at its lower end.

Finally, what of the complaint made by some *Os* that they were uncertain as to what is "zero pitch"? The two *Os* who imagined a zero equal to "the lowest note on the organ" deviated systematically from the other *Os* who, according to their introspections, thought little about the problem of zero pitch. Perhaps an experimental method which acquainted *O* with zero pitch directly would improve the reliability of the experiment and make a systematic difference in the results.

The following experiments were directed upon these problems.

APPARATUS FOR DETERMINING EQUAL SENSE-DISTANCES

In order to present the *Os* with working conditions as nearly optimal as possible, we designed what is essentially an electric piano, consisting of 20 wooden keys each an inch wide (see Fig. 1). Each key, when pressed, produced a pure tone in a loud speaker. Above each key and immediately in front of *O* was a small knob for controlling the frequency of the tone produced by its respective key. Thus 20 different tones were available at the touch of a key, and the frequency of each of these tones could be independently adjusted throughout a wide range. The total range of the piano was from 0 to 15,000 ~ per sec.

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[5] S. S. Stevens, J. Volkmann, and E. B. Newman, A scale for the measurement of the psychological magnitude pitch, *J. Acoust. Soc. Amer.*, 8, 1937, 185-190.

A few technical details may be noted. The pressing of a key introduced into the circuit on the 'fixed' side of a beat-frequency oscillator (General Radio, type 613-B) an amount of capacitance determined by the setting of a variable condenser connected to the key. The knob above each key controlled this variable condenser, whose size was chosen to fit the range of frequencies that the key was meant to cover. With no keys pressed, the beat-frequency oscillator was tuned to zero by turning its main tuning condenser, and in this condition no tone could be heard.

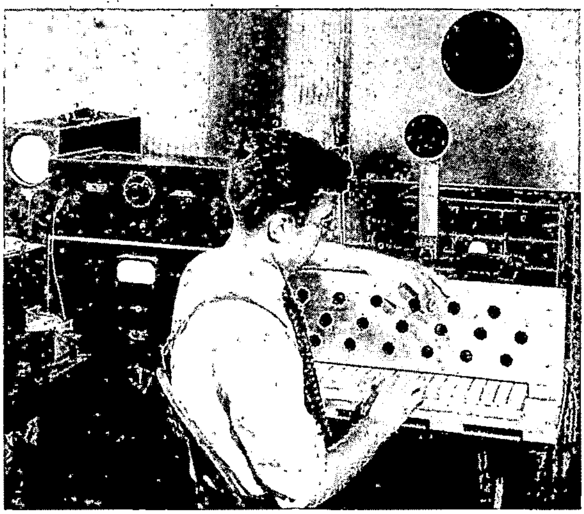

FIG. 1. THE APPARATUS

The output of the oscillator was led to an amplifier containing a so-called equalizing network. The important function of this amplifier was to keep the intensities of all tones adjusted so that tones of all frequencies sounded approximately equal in loudness. The loudness-level used was about 55 db. The equalizing network was essentially a variable filter, by means of which the frequency-characteristic of the amplifier could be altered. In order to compensate for the characteristics of the loud speakers and to maintain constant loudness throughout the frequency-range, we found that the response of the amplifier had to be increased by about 26 db. at 25 ~ and by about 24 db. at 10,000 ~. Under these conditions the *O*s made no complaints about some tones being louder than others. When there are prominent differences in loudness, the *O*s seldom fail to mention the fact, because it distracts attention.

The loud speaker was a 13-in., permanent-magnet type (Cinaudograph). It was mounted in a large baffle designed according to the bass-reflex principle, and its natural period was as low as 30 ~. The loud speaker was suspended at one end of a room lined with rock-wool. *O* sat facing the speaker 7 ft. in front of it. In order to augment the high tones (above 8,000 ~) the large speaker was supplemented by a small 'crystal' speaker suspended 3 ft. in front of *O*.

The frequency of the tone produced by each key of the electric piano could be read from the dial of a wave-analyzer (General Radio, type 736-A) which was connected in parallel with the loud speakers.

THE PITCH SCALE DETERMINED BY EQUAL SENSE-DISTANCES

Preliminary experimentation convinced us that it is impracticable to ask an *O* to adjust the frequencies of all 20 keys so as to create 19 equal intervals of pitch between, let us say, 40 and 12,000 ~. It is also rather wasteful of effort to try to divide such a wide range—40 to 12,000 ~—into only two equal intervals of pitch. After several exploratory tests, we concluded in favor of the following procedure.

Five keys were selected from the piano; the lowest key was tuned to 200 ~ and the highest to 6500 ~, and *O* was required to adjust the frequencies of the three intermediate keys until the pitch-distance between each pair of adjacent keys appeared equal. In other words, the *O*'s problem was to produce four equal intervals of pitch. He was allowed as much time as he desired, but he was instructed not to pass on his final adjustments until he had carefully compared each interval with each other interval, playing the intervals in both ascending and descending order. This requirement was meant to discourage *O* from being too easily satisfied with only successive bisections; he had to perfect his adjustments even after he had bisected the initial long pitch-interval, and had bisected each of the resulting halves.

The same procedure was carried out for two additional ranges of frequency: 40 to 1000 ~ and 3000 to 12,000 ~. Each *O* made, at different times, five settings for each of the three ranges, and 10 *O*s took part in the experiment.

It will be noted that none of these ranges can be divided into four equal intervals in such a way that the intervals are musical octaves or fifths, the two musical intervals of simplest ratio. This choice of range was designed to remove from the musical *O* any temptation to adjust the tones to musical intervals. It appears, however, that this precaution is not entirely necessary—when they are directly compared, octaves in different parts of the musical scale do *not* sound like equal intervals or extents of pitch. Rarely did the *O*s report a desire to adjust the frequencies to musical ratios, or appear frustrated because it was impossible.

Table I records the results of this experiment for the 10 *O*s. For each *O* and for each of the three ranges of frequency, the three frequencies used to divide the total range into four equal intervals are tabulated. Each entry is the average of five settings. Measures of variability are also recorded. These will be discussed later, but let us now proceed to the construction of a scale of pitch.

Assuming that the *O*s have obeyed instructions and that, subject to a certain variability, they have divided the interval between 200 and 6500 ∼ into four equal pitch-distances, we can proceed directly to plot a function between these two limits. This function is the section of the curve in Fig. 2 which is determined by the circles. (The filled circles mark the ends of the range; the hollow circles show the average adjustments by the 10 *O*s.) These circles determine a function—slightly sigmoid in form—between two limits. In plotting this section of the curve, we have had to meet only the requirement that the vertical distances between successive circles are equal—since vertical distance on the graph represents pitch-distance as perceived by a listener.

TABLE I

Data on Equal Sense-Distances

Entries give the adjustments made by 10 *O*s in dividing three different frequency ranges into four equal intervals of pitch. Each entry is the average of five settings. The bottom row gives the percentage average deviation, *V*, of the means for different *O*s about the mean of the group. The mean of this row is 6.5%—the inter-observer variability. The last column gives for each *O*, the mean of the percentage average deviation, Vo, of his own settings about his own means. The mean of this column is 9.9%, the intra-observer variability.

O	40–1000 ∼ per sec.			200–6500 ∼ per sec.			3000–12000 ∼ per sec.			Av. Vo (%)
JV	174	381	628	865	1850	3260	4090	6780	8440	9.4
SS	170	454	718	792	2230	3760	3970	5670	8080	8.6
GS	164	327	598	822	2060	3620	4090	5130	7290	6.7
CH	167	392	706	751	1696	3270	3820	5260	7540	8.7
JB	171	442	718	920	1810	2880	4620	5800	8320	11.9
DM	168	391	768	972	1920	3320	4100	5110	7840	5.4
DC	142	490	716	848	1859	3120	4410	5700	7480	12.7
AB	140	385	732	985	2390	3850	3940	5260	7120	12.2
AS	161	404	642	864	2340	3420	3840	5110	7580	13.8
FS	154	376	706	846	2065	3430	4210	5440	7740	10.3
Mean	161	404	693	867	2022	3393	4109	5526	7743	
V(%)	5.9	8.6	6.1	6.4	9.6	6.6	4.4	6.7	4.4	

Next we ask what form the function has outside the range 200 to 6500 ∼. As shown in Table I, *O*s also divided into four equal intervals another range of frequencies (40 to 1000 ∼) partially overlapping the first range. If, within this second range also, the *O*s have validly produced four equal intervals of pitch, then the settings in this range (indicated by the squares in Fig. 2) must be plotted in such a way as to meet two requirements: (1) the vertical distances between successive squares must be equal; and (2), throughout the range common to the two functions, the curve determined by the squares must coincide with the curve determined by the circles. If these two requirements can be met—as it appears in Fig. 2 they can be—then we can extend our function down to 40 ∼.

Subject to the same two requirements, the data for the range 3000 to 12,000 ~ can next be plotted, so as to extend the function to the higher frequencies. It should be noted here that an important criterion for the validity of the data expressing equal sense-distances (Table I) is met and satisfied when it is shown that the data can be plotted in such a way as to meet simultaneously the two requirements stated above. How well these requirements can be met by the data of the present experiment the reader will discover from Fig. 2. Or we may say: the fact that the data can be plotted as shown in Fig. 2 demonstrates that, even when the *O* is made to work with different ranges of frequency, the rate of change of

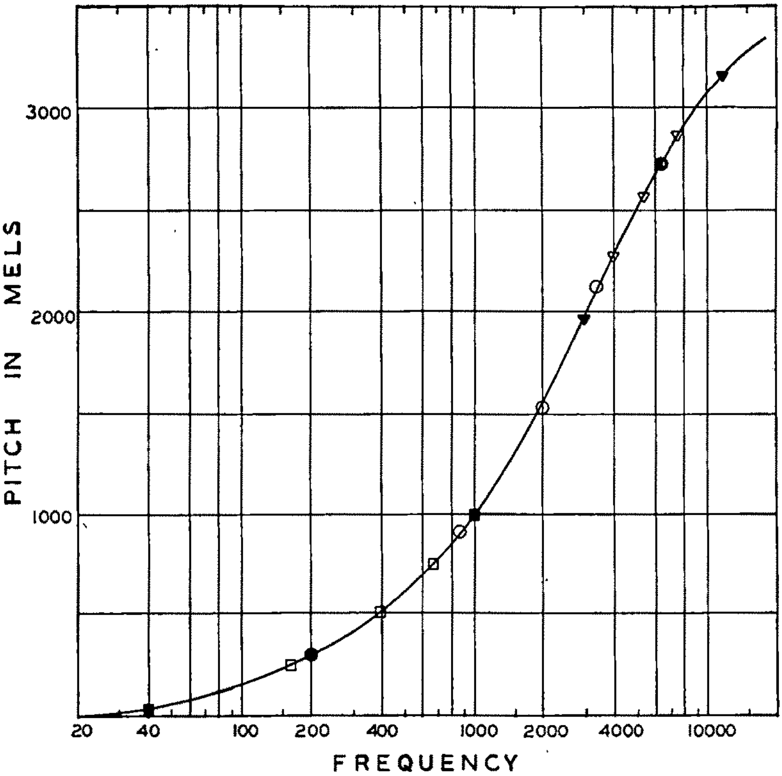

FIG. 2. THE PITCH-SCALE

The curve shows how pitch, scaled in subjective units (ordinates), varies with frequency. The circles, squares and triangles represent data obtained in the experiment on equal sense-distances. The filled figures mark the ends of three frequency-ranges and the hollow figures show the points arrived at when the *Os* divided the ranges into four equal intervals of pitch.

pitch with frequency is a function of the frequency and not of the range bisected, or quartered. In other words, the *slope* of the curve at each frequency within the region of overlap can be made the same for either of the two sets of overlapping data chosen from the three sections of Table I.

In constructing the curve between 40 and 12,000 ~ in such a way as to satisfy simultaneously the two requirements previously stated, we proceed by purely graphical methods involving no small amount of trial and error. The aim is to find the curve that will best represent all the data. The final construction is a curve whose ordinate has this property: equal distances along the ordinate represent equal pitch-distances. Such a curve could serve, between 40 and 12,000 ~, as an *intensive* scale of pitch, but it does not yet meet all the requirements of an *extensive* scale. It needs a zero point. The location of zero pitch on the ordinate of Fig. 2 can be determined in two ways: (a) by extrapolating the function to the frequency marking the lower limit of pitch perception, or (b) by making a direct estimate of the amount of pitch left in the region below 40 ~. Fortunately, in the present experiment both methods lead to the same result, provided we accept 20 ~ as approximately the lowest frequency giving rise to a sensation of pitch.[6] In order to test the validity of the extrapolated curve, 7 of the *O*s equated the pitch-distance between 25 and 40 ~ to an equal-appearing interval above 40 ~. In order to appear equal, this upper interval had, on the average, to extend from 40 to 52 ~. This result agrees with what we should predict from the extrapolation of the pitch-function to 20 ~. Consequently, we can extend the curve in Fig. 2 down to what we may reasonably call zero pitch.

Now we are in a position to choose a unit for our scale of pitch—an extensive scale at last. We can assign numerals to positions along the ordinate of Fig. 2 in such a way that the numerals, and their series-relations, will reflect the relations among perceived pitches. Accordingly, we may assign the numeral 1000 to the ordinate corresponding to a frequency of 1000 ~, and this numeral we may allow to stand for the number of *mels* in the pitch of a 1000 ~ tone. The *mel,* then, is a unit of pitch, and the magnitude of the discriminable aspect, pitch, exhibited by tones of various frequencies can be read from the plot in Fig. 2.

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[6] For a discussion of the threshold for very low tones, see E. G. Wever and C. W. Bray, The perception of low tones and the resonance-volley theory, *J. Psychol.,* 3, 1936, 101-114; G. von Békésy, Über die Hörschwelle und Fühlgrenze langsamer sinusförmiger Luftdruckschwankungen, *Ann. d. Physik,* 26, 1936, 554-566.

RELATION OF THIS NEW SCALE TO THE EARLIER SCALE OF PITCH

In the scale constructed by Stevens, Volkmann, and Newman,[7] the mel appeared as the unit and was defined in the same way: as one thousandth of the pitch of a 1000 ~ tone. The function relating pitch to frequency, however, turned out to be slightly different from that pictured in Fig. 2. Plotted in the coördinates of Fig 2 the two curves would coincide at 20 and at 1000 ~, but between these two frequencies the earlier curve would lie below, and above 1000 ~ the earlier curve would lie above the curve shown in Fig. 2. It was partly in order to discover, if possible, the reason for this discrepancy that the next experiment was designed.

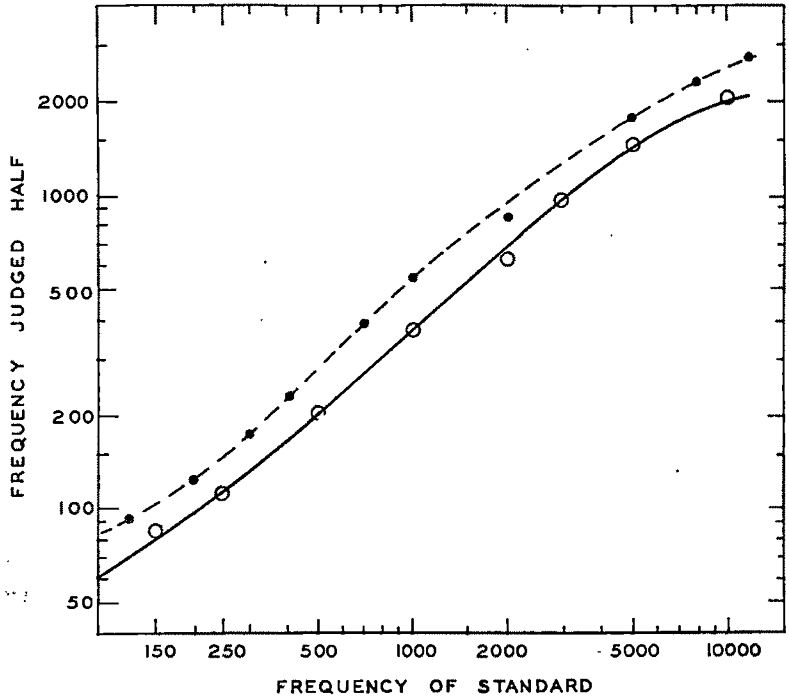

FIG. 3. FRACTIONATION

The results of two different experiments in which *O*s adjusted the frequency of one tone (ordinates) until it sounded half as high in pitch as a second tone (abscissas). The points on the dashed curve are for data obtained in 1936. The solid curve gives the locus of fractionations as they are predicted from the curve in Fig. 2. The hollow circles indicate the data obtained in the present experiment.

The nature of the discrepancy between the two pitch-functions is best shown in Fig. 3, where are plotted the data of the earlier experiment (dashed line) showing what frequencies were judged half as high in pitch as what other frequencies. The frequencies that would have been heard as half as high, *provided* Fig 2 contains the true pitch-function, are represented by the solid curve in Fig. 3 (let us ignore, for the present, the hollow circles). This solid curve, in other words, gives the predicted results for an experiment on fractionation.

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[7] Stevens, Volkmann, and Newman, *op. cit.*

It will be seen that in the earlier experiment, the frequencies judged half as high in pitch were systematically too high. Now, a study of these earlier results shows that the two *O*s, who made their fractionations in terms of a zero pitch which was definitely conceived as the lowest note on the organ, made fractionations which agree not too badly with the solid curve in Fig. 3. Perhaps, then, if the *O*s were allowed to hear a pitch closely approximating zero they would produce fractionations agreeing more closely with the results predicted by the experiment on equal sense-distances.

A NEW EXPERIMENT ON THE FRACTIONATION OF PITCH

In this experiment *O* was required to adjust the frequency of one tone until it sounded half as high in pitch as another tone of predetermined frequency. The two tones were presented alternately by a timing relay at 2-sec. intervals. Each tone lasted 2 sec. and was followed immediately by the other tone. *O* controlled the frequency of the second tone by turning a knob connected to a variable condenser which tuned a beat-frequency oscillator. The oscillator, amplifier and speakers were the same as were used in the experiment on equal sense-distances. The pro-

TABLE II

DATA ON FRACTIONATION

Entries give the results for 12 Os who adjusted one tone to half the pitch of various standard tones. Each entry is the average of five settings. The next to the last row gives the percentage average deviation, V, of the means for different Os about the mean of the group. The mean of this row is 9.4%—the inter-observer variability. The last row gives the mean of the percentage average deviations, Vo, of the individual settings about their own means. The mean o this row is 15.6%—the intra-observer variability.

O	Standard tones							
	150~	250~	500~	1000~	2000~	3000~	5000~	10,000~
JV	72	105	152	396	508	870	1166	1740
SS	80	108	232	338	577	1012	1358	2260
GS	81	110	174	356	546	1164	1380	1800
CH	89	120	190	376	664	1105	1980	2480
JB	91	125	214	416	724	1108	1206	2090
DG	82	104	210	388	494	856	1436	2120
ED	81	115	224	346	760	1118	1630	2090
JN	93	102	233	446	620	836	1226	2005
DM	92	116	252	400	594	—	1520	2520
WH	91	113	207	373	724	—	1566	2057
DC	81	103	197	261	638	—	1500	1850
AB	84	116	182	384	748	—	1270	1760
Mean	85	111	206	373	633	1009	1437	2064
V (%)	5.9	5.4	10.7	8.9	13.6	9.3	11.8	9.5
Av. Vo % for all *O*s	8.0	10.8	17.2	16.1	19.6	14.2	21.1	17.7

cedure was the same as that used in the earlier experiment, except for this crucial difference: *O* could, whenever he desired, turn a knob and introduce a 40 ~ tone. He was told that this tone was at very nearly zero pitch, and that he should make his fractionations with that in mind. In practice, the *O*s usually referred to the 40 ~ tone two or three times in the course of each fractionation.

The error introduced by the fact that 40 ~ is not precisely zero pitch is certainly negligible when high tones are being fractionated, but the same is not true for very low tones. Consequently, when the *O* was asked to fractionate a tone of 150 ~, he was not allowed to use 40 ~ as a reference tone.

The results obtained from 12 *O*s are shown in Table II. Each entry is the average of 5 settings. All but one of the 5 *O*s employed by Stevens, Volkmann, and Newman[8] served also in this experiment. These *O*s expressed the belief that the presence of the 40 ~ tone made the judgment easier, and, as we might expect, the agreement among the results was better when the reference tone was provided. Not only did the results agree better among themselves, but they also agreed better with the solid curve of Fig. 3. The hollow circles in Fig. 3 represent the means of the columns in Table II.

Two facts are strikingly in evidence: (1) the availability of a 'zero' for purposes of reference tends to lower the fractionation—a result which suggests that, without an objective reference, the *O*s tend to assume a zero pitch that is too high; and (2) the results obtained by the method of fractionation adequately confirm those gotten by the method of equal sense-distances. *The agreement of these two methods leads us to propose the function represented in Fig. 2 as the scale most acceptable, at present, for relating pitch to frequency and for measuring subjective pitch.*

Let us now consider certain minutiae. First of all it might be objected that the introduction of the 40 ~ tone transforms the experiment on fractionation into one on bisection. Perhaps this is so. We shall not quarrel with anyone wishing to use the latter term. Considered formally (logically), a fractionation to the value one-half is equivalent to a bisection between some higher point and zero. In any case, the procedure in this second experiment differed from that of the experiment on equal sense-distances in two important ways: (1) *O* used the reference tone only occasionally; and (2) he was instructed to use it as a reference tone, aiding him in his task of fractionation, and was instructed not to use it as a limiting tone in a self-imposed task of bisection. Despite these differences in procedure, the results of the two experiments are in striking agreement.

The fractionation of the 150 ~ tone, where the 40 ~ reference tone was not used, occurs at a lower frequency than we should have predicted on the basis of the earlier experiment. This difference is most likely due to improved apparatus. The earlier experiment suffered from the lack of devices capable of producing very low tones at a constant loudness-level. In the present experiment the *O* was given the opportunity of exploring the lower ranges of frequency down as far as 25 ~.

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[8] Stevens, Volkmann, and Newman, *op. cit.*

The fractionations of the 2000 ~ tone call for special attention. In both experiments this point is out of line (see Fig. 3). An examination of the individual adjustments shows what appears to be an explanation: in fractionating 2000 ~ the *O*s tend to set the second tone to one of the octave relations, either 500 or 1000 ~. There is a large cluster of results near 500 ~ and a smaller one near 1000 ~. The *O*s appear to be unaware of this tendency, and at no other frequency did such a tendency show itself. Upon discovering this anomaly at 2000 ~, we obtained judgments at 3000 ~ from as many *O*s as were still available, but found no tendency for the individual results to cluster about the lower octaves. All we can say is that the octave, or double octave, relation asserts itself when *O* is seeking a tone that sounds half as high in pitch as a tone of 2000 ~, but that these octave relations do not affect his judgment at other frequencies.

THE QUESTION OF VARIABILITY

The precision with which we can assign numerals to things depends, of course, upon our powers of discrimination, and these in turn are reflected in the variability of our judgments. The reliability of the scale of pitch here proposed depends, then, upon the variability of the settings made by the several *O*s. Table I shows what this variability amounted to when the *O*s were asked to divide an interval into four equal sense-distances. The variabilities are expressed in terms of the coefficient of variability (V)—the average deviation taken as a percentage of the mean. The bottom row of Table I tells us the variability found when we compare the average results for different *O*s (inter-observer variability), and the right-hand column gives the average of the variability shown by each *O* in making his own frequency-adjustments (intra-observer variability). The average intra-observer variability is 9.9%; the average inter-observer variability is only 6.5%. The relation between these two figures we will discuss shortly.

In the experiments on fractionation the variability was greater, as we should expect, than in the experiment on equal sense-distances. Table II gives the values for fractionation. Here the average intra-observer variability is 15.6% and the average inter-observer variability is 9.4%. In the earlier experiment, where fewer *O*s and no reference tone for zero pitch were used, the average inter-observer variability was 14.9%. When we compare directly the results of the four *O*s who took part in both experiments, we find that the procedure of the later experiment was effective in reducing the inter-observer variability by about 57%. Consequently, for an *O* whose task it is to fractionate a tone, an adequate point of anchorage for his own subjective scale appears to be definitely required.

There is a tendency for the average deviation to increase when the tone to be fractionated is made higher in frequency. This increase appears to be no simple function of the frequency, but on the assumption that the increase might be related to the subjective pitch of the tone fractionated, the variabilities shown in Table II were translated into subjective units and plotted in Fig. 4. The intra-observer average deviations, measured in mels, are plotted as hollow circles; the filled circles represent the inter-observer average deviations. The transformation of variabilities into deviations expressed in mels was accomplished with the aid of Fig. 1, or, more correctly, with the aid of its enlarged prototype. Fig. 4 indicates that the extent, in pitch, of the average deviation to be expected when an *O* fractionates a tone is approximately proportional to the perceived pitch of the fractional tone. Likewise, the variations among different *O*s are directly proportional to the subjective pitch of the tone with which they are working.

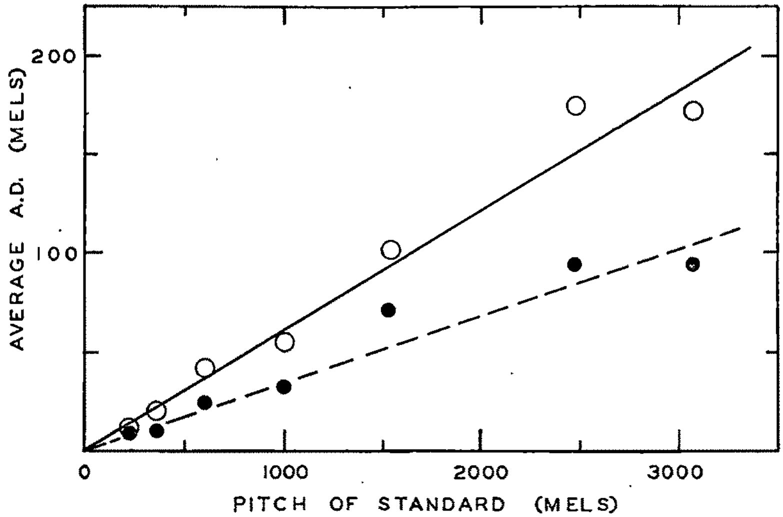

FIG. 4. VARIABILITY

The variability of fractionations depends upon the pitch of the tone fractionated. Both the average deviations and the pitches of the tones are expressed in subjective units (mels). The hollow circles show the intra-observer deviations; the filled circles show the inter-observer deviations.

One more interesting relation emerges from the data on variability. The ratio of the inter-observer to the intra-observer variability is 0.66 for the experiment on equal sense-distances and 0.61 for the experiment on fractionation. Now, if our assumption is correct that all *O*s are carrying out the same task, as set for them, and if we assume that the deviations of their settings from the 'true' value are due to 'chance' factors, and not to the fact that different *O*s do different things when asked to judge pitch, then we can apply certain notions derived from the theory of sampling. There is a formula relating the variability in a small sample chosen at random from a normal distribution and the variability of the means of several small samples so chosen. It is the familiar formula for the reliability of the mean: $AD_m = AD/(N\text{-}c)^{1/2}$. Applied to the present experiment, AD is the intra-observer variation, AD_m is the inter-observer variation, N is the number of settings made by each *O* for a given tone, and c is a factor dependent upon the size of N. The value of c for N less than 10 is usually taken as 3, but this is admittedly only an approximation. For $N = 5$ and $c = 3$ this formula says that the ratio of the inter- to the intra-observer variability should be 0.707—a value reasonably close to the two values cited above.

In making this comparison between the ratio predicted and the ratio obtained, we have lumped the variabilities for all *O*s at all frequencies. Nevertheless, despite a possible obscuring of minor irregularities, it is probably safe to conclude that this analysis of variability supports the assumption that the *O*s were operating under the same set and *Aufgabe* and that their deviations were due mainly to chance factors. At least a necessary condition for such an assumption has been fulfilled. If each *O* had been performing under a different attitude—some *O*s trying to adjust to various musical intervals, others to equal pitch-distances, for example—the ratio of inter-observer to intra-observer variability would most likely have been larger than 0.707 instead of smaller.

Agreement with Antecedent Experiments

The data plotted in Fig. 2 do not, of course, represent the first attempt of psychologists to establish equal intervals of perceived pitch. Other experimenters have bisected and equated pitch-distances. Consequently, it is instructive—and in this case somewhat comforting—to compare the results of previous workers with the function in Fig. 2. A section of this function (extending from 100 to 1000 ~) is reproduced in Fig. 5 and with it are plotted data obtained by Pratt, Münsterberg, and Lorenz in experiments on the bisection of tonal intervals.

Lorenz[9] performed the experiment which launched the historic feud between Wundt and Stumpf.[10] He found that the bisection of the interval between two frequencies occurred at or near the arithmetic mean of these frequencies. Wundt stamped these results with his approval, but Stumpf dissented. Stumpf, the musician, held that the bisection should be nearer the geometric mean—the point which divides an interval into two equal *musical* intervals. Our present pitch function (Fig. 2) suggests that both parties to the controversy were correct, provided one is free to choose the range of frequencies to be bisected. In the region where the curve (Fig. 2) is rectilinear, or nearly so (from about 1500 to 6000 ~) Stumpf's contention is borne out in experiment. In this region bisections are made at or near the geometric mean and equal musical intervals sound approximately equal in pitch-extent. But in the region below 1500 ~, bisections are made slightly nearer the arithmetic than the geometric mean, although they seem never to be made quite at the arithmetic mean. This point is illustrated in Fig. 5 where two of the longer intervals used by Lorenz are plotted. The points of bisection are

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[9] C. Lorenz, Untersuchungen über die Auffassung von Tondistanzen, *Philos. Stud.*, 6, 1890, 26-103.

[10] E. G. Boring, *A History of Experimental Psychology*, 1929, 371.

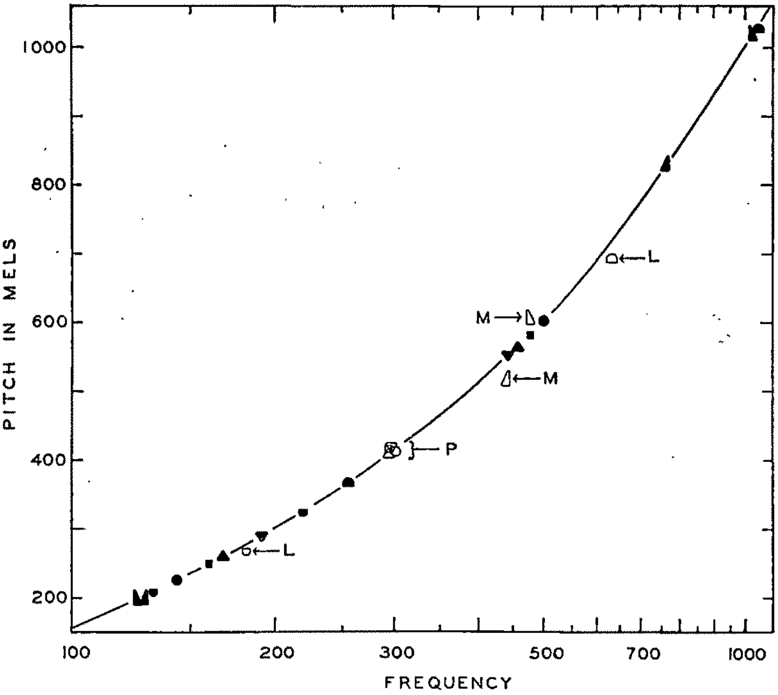

FIG. 5. HISTORICAL COMPARISON

Comparison of the pitch-function with data on the bisection of intervals obtained by Pratt (P), Münsterberg (M), and Lorenz (L). The hollow figures mark the points of bisection. The ends of the interval bisected are marked by filled figures of corresponding shape.

marked L, and it will be seen that these points lie near but definitely below the pitch-function.

Münsterberg set out to settle the issue with some bisections of his own.[11] His results appeared to him to confirm Lorenz, but when we plot Münsterberg's data for the longer intervals we obtain the result shown in Fig. 5. The figures marked M represent these bisections. Both bisections fall very near the point predicted by the pitch-scale; and, unlike Lorenz's data, the deviations are in both directions. It should be noted too that we have plotted the data for the two longest intervals employed by Münsterberg; his results for shorter intervals fall even closer to the curve in Fig. 5.

Further support for the validity of our present pitch-scale is found in Münsterberg's experiment in which he equated two pitch-extents that were not adjacent.[12] The ends of these two extents were defined by four different tones, as shown in Table III. Here we can compare the results obtained with what we should predict from the pitch-function in Fig. 2. Two of Münsterberg's four intervals check perfectly; the other two deviate slightly, and in opposite directions, from the predicted values.

TABLE III

A COMPARISON

Data obtained by Münsterberg are here compared wtih the values predicted by the pitch-scale.

Standard interval 256~ to —	Obtained interval — to 456~	Predicted interval — to 456~	Size obtained interval in mels	Size predicted interval in mels
276	426	430	22	23
296	406	406	44	44
316	388	388	63	63
336	374	366	80	75

Perhaps the most adequate of the previous experiments, from the point of view of apparatus and technique, is that of Pratt.[13] He used four intervals, each larger than an octave, and required his *O*s to bisect the intervals by themselves adjusting the frequency of the bisecting tone. The results are shown in Fig. 5 by the group of points marked P. It is clear that the present pitch-scale confirms the results of Pratt as closely as experimental error would allow us to expect.

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[11] H. Münsterberg, *Beiträge zur experimentellen Psychologie*, 4, 1892, 147-177.

[12] For these results see E. B. Titchener, *Experimental Psychology*, vol. 2, part 2, 1905, 243.

[13] C. C. Pratt, Bisection of tonal intervals larger than an octave, *J. Exper. Psychol.*, 11, 1928, 17-26.

Assuming, then, that we have in Fig. 2 a pitch-scale possessing adequate validity, as shown by comparison with other experiments, and reasonable reliability, as revealed by the *Os*' variability, what can we do with it? The remainder of the paper is devoted to this question.

THE SUBJECTIVE SIZE OF MUSICAL INTERVALS

As was pointed out in connection with the earlier pitch-scale, we can measure the subjective size of various musical intervals by determining from Fig. 2 how much the pitch changes in going from one end of the interval to the other. In general, the subjective size of a musical interval is proportional to the slope of the pitch-function at the frequency which falls in the middle of the interval.

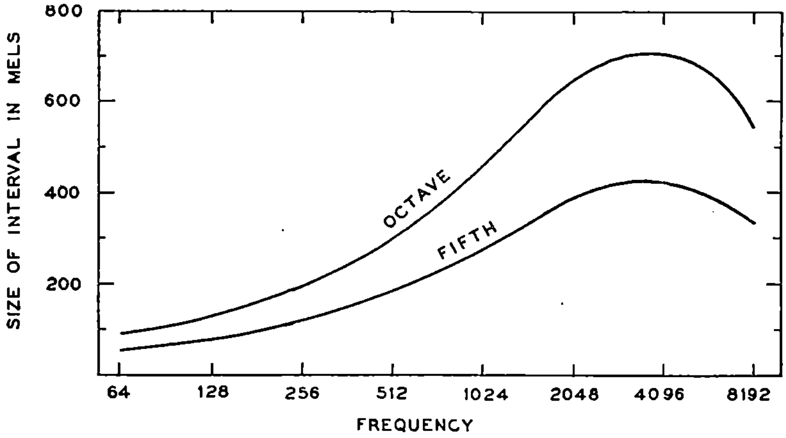

FIG. 6. PITCH-EXTENT OF MUSICAL INTERVALS

The pitch-extent of musical intervals depends upon where the intervals are located on the frequency-scale. The values on the abscissa represent the mid-points of the intervals whose subjective extents are determined from the ordinates.

In order to illustrate these relations, the subjective size of successive octaves and fifths, as measured in mels, is plotted in Fig. 6. The plot for other intervals would be similar in form but different in ordinate value.

Quite definitely, musical intervals become subjectively larger as frequency increases up to about the fourth octave above middle C. In other words, throughout what is usually called the musical range, intervals made up of equal frequency-ratios increase in perceived pitch-extent with increasing frequency of the stimulus. This conclusion is not entirely novel.

Stumpf declared that, according to his own perception of them, the upper octaves are perceptually larger than the lower octaves.[14] Thus the pitch-scale enables us to confirm and quantify facts apparent to an old master.

Incidentally, as pointed out elsewhere,[15] these facts contradict the wide-spread notion that "equal ratios of frequency give rise to equal intervals of pitch."[16] They also discourage what appears still to be a prevalent belief; namely, that the musical scale is a subjective scale. Woodworth says: "A well developed and much used subjective scale is seen in music; the violinist tunes his four strings to equal intervals by ear, and not by counting the vibrations."[17] That to which the musician actually tunes his strings is a certain ratio of *frequencies,* not *pitches,* because the musical scale is a scale of frequencies. On this scale, "equal interval" means "equal in frequency-ratio," but Fig. 6 shows that equal frequency-ratios do not, in general, produce equal pitch-distances.

RELATION OF THE PITCH-SCALE TO THE FUNCTIONING OF THE INNER EAR

Stevens, Volkmann, and Newman compared their pitch-scale of 1936 with the function relating the frequency of tones to the region stimulated on the basilar membrane.[18] They found reasonably good agreement, but in the case of the revised pitch-scale the agreement is, if anything, even better. The comparison is made as follows.

Experiments designed to determine the location of the region on the basilar membrane which is made to vibrate most vigorously by different frequencies have been conducted by means of the electrical recording of potentials generated in the ears of guinea pigs. As a result of these studies, maps have been made of the basilar membrane locating the region most sensitive to each audible frequency. The data obtained in such an experiment by Stevens, Davis, and Lurie[19] are shown by the hollow rectangles and circles in Fig. 7. The filled circles are taken from Steinberg's[20] report of a 'mapping' experiment conducted by Culler. Now, if we lay the pitch-scale off along the distance (ordinate) representing the linear extent of the basilar membrane, we can plot the solid curve shown in Fig. 7, and we find that the functions coincide to a degree well within experimental error. If, as appears probable, the maps of the basilar membrane shown in Fig. 7 approximately represent the human cochlea, we can proceed to some interesting conclusions.

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[14] C. Stumpf. *Tonpsychologie,* 1, 1883, 250.

[15] Stevens and Davis, *op. cit.,* 84.

[16] A. H. Davis, *Modern Acoustics,* 1934, 235.

[17] R. S. Woodworth, *Experimental Psychology,* 1938, 394.

[18] Stevens, Volkmann, and Newman, *op. cit.*

[19] S. S. Stevens, H. Davis, and M. H. Lurie, The localization of pitch perception on the basilar membrane, *J. Gen. Psychol.,* 13, 1935, 297-315.

[20] J. C. Steinberg, Positions of stimulation in the cochlea by pure tones, *J. Acoust. Soc. Amer.,* 8, 1937, 176-180.

Apparently frequencies separated by equal intervals of pitch stimulate the basilar membrane at equi-distant points. Consequently, when an *O* is asked to adjust one tone until its pitch is half as high as that of another tone, he would be adjusting the frequency until it stimulates a position on the basilar membrane midway between the position stimulated by the higher tone and the apical end of the membrane. He would not, of course, be aware of these locations as such, but the underlying physiological process which makes comparison of pitches possible appears to be characterized chiefly by a spatial differentiation of such a simple sort. This inference accords with the fact that, except for slight differences

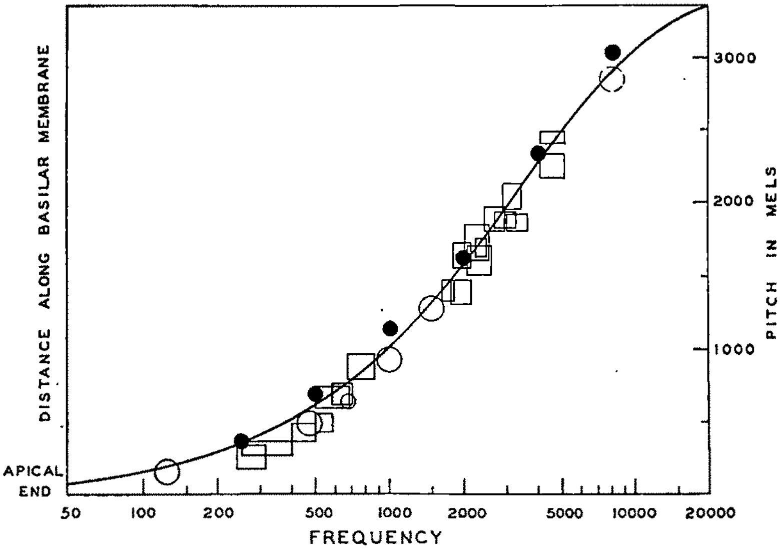

FIG. 7. LOCATION OF PITCH ON THE BASILAR MEMBRANE

Comparison of the pitch-function with data locating the positions on the basilar membrane which are stimumlated by tones of various frequencies. The linear extent of the basilar membrane is represented by the ordinate (left) and along the same axis is laid the pitch-scale (right). The curve represents the pitch-function; the hollow rectangles and circles (Stevens, Davis, and Lurie) and the filled circles (Culler) show the locations in the cochleas of guinea pigs of stimulation by various frequencies.

at the two ends, the neural innervation of the basilar membrane is of essentially uniform density throughout its length.[21]

Although there are subsequent central nervous processes, it appears then that the form of certain discriminatory functions is imposed by the receptor mechanism.

RELATION OF THE PITCH-SCALE TO DIFFERENTIAL SENSITIVITY

When Fechner performed the mathematical integration leading to what we call the Weber-Fechner law, one of his assumptions was that just perceptible differences are not only equal in the sense of being equally noticeable, but are subjectively equal as well. Psychologists have long debated the validity of this notion. Equipped with an adequate scale of subjective pitch, we should be able to resolve this question as to whether two frequencies which are just noticeably different are also separated by equal intervals of pitch. Stevens, Volkmann, and Newman[22] made the necessary comparison of the earlier pitch-scale with some of the data on differential sensitivity obtained by Shower and Biddulph[23] and they were able to conclude that, within the experimental error, all jnd's determined at a constant loudness-level were subjectively equal, as measured by the 1936 scale of pitch.

Now, since considerable variety exists among data reported in experiments on differential sensitivity to frequency, we propose here to follow a somewhat novel procedure. We shall not compare the new pitch-scale with a function obtained by summating DL's; instead, we shall first make the assumption that jnd's for pitch are subjectively equal, and then proceed to differentiate the pitch-function (Fig. 2) in order to discover the size of the DL's as predicted by the pitch-scale. In other words, we are going to carry out a reversal of Fechner's procedure: he began with the DL and arrived at a magnitude function for sensation—we begin with a magnitude function and find something out about the DL.

According to the assumption just made, the number of DL's within any small frequency-interval is proportional to the number of mels within the interval. In order to evaluate the constant of proportionality here involved, let us make a second assumption: that between 2000 and 3000 ~ the value of the fraction $\Delta F/F$ is 0.002. All experimenters are agreed that over this region the fraction is substantially constant and there is adequate evidence that 0.002 is a reasonable value. We can now proceed to compute the number of DL's between 2000 and 3000 ~ (the number is 202), and, since we know the number of mels between these frequencies (about 420 mels, as determined from Fig. 2), we can take the ratio of these two numbers and thereby obtain the constant of proportionality relating DL's to mels. By graphical methods we can now determine from Fig. 2 the number of DL's within any small frequency-range and hence discover the size, in cycles, of DL's at various frequencies. Next we change the DL's into relative DL's, *i.e.* we express the DL as a fraction ΔF/F, and plot the result as shown by the solid curve in Fig. 8.

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[21] For a plot showing the density of innervation of the basilar membrane, see H. Fletcher, The mechanism of hearing as revealed through experiment on the masking effect of thermal noise, *Proc. Nat. Acad. Sci.*, 24, 1938, 265-274, esp. Fig. 6.

[22] Stevens, Volkmann, and Newman, *op. cit.*

[23] E. G. Shower and R. Biddulph, Differential pitch sensitivity of the ear, *J. Acoust. Soc. Amer.*, 3, 1931, 275-287.

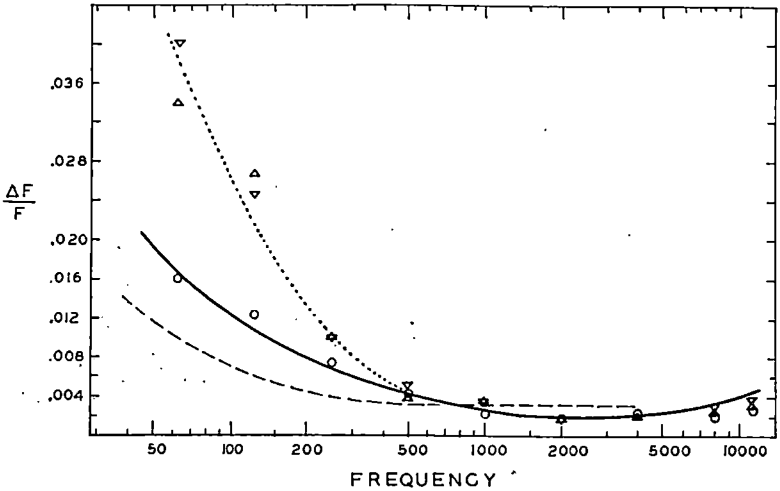

Fig. 8. Weber's Fraction as Computed from the Pitch-Function
The solid curve shows the value of Weber's fraction as predicted by a differentiation of the pitch function in Fig. 2. Knudsen's data for measurements of ΔF/F are plotted as a dashed line. The remaining points are for data gotten by Shower and Biddulph: the inverted triangles they obtained at a sensation-level of 40 db., the other triangles at 50 db. The hollow circles stand for data obtained under their method of abrupt variation—the traditional technique for measuring differential sensitivity.

This solid curve in Fig. 8 is our prediction, made from the pitch-scale, of the function relating Weber's fraction, ΔF/F, to frequency. The agreement of this curve with actual experimental determinations is satisfactory for frequencies above 500 ~, where the experimental results themselves are generally consistent. Below 500 ~, however, different experimenters, using different experimental techniques, have produced widely divergent data. The curve we have derived from the pitch-scale may or may not aid us eventually in selecting among these data, but certain comparisons are interesting. Knudsen's data are presented in Fig. 8 as a dashed line.[24] Among the lower frequencies, Knudsen obtained smaller values for $\Delta F/F$ than are predicted by the solid curve. Shower and Biddulph criticized Knudsen's technique because it allowed transient frequencies (introduced by the switching on and off of the stimulating tones) to provide the *Os* with unwanted cues. These critics then set out to remedy the situation by means of an apparatus which changed a tone from one frequency to the other in a continuous manner. Their stimulus, therefore, was a frequency-modulated tone.[25] With this stimulus they obtained the data represented by the triangles in Fig. 8.

Shower and Biddulph also made some determinations of $\Delta F/F$ using the more traditional method of abrupt variation, where the switching from the lower to the higher tone is instantaneous rather than gradual. Their results under this method are shown by the circles in Fig. 8. These results accord with the values predicted by the pitch-scale, but, before we can decide upon the validity of this agreement, we shall have to probe more thoroughly the factors influencing differential sensitivity to frequency—especially for the lower tones.

Hypothesis for a General Law of Psychophysics

It will be remembered that the solid curve of Fig. 8 was derived from the assumption that all jnd's for pitch are subjectively equal, *i.e.* all tones separated by a given number of DL's will appear equi-distant in pitch. The situation disclosed by Fig. 8 suggests that, to the extent that present data are able to decide the issue, this assumption is confirmed by experiment. We can consider the jnd's for pitch, at a constant loudness level, to be subjectively equal.

On the other hand, an analogous treatment[26] of the data for the discrimination of intensity shows clearly that jnd's for loudness are *not* subjectively equal. Instead, these jnd's increase rapidly in size as the intensity of the stimulus is raised.

Now, the physiological mechanisms which mediate our discriminations of pitch and loudness are of two basically different types. When the frequency of a tone is raised the pattern of excitation is shifted along the basilar membrane—new excitation is *substituted* for old. Thus we tell one pitch from another because the locus of excitation on the basilar membrane has been altered. When we increase the intensity of a tone, however, the general locus of the excitation in the cochlea remains relatively fixed and new excitation is *added* to old. Thus we detect that one tone is louder than another tone of the same frequency whenever there is an addition of sufficient excitation. Perhaps the subjective size of jnd's is related to the kind of discriminatory mechanism which mediates them.

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[24] V. O. Knudsen, The sensibility of the ear to small differences in intensity and frequency, *Phys. Rev.*, 21, 1923, 84-103.

[25] For a discussion of the nature of this stimulus, see Stevens and Davis, *op. cit.*, 89-94.

[26] Stevens and Davis, *op. cit.*, 148-152.

Considerations of this sort have led to the following hypothesis. All jnd's based upon a physiological process involving a change in the general location of excitation (or possibly a change in its form of distribution) will be found to be subjectively equal in size. All jnd's produced by a process of adding excitation to excitation already present will be found to increase in subjective size with increased magnitude of the stimulus.

Such an hypothesis may eventually find itself refuted.[27] It will almost certainly have to submit to some degree of revision. Corrections will be in order after we have made reliable measurements of differential sensitivity and after we have created adequate subjective scales by methods similar to those of the present experiment.

SUMMARY

(1) Our purpose was to erect a new subjective scale for the measurement of pitch. This scale was to be an *extensive* scale—one to which numerals are assigned in such a way that the positional relations among the numerals in the numeral-series are reflected in perceived relations among the pitches.

(2) We also sought the answer to certain questions arising from previous experimentation: Can the same scale be obtained by different methods, such as fractionation, bisection and equal sense-distances? How much variability is to be expected within the results of each *O* and among the results of different *O*s? How does pitch behave at very low

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[27] The hypothesis appears to be confirmed by such meager data as are now available for the discrimination of visual brilliance and saturation. As already pointed out elsewhere (for reference see footnote 2), the data indicate that jnd's for saturation are subjectively equal, whereas the jnd's for brilliance grow in subjective size as the intensity of the stimulus increases. A jnd in brilliance, we might assume, occurs when the total amount of visual excitation is adequately increased; whereas a jnd in saturation is probably mediated by a change in the relative distribution of the excitation among the different types of receptors in the retina.

frequencies? What happens in an experiment on fractionation when *O* is supplied with a reference tone defining zero pitch?

(3) Two experimental methods were employed. (a) Ten *O*s adjusted groups of frequencies in such a way as to divide each of three partially overlapping frequency-ranges into four equal pitch-distances. (b) Twelve *O*s adjusted a second tone until it sounded half as high in pitch as a given standard tone.

(4) The results of these two experiments showed excellent agreement—well within experimental error—and from the data a scale of pitch (Fig. 2) was constructed. The unit of the scale is called a *mel* and is defined as one thousandth of the pitch of a 1000 ∼ tone.

(5) This new scale is like, but not identical with that proposed in 1936 by Stevens, Volkmann, and Newman. The differences are due to apparatus and to method. The new scale accords strikingly with data on the bisection of tonal intervals obtained by Pratt, and is in fair agreement with the older data of Lorenz and Münsterberg.

(6) Using the pitch-scale to measure the subjective size of musical intervals, such as octaves and fifths, we find that the pitch-extent corresponding to a given interval varies with the frequency-region in which the interval occurs. Thus octaves and fifths increase in subjective size as we go up the musical scale to about the fourth octave above middle *C*.

(7) Comparison of the new pitch-scale with functions showing the location of stimulation on the basilar membrane demonstrates that frequencies separated by equal intervals of pitch excite the basilar membrane at points of approximate equi-distant separation.

(8) The pitch-scale was used to predict the differential sensitivity of the ear, as a function of frequency. From this analysis we are able to conclude that all jnd's for pitch are essentially equal in subjective size.

(9) Since the jnd's for loudness are not equal in subjective size, and since the mechanism for pitch discrimination differs from that for loudness, the facts of the present experiment support the hypothesis that the subjective size of jnd's depends upon which of two basic mechanisms is involved in their production.

THE CRITERIA USED IN PREFERENTIAL JUDGMENTS OF GEOMETRICAL FORMS

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Few attempts have been made by psychologists to disclose the nature and manner of operation of the factors determining our reactions to forms although a considerable amount of work has been done on determining preferences for them. The most extensive treatment is that of Fechner,[1] who discussed in 1876 some of the factors which determine our preferences for geometrical shapes, distinguished between 'formal' and 'associative' factors, and carried out experiments on preferences for rectangles.

The formula of the 'esthetic measure' devised by Birkhoff treats only of the formal or geometrical properties of forms.[2] Holding that the esthetic experience in general is determined by the density of ordered relations in the esthetic object, he expressed this mathematically by the equation $M = O/C$, where M is the esthetic value, O the order, and C the complexity. Applying this general formula to various types of esthetic objects, he attempts to specify exactly what O and C are for a series of polygons and derives their 'esthetic measure.'

The validity of the reasoning upon which the formulae are based has been attacked by Davis who found that there was no correlation between the esthetic measure of the forms and the average rank assigned by two groups of *Ss*.[3] Beebe-Center and Pratt also found large individual differences among their *Ss* in evaluating the forms.[4]

Harsh, Beebe-Center and Beebe-Center attempted to isolate some of the "differential factors" or "principles of judgment" by means of factor analysis.[5] Thirty *Ss* ranked 26 forms selected from the entire range of values as given by Birkhoff's formula. Forms which possessed possibilities of meaning, or common associations, such as crosses, stars, and crowns, were eliminated. Four independent common factors were disclosed which were of differing importance in the judgments. Psychologically interpreted they were described by the authors, in approximate order of their importance, as "smoothness of contour" or "complexity of design within a simple

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[1] G. T. Fechner, *Vorschule der Aesthetik,* 3rd ed., 1925, 100-108; 184-202.

[2] G. D. Birkhoff, *Aesthetic Measure,* 1933, Chaps. 1-3.

[3] R. C. Davis, An evaluation and test of Birkhoff's aesthetic measure formula, *J. Gen. Psychol.,* 15, 1936, 231-240.

[4] J. G. Beebe-Center and C. C. Pratt, A test of Birkhoff's aesthetic measure, *ibid.,* 17, 1937, 339-353.

[5] C. M. Harsh, J. G. Beebe-Center, and R. Beebe-Center, Further evidence regarding preferential judgment of polygonal forms, *J. Psychol.,* 7, 1939, 343-350.

outline;" symmetry, with rotational and diagonal more important than horizontal and vertical; simplicity and regularity; and presence of "odd points" or "tall upward reachingness."

It was our purpose in this study to discover what reasons would be given by a group of *S*s for their preferences for a set of geometrical forms chosen at random, as expressed in a rank order. It was undertaken more as an exploration to discover what types of considerations, either arising from the actual characteristics of the shapes or on their 'meaning' for the *S*s, would be found and what information about them could be secured from the *S*s' verbal statements than to measure quantitatively their importance or analyze their psychological nature.

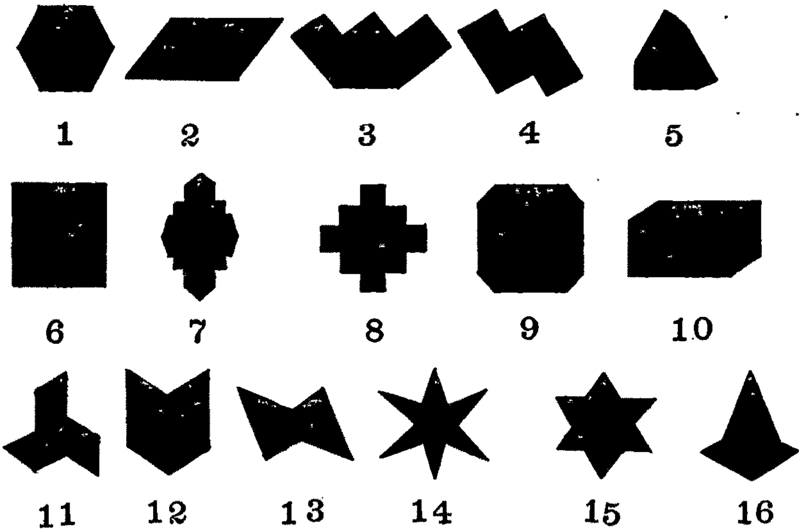

FIG. 1. THE GEOMETRICAL FORMS USED AS STIMULUS-FIGURES

Since this type of experimental analysis is based on the *S*s' remarks, it has obvious limitations since it depends on the extent to which the *S*s were aware of the factors operating to bring about their judgments and in their ability to analyze and describe them. Nevertheless it seemed desirable to ascertain what principles of judgment would be found and to gain some idea of their relative importance.

MATERIALS AND INSTRUCTIONS

The *S*s were 50 women; sophomores or juniors and members of an introductory class in psychology. The stimulus-figures, 16 geometrical forms, were cut out of dull blue paper and mounted on white cardboard rectangles 3 x 4 in. in size. These forms were selected from the list of 90 analyzed by Birkhoff and given an 'esthetic measure.' They are illustrated in Fig 1, with their number as used in this experiment.

These forms were presented to the *Ss* individually with the request that they be arranged in rank-order according to preference. When this was completed, *E* asked *S* to give the reasons for the judgments, allowing *S* complete freedom of expression and order of treatment. If some reason was not clear, *E* asked a question or two, but no attempt was made to push the *S* into definitions or esthetic theory, and if a clear reply was not forthcoming the matter was dropped. An attempt was made in every case to treat the matter easily and not to make it so important that the *S* would feel forced into rationalizations or "conceptualizations." When this was completed *S* was requested to successively arrange these same forms in the order of their complexity, symmetry, and familiarity.

To determine the esthetic sophistication of the *Ss* they were asked a number of questions at the end of the experiment about the art work, drawing, or painting they were then taking or had taken in high school, the appreciation courses they had had or were then taking, the household art courses taken, the extent of their interest in visual art, as shown by museum attendance, leisure-time artistic activities, or other behavior, and the extent to which they remembered any theories taught them as to what made a figure or shape beautiful or ugly.

Eight *Ss*, 16% of the group, indicated by their answers that they were at the time of the experiment more or less actively engaged in design work, generally in household art courses.[6] The remainder had had no actual art courses or very few and did not show any interest in visual arts. In general, then, we were apparently dealing with an unsophisticated group to the extent that they were not overly interested in visual art, had very little training or particular experience with it and did not show evidence of being motivated by theories of proportion or esthetic principles relevant for the material of the experiment.

RESULTS

Preferences. Table I gives the group order of preferences for the forms. The specimens are ranked according to their median rank values. Their 'esthetic measure' as given by Birkhoff is also included.

It will be seen from this table that the *Ss* did not agree very well in their relative evaluations of the stimulus-figures. Five of them were placed in every rank position from first to last place. Almost every one was liked most by some *Ss* and disliked most or was placed low in order by others. A rough group-order is, however, discernible.

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[6] Of this group of *Ss* only 3 stated that they remembered theories of beauty taught to them in school which might have influenced their judgments. They were on the whole, however, quite vague. One *S* had some recollection that slenderness and gracefulness were always beautiful. Two of them remembered a little about the principles of composition and proportion but were unable to be specific when questioned. One *S* stated that she remembered a great deal, which was an over-estimation, for it turned out that she only remembered being taught that "plain forms were not as interesting as complicated and fancy ones, especially those that looked different." Her rank order did not follow this principle.

Despite the lack of agreement among the *S*s some of the stimulus-figures are evaluated fairly uniformly. No. 5, for instance, is clearly disliked by the group, though some rank it as high as fourth. Nos. 14 and 7 were rather uniformly liked, though placed in last place by some of the *S*s.

Examination of the individual arrays showed that certain of them departed considerably from the consensus of opinion and were quite different from the group order. Eight such orders stood out as being greatly at variance with the judgment of the other *S*s.

Bases of judgments. A survey of the reasons or 'criteria' advanced by the *S*s for their preferences reveals a wide variety. Some were apparently interested in the properties of the figures, in such qualities as symmetry or asymmetry, complexity or simplicity. Connotative, or associative factors were also present, as were statements indicating the *S*'s recognition or lack of recognition of the shape and her past experience with it. To other *S*s, the potentialities of the form for use in large designs, as on textiles or tiles, served to determine its rank-order.[7]

TABLE I

RANK-ORDER OF STIMULUS-FIGURES JUDGED FOR PREFERENCE, WITH MEDIAN RANK, STANDARD DEVIATION, RANGE, AND BIRKHOFF'S VALUE OF ESTHETIC MEASURE

Rank-Order	Stim.-Fig.	Med. Rank	S.D.	Range	Esth. Meas.
1	14	.4	3.4	1–16	.33
2	7	.7	3.9	1–16	.25
3	8	3.4	3.5	1–15	.50
4	15	3.8	3.7	1–15	1.00
5	16	4.7	4.4	1–16	.00
6	1	6.4	2.8	1–14	1.00
7	12	8.3	2.8	2–15	.17
8	9	8.6	3.6	2–16	.50
9	10	9.0	3.4	2–15	.33
10	4	9.4	3.7	1–15	.29
11	2	9.7	3.2	2–16	.50
12	3	9.8	3.8	1–16	.11
13	13	10.4	3.4	3–16	.00
14	6	10.6	3.5	1–16	1.50
15	11	12.0	3.9	2–16	−.11
16	5	15.1	2.7	4–16	.00

'Formal criteria' are those which refer to the actual properties and characteristics of the form, either specific parts or the general appearance and basis of construction. 'Connotative criteria' are those which are based on the *S*s experience with the form in the past, either a specific association or a recognition of the forms familiarity or novelty. 'Design potentiality' refers to the considerations by the *S*s of the possibilities of practical application of the form in textiles, tiles, or in non-specified contexts.

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[7] As might be expected, many statements were made, presumably as reasons, which were indefinite and which the *S* could not elucidate. Among these were statements expressing reasons which apparently referred to the characteristics of the forms, such as "clear-cut" and "completeness of figure," while some apparently were more like evaluative comments, "interesting" or "nice shape." Three *S*s could give no clear reasons for their judgments.

In Table II is presented a list of the reasons for judgments which were understandable, the frequency with which they were mentioned by the group of 50 *S*s, and the number of times each of these criteria were the sole basis of a *S*'s judgments.

TABLE II

TYPES OF CRITERIA AND FREQUENCY OF OCCURRENCE

	No. Ss mentioning	Frequency with which only criteria given
(1) Formal criteria		
(a) Part interests		
Angles	7	1
Lines	4	
(b) Whole interests		
Symmetry	16	1
Complexity	14	2
Simplicity	7	
Regularity of construction	4	1
Irregularity	1	
Definite pattern or form	9	1
Organization; 'good design'	2	
Rhythm of construction	2	1
(2) Connotative criteria		
(a) Specific associations	13	1
(b) Recognition		
Familiarity	3	
Unfamiliarity	11	1
(3) Design potentiality	8	
		Total 9 (18%)

It is apparent that, for this group of *S*s and these forms, those interests which we classify as formal were most important; and of these those concerned with the appearance or structure of the form are of greatest significance. Symmetry of construction and complexity-simplicity were frequently given formal criteria.

Table III gives the frequency with which judgments were based on one type of criterion or on several, and the combinations that were found. Nine *S*s (18% of our group) held to one criterion for all their judgments, counting the *S* who judged on the basis of the specific associations evoked. Almost half of the *S*s (46%) based their judgments on one *type* of criterion only.

It is apparent that evaluative judgments for this type of material are likely to be based on a number of different *types* of criteria or on a number of different criteria of the same general type, as with the many formal interests. The judgment of a whole series of forms on the basis of the same demand or interest is not the usual thing.

Formal criteria. The formal interests fall into two fairly distinct types: those concerning a specific quality or property of *parts* of the figure, or presence in the shape of a valued characteristic, such as the presence of angles, lines of various kinds, and proportions, and those concerned with the appearance or characteristics of the *whole* shape or its "structural relationships," such as symmetrical construction and complexity.

TABLE III

FREQUENCY WITH WHICH PREFERENCES WERE MADE ON BASIS OF ONE OR MORE TYPES OF CRITERIA AND THEIR COMBINATIONS

One type of criterion only			
Formal			
1 specific criterion only	7		
2 or more specific criteria	13		
	20	20	
Connotative			
Specific association	1		
Unfamiliarity	1		
	2	2	
		22	44%
Two types of criteria			
Formal and specific association		8	
Formal and familiarity	2		
Formal and unfamiliarity	5		
	7	7	
Formal and design potentiality		2	
		17	34%
Three types of criteria			
Formal, specific association and familiarity		1	
Formal, specific association and unfamiliarity		1	
Formal, specific association and design potentiality		3	
Formal, design potentiality and unfamiliarity		3	
		8	16%
No criteria		3	6%
Total		50	100%

(a) Part characteristics. A small number of *S*s (7 or 14%) mentioned the specific characteristic of the angles on the forms as important in determining their preference for the forms.

Three *Ss*, *C*, *J*, and *L*, stated that they liked forms with "angles" protruding. *C* qualified this by saying that there must not be too many of them. Four subjects described them as "points." For one *S*, *J*, the specific characteristic of "protruding points" was the sole criterion for judgment. Forms were liked with "many slender" points. Her order of preference was No. 14, 16, 15, 11, etc.

A few *Ss* mentioned line characteristics as important considerations.

G stated that there must be a "harmony of lines." On further questioning this turned out to be close to "opposing lines in the shape being parallel or equal." This was similar to the criterion of No. 12 who liked forms with strongly present "parallel lines." No. 9 liked "even lines," or long unbroken lines bounding the form. No. 19 preferred long "diagonal" lines in the forms.

For no *S* was this interest in lines the sole criteria of preference.

(b) Whole-form characteristics. The majority of the *Ss* mentioned as important considerations the appearance of the whole form, its structure, its complexity or simplicity or other characteristics.

(c) Symmetry. Symmetry of one type or another was mentioned by 16 *Ss*, or about a third of the group, as a reason for preference.

(d) Complexity. Complexity was given by 14 *Ss* (28%) as a reason for preference. Contrariwise, 7 *Ss* stated that the simplicity was a desirable characteristic. As one *S* mentioned both as leading to preference, a total of 20 *Ss* (40%) mentioned one or another of these characteristics.

As with symmetry, various views on what is meant by complexity will be discussed later.

The regularity or irregularity of the figure was a fairly important consideration. Four *Ss* (8%) preferred what they termed "regular" figures, while one *S* preferred those termed "irregular."

Dl, holding to the criterion of irregularity stated that irregular forms were "more alive and interesting" and were preferable to the "static forms" which were regular. This quality she felt came from the parts "shooting out in odd directions." Her first eight forms were interesting from this standpoint while the rest were not "good" as they were "regular and uninteresting." Her order was No. 11, 4, 3, 16, 14, 5, 13, etc.

Of the 4 *Ss* mentioning regularity, *Bd* felt that it was "being the same on all sides." She followed this criterion fairly closely (order: No. 1, 6, 8, and 9). She expressed a dislike for stars and a preference for square-like figures over those with long arms or points. *Cb* felt that regularity consisted in the figure having "an equal number of units on each side of the center, horizontal and vertical" which was the same definition approximately that she gave for symmetry. A regular figure was then one in which the components were arranged around a center in a recognizably clear manner. This arrangement could be complex, in fact she mentioned a preference for complexity and her order among the forms which were regular was from the most complex to the simplest; her order began No. 7, 14, 8, and 15. *Cd* also stated that regularity was equivalent to symmetry and consisted in being "even all around." *Ck* felt that regularity consisted in the figure being divisible into units that were "regular" that is, the common geometrical shapes, squares, rectangles, and triangles.

Apparently closely allied to this interest in definite pattern was the criterion of "organization" which, however, remained unclear, and was in two cases synonymous with "good design."

Cb used the word and stated that it means "the way in which the form is put together." *N* mentioned "organization," and "good design," but could not do more than point out those forms that possessed it. Both their orders began with No. 7, 8, and 14.

Rhythm was mentioned by 2 *S*s and was apparently an almost ineffable quality.

For one *S (Ck),* it constituted the sole criterion. To her it meant something akin to "movement," a property of the whole form, which made it "alive." It apparently had to do with the arrangement of protruding angles and the directions of the lines. Her order began No. 11, 13, 4, and 5 and ended with No. 16, 7, 8. *Q* mentioned it as important but could not define it. Her order began No. 16, 12, 2, 11. She thus differed from *Ck.*

Definite pattern or shape was the criterion advanced by 8 *S*s and formed the sole judgmental basis of a ninth. In most cases this depended on the analysis of the form into elements and concerned their pattern of assembly. In a few cases it seemed to refer to the fact that the form could be seen as built upon a fundamental background or net-work of lines, either horizontal-vertical or diagonal criss-crossing.

C stated that this meant a "definite pattern of arrangement." The basis on which the form was built must be clear, that is, the constituent elements must have a "regular" or orderly manner of assembly. Her order began: No. 8, 7, 14, and 4. *D*'s view was similar to this, as was that of *H* who stated that the "parts (must be) fairly evenly distributed in a definite relation to one another." *N* expressed a dislike for forms that were "a bunch of lines drawn any which way, no purpose in them" and demanded forms with a "definite pattern" which she explained consisted in a form having a "unity of lines, all parallel to each other." *Bg* also visualized the form's constituents, "little elements." The form was liked when the manner in which it was put together was "clear." *Dh* stated that a form must have a "plan" on which it was built. *Cj* stated that there must be some "reason" to the manner in which the form was made.

The general appearance of the form, its structure and the relations of its parts were thus important considerations in the judgments of the *S*s. In all 41 *S*s (82%) mentioned "whole" factors and 10 *S*s (20%) mentioned "part" factors. The complexity of the shape and its symmetry were the two most frequently mentioned characteristics leading to favorable judgment; complexity was mentioned by 14 *S*s and symmetry by 16.

The criteria advanced by the 8 *S*s whose arrays we have distinguished as being quite different from the consensus order of the group shows that six of them did not advance different formal bases than the majority did, and that none of the irregularities are to be explained on the basis of associations. From their statements one would not conjecture that their orders would differ from those of the other *S*s giving the same reasons. Either from a misuse of the criteria or from an unusual manner of seeing a criterion exemplified in the forms, they gave, however, the same reasons as many other *S*s and yet ordered the forms differently.

The 2 *S*s *(Ck* and *Dl)* whose criteria as well as preference orders did differ held respectively to criteria of 'rhythm,' *(Ck)* and irregularity and simplicity *(Dl).*

Connotative criteria. We classify as "connotative" those considerations which were given as bases for preference and which were not concerned with the specific characteristics of a form but arose from the *S*'s previous experience with the form and her recognition of this. We distinguish here two types: specific associations and a feeling of familiarity or unfamiliarity.

(a) Specific associations. For some *Ss* associations did not play a rôle while for others it was a decisive factor. Table IV gives the frequency with which the *Ss* indicated the presence and influence of associations of this type.

Half of the *Ss*, therefore, reported, after they had arranged the forms and given their criteria, that the forms had not suggested anything to them or had caused them to recall an experience. About one-fifth, *i.e.* 11 *Ss* had had some associations but stated that they did not believe these were important in their judgments. One of these *Ss* stated that every one of the shapes had aroused some association but that none of these were important in judgment.

Of the 13 *Ss* in whose judgments the associations which the forms aroused were definitely influential in judgment they were the decisive factor for one *S*, for 6 they were joint factors or of secondary importance, and for 6 they were of small importance but nevertheless present.

TABLE IV

FREQUENCY OF OCCURRENCE OF ASSOCIATIONS AND INFLUENCE ON JUDGMENTS

	Ss	
	No.	%
(A) Associations not determining judgment		
(1) No association at all	24	48
(2) Association after judging	2	4
(3) Associations present but not considered	11	22
	37	74
(B) Associations influencing judgment	13	26
Total	50	100

Bf stated that her preferences were determined throughout by the object which the form suggested. If the reference was pleasant, the form was liked. Forms which had no associations or to unpleasant things were disliked. Her order was close to the group order: No. 4, 8, 15, 7, etc. The stars were preferred because "stars" were "pleasant" and "beautiful;" form No. 8 resembled a "jewel-box;" No. 7 a cut gem, and so forth.

For 6 *Ss* specific associations played a rôle together with some other factor, such as an interest in formal characteristics.

J divided the forms into two groups: those having "good" associations and which were symmetrical, and those having "bad" associations and which were unsymmetrical or irregular; the former were preferred. The remaining forms ranging in between these groups were "indifferent." Four forms were in the first group: No. 3 a crown; 14 pin-wheel; 7 snowflake. In the disliked series of five forms a "bad" association was the "Red Cross" to form No. 8.

L associations of "evening star" No. 14; gem 9, stove 16, and a box 10 led to these being liked; her other criteria were presence of angles and symmetry. There were no associations with forms at the disliked end.

Bk stated that all the forms had "meaning" to her, which arose immediately. The more meaning they had, the more they were associated with things and objects she remembered, the more they tended to be liked; those at the disliked end of the order had "less" meaning. She estimated such associations as forming one-half the basis of her judgment; the other half was "good design." *Cd* had unpleasant associations with forms which reminded her of geometry and physics courses and such forms she placed at the disliked end. *Ck* had pleasant associations with the first six forms in her order: No. 14 and 16 stars; 12 an open book; 10 a box; 3 a bowl, and 16 a peaked hat. She stated that these associations were important in determining her preferences. *Db* said that probably one basis for judgment was the suggestions of Indian designs. Her other criterion was formal, a liking for "ornate and definite shaped figures."

Associations were minor factors in the judgments of 6 other *S*s. We find here that they specifically influenced the judgment of only one shape or were of a "general" influence for 5 *S*s which was quite similar to the general feeling of familiarity.

P had only one association, to No. 7, which suggested a diamond ring which was quite pleasant since she had become engaged shortly before the experiment. This was placed first in the series; her other interests were formal.

For 5 *S*s *(U, V, Bd, Cg,* and *Dk),* a fairly general association was evoked by a number of forms and determined their rank as a group. For *U, V,* and *Bd,* geometry in high school caused a number of shapes to be disliked. *Cg* was favorably reminded of some designs she had once made, and *Dk* had a generalized recollection of paper-cutting in grammar school which was a pleasant recollection and led to the preference for four or five forms. Shape was determinative, however, and she expressed formal interest in "points" and symmetry.

Of the 8 *S*s with a background of art, 3 were influenced by associations—relatively the same number as for the non-art group. As we mentioned above, none of these 8 *S*s with irregular arrays reported that associations were decisive or even important.

(b) Familiarity. We have classified as 'specific associations' those specific and definite recollections evoked by the forms. When *S* was unable to recall particular experiences, when the referent of the association was not clear, or the form did not actually refer to an object but the subject states that she had seen the form before and was familiar with it, we classify these statements under the heading of 'familiarity'—as a separate type of connotative factor.

Fourteen *S*s (28%) explicitly stated that the familiarity or unfamiliarity of the forms were important; 11 *S*s liked forms because they were "unusual," "different," or "new to me," while 3 liked those which had been "seen before," or were familiar.

Among the 11 *S*s who were influenced by the novelty of the shapes, only one, T, stated that novelty was the chief criterion. This *S* arranged the forms in the order of their "unusualness," with no formal interests of importance. For the other *S*s, this was a secondary factor.

In the judgments of the 3 *S*s (*X, Bh,* and *Cg*) who liked familiar shapes, this was a secondary factor and operated in conjunction with formal interests.

Design potentiality. One of the reasons advanced by some of the *S*s for preferences was the possibility of using the various shapes for patterns or designs. These were usually visualized as larger in size and forming an all-over pattern with other identical forms. The degree to which this pattern was attractive and the extent to which the form lent itself to such designing, determined the degree of preference.

No such considerations were present in the remarks of 31 *S*s (63%). Some interest was indicated by 11 *Ss* but this did not, according to them influence their judgments, and remained as a general consideration.

Eight *Ss* (16%) mentioned this factor as an important consideration in their judgments.

D saw the forms as tiles and imagined them in an all-over tile pattern. *G* stated that she "expanded the forms and considered the result as a pattern." *Q* saw them in material as all-over patterns on a dress or textiles; *Y* as the basis for quilt design. *Bd* did not mention the application of the imaginary design. *Cf* saw each form as an element in a larger design, much larger and greatly multiplied, and the relations which each form bore to its identical neighbors determined its preference; a good pattern led to the form being placed high in the order.

TABLE V

Rank Order of Specimens Judged for Complexity, Symmetry, and Familiarity with Median Rank and Deviation

Rank order	Complexity			Symmetry				Familiarity		
	Spec.	Med. rank	S.D.	Spec.	Med. rank	S.D.	Freq.*	Spec.	Med. rank	S.D.
1	7	1.1	.7	6	1.1	3.6	11	6	1.1	1.6
2	8	1.9	2.0	8	2.6	2.8	12	15	4.0	2.3
3	14	2.7	1.8	14	2.4	3.6	11	10	3.6	2.9
4	15	3.8	2.1	1	4.6	2.2	11	1	4.0	1.9
5	16	4.9	2.0	9	4.7	2.5	10	2	4.0	2.5
6	3	5.2	2.4	7	4.7	4.1	10	14	4.4	2.6
7	11	5.8	2.3	15	5.5	3.4	11	9	5.3	1.9
8	4	7.3	2.2	2	7.4	3.7	6	12	8.5	2.8
9	13	8.8	1.7	10	8.6	3.2	4	8	8.5	3.0
10	5	9.3	3.3	12	9.0	2.6	8	4	10.8	2.6
11	12	9.9	2.0	16	10.3	3.5	6	7	11.0	2.5
12	1	11.7	1.7	4	10.8	3.1	4	16	11.9	2.8
13	10	12.9	1.5	3	12.0	2.8	7	3	12.3	2.0
14	9	13.00	1.2	11	13.2	2.8	1	13	12.8	2.3
15	2	14.1	1.1	13	13.7	1.6	0	11	13.6	2.3
16	6	15.5	.3	5	15.4	1.1	0	5	15.2	2.6

* Frequency with which form was adjudged symmetrical by *Ss* not ranking items but grouping them.

Despite the importance of this consideration in the judgments of these *Ss*, it did not form the sole criterion for any. This interest did not lead to any preference orders departing from the group array. Of the group of 8 *Ss* influenced by the design potentiality, 3 were engaged in design work at the time.

Judgments of complexity and its basis. Before summarizing these results, we turn to the results of the symmetry and complexity investigations. When the preference judgments had been completed and the *Ss* asked about their reason for their judgments, they were each asked to arrange the forms in the order of their complexity, and then they were asked to state what their basis for the judgment was.

Table V gives the group order for these judgments according to their median rank value. There was more agreement here than there was in preference.

Examination of the criteria of complexity reveals three major bases, more or less distinct, two dealing with objective characteristics, one with considerations of construction, and an infrequent basis, familiarity. Table VI indicates the frequencies with which they were mentioned. One third of the group mentioned two or more bases.

The character of the form's outline was the more frequently given basis of complexity. Of this group, 19 *Ss* mentioned angles protruding or "points" as the characteristics chiefly responsible for a complex appearance.

Typical statements were: *A*, "cut-out entrances are complex;" *X* "Lots of angles and points make it complicated;" *Cc* "It's the number of angles and the sharper the more complex;" and *Cj* "The more angles, the more complex looking; inward angles are important too."

TABLE VI

Frequency With Which Various Bases of Complexity Were Given

	Mentioning	Sole basis	
	No. Ss	No. Ss	%
(1) Form outline	31	16	32
(2) Construction difficulty	21	8	16
(3) Element analysis	12	6	12
(4) Familiarity	3	1	2
Total giving 1 basis only		31	62
Total giving 2 or more bases		19	38
Total		50	100

Fourteen *Ss* stated that the number of lines making up the outline was the chief consideration. Some of these felt that the sheer quantity of lines was important. Others stated that short lines gave a form the appearance of complexity.

Bd stated that "more lines and a great number of sides" made a form complex appearing. *D* stated that complex forms were those having lines "going in different ways." *Da* held that "little lines, broken up outline, make it that way" (*i.e.* complex appearing).

Eight *Ss* indicated that the sheer number of "parts," lines, or angles, in the outline was the chief factor.

Typical statements were: *Bb*, "many angles and lines;" *Fl*, "great variety of corners, angles and lines."

The second most frequently mentioned basis for complexity was the estimation of the difficulty that would be met in constructing the forms by others or by *S* herself.

For *A* it was "the idea of the trouble taken to make it oneself," and *F* the "considering the effort to make them." *D* ranked the forms on the basis of her estimation of her difficulty in drawing them, the easiest to draw were the simplest. To *Ch* the most complex were those that "needed more care in drawing."

Three *S*s differed from the majority opinions in stating that their basis for complexity lay in the degree of familiarity the form had for them.

U gave as her basis for judgment for the whole series the degree of familiarity; the complex were those which were completely new to her. Her order was close to the groups. *Da* stated that "simple lines and a familiar shape" made for simplicity and a lack of associations or meanings led to complexity. For *Dc* those forms having the "most lines" were complex; those that were unusual and unfamiliar were highly complex.

Fourteen *S*s gave complexity as a criteria for preference. The reasons advanced by this group were representative of those advanced by those not giving this reason, all four types were present. The *S*s with a background of art also gave all types just as the non-art group did.

Another group of *S*s visualized the forms as divided up into constituent elements of different shapes. The behavior of this group was remarkably similar in that the visualized constituents were simple geometrical shapes, squares, rectangles and triangles, and generally the forms were seen as subdivided in much the same manner.

G partitioned form No. 7 into a central square with a little square on each side, each of these being capped with a triangle. No. 8 was seen divided in the same manner without the triangle, of course. No. 7 was ranked higher in complexity since it had a greater variety of elements, triangles in addition to squares. *L* subdivided No. 7 in the same manner as did *G*, but complexity was determined to her by the "number of parts I can see." She subdivided the other forms similarly into the common type of shapes. *Bf* was the more consistent and prolific form analyzer. All were subdivided carefully, and the more elements involved, quantitatively, the more complex or a greater variety. All were reduced to squares and triangles, or other shapes if necessary.

We find, therefore, that there is some disagreement among *S*s in their estimation of the relative complexity of a group of geometrical forms, and that a number of fairly distinct criteria were advanced as the bases of complexity. Allowing for the fact that verbal description is inexact, it seems likely that several different considerations regarding complexity were present, leading to differences in estimates of degree of complexity, and hence to lack of agreement. Moreover, we found that the group mentioned two or more bases as considerations in their ordering. The extent to which agreement would be found among the *S*s holding the *same* criteria is a mater for further experiment. This consideration holds also for judgments of preference, symmetry, and others.

Judgments of symmetry and its basis. The second request of the *S*s was to arrange the forms in the order of their symmetry, or to indicate which ones were symmetrical and which were not. Four *S*s were unable to do this. They either did not know what symmetry meant, or, if they said that they did have some conception, they were unable empirically to indicate its presence or absence. Twelve *S*s grouped the forms into two or three groups: symmetrical, non-symmetrical, and slightly symmetrical. The consensus of opinion is given in Table V.

As we have mentioned above, the aim of this inquiry was to ascertain what *S* considered symmetry to be—or what she meant when she used the term. A number of bases were advanced which, with their frequency of occurrence in this group, are given in Table VII. It was not often that the *S*s would use terms such as "horizontal" or "vertical symmetry" or even axis, but in general their meaning was clear.

The most frequent view held was that symmetry arose from the opposition of "like parts" across an axis. Nineteen *S*s (38%) mentioned this type and stated that the axis must be the vertical one. Opposing "parts" could refer either to parts of the figure, such as arms of stars, or entrant angles.

TABLE VII

FREQUENCY WITH WHICH VARIOUS BASES OF SYMMETRY OCCURRED

	Frequency	
	No. Ss	%
(1) Relation of parts to linear axis		
(a) Opposition of like parts; vertical axis	19	38
(b) Opposition of like parts; vertical or horizontal axes or both	8	16
(c) Opposition of like parts; any axis	4	8
Total	31	62
(2) Relation of parts to center		
(a) Relation of analyzed elements to center	2	4
(b) Distribution of like parts around center (radial symmetry)	2	4
Total	4	8
(3) Similarity of form to square	6	12
(4) Rhythmic coördination of parts	1	2
Total no. of Ss advancing basis	42	84
Total no. judging on "feeling"	4	8
Total no. unable to judge	4	8
Total	50	100

Typical statements are: *C*, "The figure must be the same on both sides, up and down;" *N*, "The opposite parts must be exactly the same across the center line;" *V*, "Same parts on each side, that is, if an angle sticks out on one side, it must on the other too;" *Cd*, "There must be a balancing of parts of the shape;" *Da*, "If the form was divided into two parts down the center, they would be alike."

Eight *S*s holding this view stated that the opposition could be across a horizontal axis also. This view was never given independently but always in connection with the view of symmetry as based on vertical-axis. Several of these *S*s felt that vertical symmetry was a "higher" type of symmetry than the horizontal and some felt that when the two were combined in a form a very high degree of symmetry was achieved.

Among these statements are those of *Bh*, "Balance of parts across either the vertical or horizontal axis but more symmetrical when the sides are alike than when top and bottom;" and *Z*, "Very symmetrical when same in two ways (*i.e.* both horizontal and vertical symmetry) but still symmetrical if just so across the middle line (*i.e.* horizontal)."

Some *S*s felt that the horizontal and vertical axes were not the only possible bases for symmetrical arrangements. Three *S*s stated that other axes were equally "logical," to use the term of *Q*, who stated that symmetry meant the "division of a form along a line, an axis; being equal on both sides. The axis may go in any direction, even various kinds of diagonals."

Y stated that a form was symmetrical when the parts were arranged equally about an axis, generally horizontal or vertical, "but not always necessarily so." *Cj* stated that symmetry meant the opposing of similar parts and like parts" and that the dividing line "follows the natural outline of the figure" (No. 4 to her was symmetrical).

All in all these views were held by 62% of the group. The remaining three types were in a minority.

The dividing-up of the forms was present in the consideration of complexity and 2 *S*s who held to this view of complexity mentioned it also as the basis for a form's symmetry.

Bk stated that the highest degree of symmetry arose from forms that could be subdivided into equal parts that were related together in a definite way around a point; as she said, "the balance of parts around a center, not radiating, but same parts across the center." She listed forms No. 6, 8, 9, 2, 1, 7, 4, 12, and 10 as possessing high degree of symmetry, in that order, since they possessed "symmetry of line, equal parts, and especially were divisible." (It will be noticed that several of these are not radially organized and her basis is actually divisibility along an axis.) Forms 13, 14, 15, and 3 had less symmetry but "symmetry of outline" and were not divisible. *Bf*, whose views on complexity we have given above, felt symmetry to be a type of radial arrangement of parts about a center. She divided each figure into components; if the form was composed of similar parts it was symmetrical. She also considered the points at the protruding angles: all of them must be the same distance from the center. Those forms with vertical or horizontal symmetry were the second group.

Two *S*s held to a radial view of symmetry without analysis of the forms into elements.

J said that she "felt lines running from the center outward. If the figure were turned it would be the same." *V* stated that the basis of symmetry was "all the angles (being) equal on the same cross lines." She felt lines through the center to the protruding angles and a symmetrical form was one which had equal angles opposite in this manner.

Six *S*s felt symmetry to be a product not of opposition of similar parts with reference to an axis, but of the similarity of the figure to a square shape. If the figure could fit neatly into a square outline it was symmetrical. There was also a feeling that simpler forms of square-shape were more symmetrical than more complex ones.

P expressed this clearly; her basis for judgment was "even sides, matching in length" and an explicit statement of the consideration of the extent to which the form was similar to a square: four equal and similar sides, and four corners. *Bg* said that forms that "approached to a square shape were the most symmetrical." And *Db* stated that "plain four-sided ones" were more symmetrical.

One subject, *Ca*, advanced the unique view that symmetry arose from the "rhythm" of the form, the "coördination" shown in it. She could not explain her views more closely; her order of symmetry began with forms 11, 16, and 4. Four *Ss* could not give objective bases for symmetry but stated that they "just went by the feeling" or the "appearance" of symmetry in the form.

Cl said that it was "just the way in which places and points are arranged." *W* had "no specific theory, just a feeling for it." I went by the "appearance only," and *B* stated that symmetry was "not necessarily bound to an axis" and that she "just went by a feeling for it."

Every *S*, with the exception of two, held to only one view of symmetry. These two added other elements to one or another of the types explained above. *C*, holding to a view of vertical symmetry, added that a "feeling of weight, larger at the bottom and smaller at the top" was important. This is equivalent to 'balance;' her form order does not show that this was followed in her ranking. *Cg* added the factor of simplicity, recognizing the vertical as the basis of arrangement. Among the forms that she arranged on the vertical symmetry basis, she awarded highest positions to the simpler forms: No. 6, the square, was first, followed by 2, 10, and 1.

It is apparent that symmetry meant a number of different things to the individuals in this group, undoubtedly one reason for the only fair degree of agreement in their estimates. The major factor was the axial basis of construction with the vertical axis most important. A radial type in which the analyzed parts of the form were seen as distributed around the center point was apparently a minor consideration. The third type, classified on the basis of statements that square-like forms were symmetrical, might be another way of expressing this view except for the fact that simpler forms were generally held by this group to be the more symmetrical, which is not the case with the majority of *Ss*, as can be seen by the group order, where No. 7, 8, and 14, high in the complexity order, were adjudged more symmetrical than simpler forms on the same basis.

Estimates of familiarity. The *Ss* were requested to arrange the forms in the order of their familiarity with them. This order is given in Table V and reveals a fair degree of agreement in judgment.

CONCLUSIONS

(1) The reasons advanced by a group of *Ss* as their bases for preference judgments of a group of 16 geometrical forms revealed three main interests: the formal characteristics of the forms, the connotative or associative meanings, and their potentialities for designs.

(2) One or another formal criteria, those concerned with the structure and appearance of the form, were given by 90% of the group. The aspect of the whole form was much more important than the nature or presence of different parts.

(3) Connotative factors were of two types: specific associations which the forms evoked, and generalized recognitions of the familiarity of the forms, or their novelty. Specific associations were important in determining the judgments of approximately a quarter (26%) of the *S*s. Familiarity was important for 3 *S*s (6%) and unfamiliarity for 11 (22%).

(4) Design-potentiality was influential in the judgments of 8 *S*s (16%). This interest lay in the possibilities of the forms for use in textiles or other all-over pattern uses.

(5) Approximately three-quarters of the group (78%) gave two or more criteria as their bases for judgments. Formal criteria of various types were the most frequent and most important. Connotative factors, specific associations, or recognition of familiarity or unfamiliarity determined some judgments in addition to formal criteria in the orders of one-third (34%) of the *S*s.

(6) Analysis of the judgments and bases advanced by the *S*s for complexity revealed a number of dissimilar views. Two-thirds of the group felt it to consist in the type of outline and a multiplicity of angles or lines; difficulty of construction was mentioned by 42%; the imaginary divisibility of the form into elementary components was given by 24%; familiarity was mentioned by one *S*.

(7) A number of different bases were given for symmetry. Two-thirds defined it as being the opposition of similar parts across an axis which was usually vertical but for some could be horizontal or oblique. Some arranged the forms on the basis of a rotational symmetry, and several infrequent bases were given.

(8) A correlation of only 0.21 was found between the 'esthetic measure' of the forms and the average preference of the group. Birkhoff's formula gives high esthetic value to simple forms whereas it was the complex ones that were generally preferred by our group.

THE RÔLE OF THE AUDITORY AREA OF THE CORTEX

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The ability to localize laterally produced sounds is a function of some differential produced at the peripheral acoustic apparatus. These left-right orientations normally require the use of both ears, and depend upon a difference in the properties of the stimulus impinging upon the cochleas: intensity, phase, or time differences. It has been shown that the cortical participant in this behavior is the auditory cortex of the left, or the right cerebral hemisphere.[1] This 'safety' factor is such that in the dog the L-R discrimination can be continued uninterrupted or initially acquired after hemidecortication.

Similar findings had been previously reported in the white rat by Pennington.[2] From his data, he concluded that the auditory cortex was equipotential, since errors in the L-R discrimination increased in proportion to the amount of cortex extirpated. For stimuli Pennington had used buzzers, which introduced an experimental artifact invalidating his interpretation. That is, the percentage of errors would have also increased in a rat with *intact* cortex if the complexity of the sound source had been reduced from gross noise to a simple sine wave such as a pure tone.[3] This is supported by the fact that the L-R discrimination has not been procured in the *normal* white rat for pure tones.

If the auditory cortex is 'equipotential' then, whatever interpretation holds for the action of the peripheral acoustic apparatus, a frequency theory of hearing is the only satisfactory description of the function of this mechanism. That is, pitch would be determined by the volleys of impulses set up anywhere within the critical limits of the cortical auditory area. On the other hand, if specific parts of the auditory area mediate particular pitches, then some form of a place theory obtains at the cortical level, as it has been ascribed to the more peripheral acoustic mechanisms. These

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[1] E. Girden, Cerebral determinants of auditory localization, this JOURNAL, 52, 1939, 1-15.

[2] L. A. Pennington, The function of the brain in auditory localization: IV. Method of training and control experiments, *J. Comp. Psychol.*, 25, 1938, 195-211.

[3] Girden, *op. cit.*, 14.

implications of Pennington's study have until now been overlooked both by him and other workers in the field.

A critical test of this question was initiated by determining the effect of bilateral extirpation of the auditory area upon (1) the L-R habit for pure tones as well as bells, and (2) auditory acuity for the octave frequencies from 256 ∼ to 8192 ∼. If hearing is impaired more on some frequencies than on others, or the L-R habit is disrupted for some pitches and not others, evidence would be had for a place theory. The opposite results would be in support of some form of a frequency hypothesis.

A description of the method by which the L-R habit was established is already available.[4] Briefly, the dog was conditioned to flex right forepaw to a pure tone or a bell sounded from its right side, but to inhibit this response whenever the stimulus was presented from its left side. The intensity of the bell was fixed, and was much louder than any pure tones capable of being produced by the audiometer. The pure tones were just loud enough to permit the animal to make immediate reactions upon their presentation. Auditory acuity at the respective octave pitches was procured by a standardized procedure.[5] The cortical operations were performed aseptically under Nembutal anesthesia.

RESULTS

Protocols of the experiments on Dogs 7, 8, 9, and 10 are as follows.

Dog 7. Restricted portions of the sylvian gyri, areas 78 and 78b,[6] were extirpated in a single operation on 7/13. The first post-operative retention-test was made 48 hr. later (Table I). The L-R habit for the bells was present, but could not be evoked for the pure tones with the loudest sounds available. Unconditioned L-R head orientations were evoked by the tonal stimuli. On the second test (the fifth day after the operation), the L-R habit reappeared on all test frequencies. The intensities, however, had to be increased 10 to 15 db. over the pre-operative magnitudes. A similar process of post-operative inhibition and spontaneous recovery within five days has been reported by Marquis and Hilgard in the study of the conditioned response (CR) of the eyelid to light after the removal of the occipital lobes.[7]

A reëntry on the right side was made on 7/20, and a larger section of the sylvian gyri and some adjoining posterior ectosylvian cortex (area 68) were extirpated. On the first post-operative test the results were similar to those found after the first operation. Localization for the pure tones was suppressed whereas normal function for the bell was present. Again spontaneous recovery for all frequencies occurred on the second test (7/25), five days after this operation. Likewise the intensity of the pure tones had to be increased over the normal values; this time, 10 to 30 db.

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[4] Girden, Conditioning and problem solving behavior, this Journal, 51, 1938, 677-687.

[5] E. A. Culler, G. Finch, E. Girden, and W. J. Brodgen, Measurements of acuity by the conditioned-response technique, *J. Gen. Psychol.*, 12, 1935, 223-227.

[6] Cf. J. W. Papez (*Comparative Neurology*, 1929, 1-518) for the procedure used here in identifying the cortical areas.

[7] D. G. Marquis and E. R. Hilgard, Conditioned lid responses in dogs after removal of the visual cortex, *J. Comp. Psychol.*, 22, 1936, 157-178.

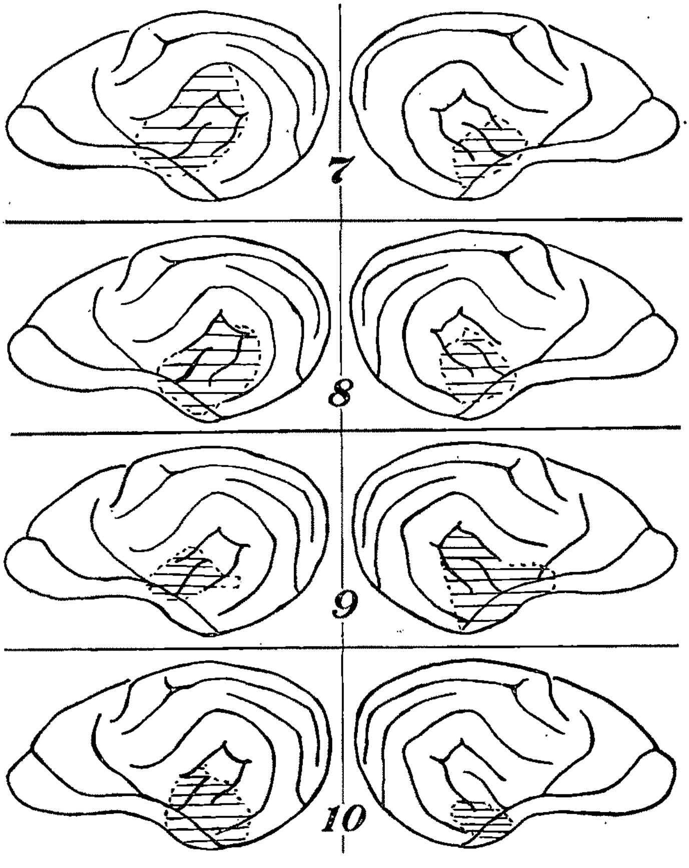

FIG. 1. THE CORTICAL EXTIRPATIONS FROM DOGS 7, 8, 9, AND 10
The extirpations are indicated by the lined areas. Single bilateral operations were performed on Dogs 8 and 9. Two reëntries were made on Dog 7 and one on Dog 10. The L-R discrimination was temporarily depressed in Dogs 7 and 10 and permanently abolished in Dogs 8 and 9.

On 8/3, a third and final operation was performed, consisting of a reëntry on the left side. The remainder of the sylvian gyri and some of the anterior ectosylvian gyrus (area 38b), anterior section of the middle ectosylvian gyrus (area 68b) were extirpated. The total amount of cortex removed in all three operations is shown in Fig. 1. Note that the dorsal cortex of the sylvian gyri on the right side had not been removed. The first test after the third operation was completely negative for all stimuli. Again, unconditioned head orientations were noted. The second test was made on the tenth post-operative day, and the L-R habit had returned completely for all stimuli. Again the intensity of the pure tones had to be maintained at a louder level: 15 to 25 db. These results are summarized in Table I. It should be noticed that the increase in the intensity of the pure tones is not the same for all pitches. The longer delay between the first and second tests after the third entry was partly due to the effects of the operation. Motor incoördinations, including that of the right forepaw, were present for a number of days. The phenomenon of 'psychic blindness' was also noted. That is, pupillary reaction to light could be evoked, but the animal would walk blindly into objects, food pans, walls of the cage, and the like. Similar results in Rhesus monkeys have been reported by Klüver and Bucy.[8]

Dog 8. A bilateral operation was made on 7/27, as shown in Fig. 1. Ten days after the operation the first test showed that the L-R habit was completely abolished for all stimuli, pure tones and buzzers alike. Unconditioned L-R head orientations to the sounds were procured.

TABLE I

RESULTS OF DOGS 7 AND 10

N indicates normal function; the figures show the decibel increase necessary to evoke the L-R habit; the dashes mean complete loss of function.

		Stimuli							
		256~	512~	1024~	2048~	4096~	8192~	9747~	Bell
	(Entry: 7/13)								
	7/15	—	—	—	—	—	—	—	N
	7/18	15	10	15	N	15	15	N	N
	(Entry: 7/20)								
Dog 7	7/23	—	—	—	—	—	—	—	N
	7/25	20	30	10	10	20	15	N	N
	(Entry: 8/ 3)								
	8/ 5	—	—	—	—	—	—	—	—
	8/13	15	15	20	20	15	25	N	N
	(Entry: 7/13)								
Dog 10	7/18	5	10	5	15	15	5	5	N
	(Entry: 7/20)								
	7/23	15	10	30	15	15	10	N	N

Dog 9. The bilateral extirpation of auditory cortex was made on 7/28, as indicated in Fig. 1. A post-operative test on 8/4 was negative for all stimuli, bells and pure tones. Again, the presence of auditory function was indicated by head orientations, but the L-R habit, as in the last animal, was completely abolished.

Dog 10. Some of the posterior ectosylvian (area 68) and the ventral portions of the posterior sylvian gyrus (area 78) on both side were extirpated 7/13. Five days after the operation the L-R habit was unimpaired, although the intensity of the pure tones had to be increased 5 to 15 db. No response was procured on 128 ~.

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[8] H. Klüver and P. C. Bucy, "Psychic blindness" and other symptoms following bilateral temporal lobectomy in Rhesus monkeys, *Amer. J. Physiol.*, 119, 1937, 352-353.

A second entry on the left side was made 7/20. The lower portions of the sylvian gyri as well as some adjoining posterior ectosylvian and suprasylvian (area 70) cortex were extirpated. The final situation is indicated in Fig. 1. The first test after this entry was made on the third day, and the L-R habit was unimpaired. The intensity of the pure tones, as in the case of Dog 7, had to be increased (cf. Table I).

Of these four animals, the L-R habit functioned normally in Dogs 7 and 10, whereas it was completely abolished in Dogs 8 and 9. It was impossible to disrupt the discrimination for specific pitches in either Dogs 7 or 10. It is true that, in these two animals, the intensity of the pure tones had to be temporarily increased. More important, these changes were not equal for all pitches. The significance of this will become clear after the data of the next two animals are discussed below.

Failure to disrupt the L-R discrimination for particular pitches suggested the following possibilities. (1) A place theory is inapplicable to cortical auditory functions; hence no 'scotoma' could be produced, whereby the L-R habit would be lost for specific tones. (2) The cortical auditory area does function according to a place theory, but the experimental technique was unsatisfactory. That is, since the projection system from the medial geniculate bodies to the cortex is unknown in the dog, there is no apriori evidence to indicate where one might expect the radiations to terminate. Under these circumstances there might be some impairment in acuity but not the complete loss of hearing, if only a section of the area mediating a particular pitch is excised. There were other possible explanations, but in any case it seemed hopeless to continue along the present lines. Clearly, tests of auditory acuity throughout the pitch-range were required. Accordingly, in the next two animals, the thresholds for the octave frequencies from 256 ~ to 8192 ~ were approximated after the L-R habit had been established.

Protocols of the experiments with Dogs 13 and 14 are given here.

Dog 13. The bilateral ablation of the posterior sylvian gyrus was performed on 3/16, as represented in Fig. 2. Testing was initiated 48 hr. later. As can be seen in Table II, hearing for 256 ~ was completely depressed for the first two tests. The other pitches were present from the beginning, but with severe losses, varying in magnitude from pitch to pitch. As the tests continued these losses were gradually eliminated, and by the final test (eighth post-operative day), the acuity had returned practically to normal. There were some residual losses on 512 ~ and 1024 ~ which, by statistical tests, were greater than could be expected by chance.

Dog 14. The cortex removed bilaterally on 3/25 is indicated in Fig. 2. All hearing, including the L-R habit, was completely depressed. This continued for four

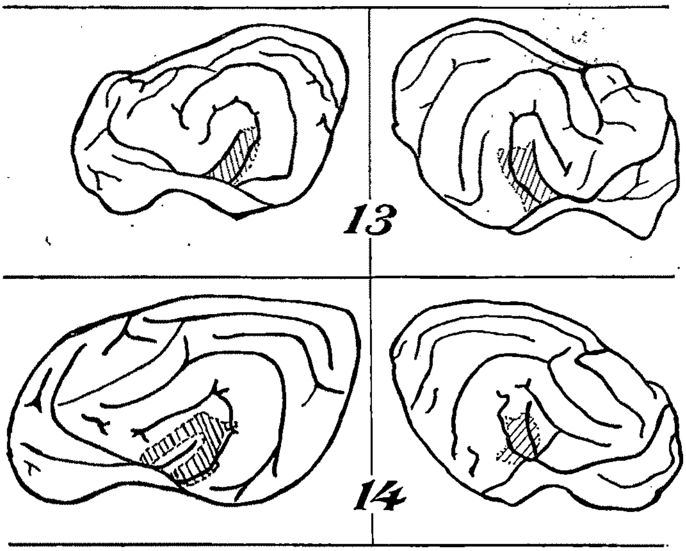

FIG. 2. THE CORTICAL EXTIRPATIONS FROM DOGS 13 AND 14

The extirpations are indicated by the lined areas. They were made bilaterally in a single operation in both animals. Large initial losses in hearing occurred which were followed by spontaneous recovery. Residual losses of 6 to 8 db. occurred in Dog 13 with 256 ~ and 1024 ~.

TABLE II

RESULTS OF DOG 13

The preoperative limen was set at 0. This does not mean that the thresholds at the different pitches are the same, but merely that all changes are measured from the normal level. The loss in hearing is, therefore, shown directly by its positive distance from zero. A minus value indicates a gain in hearing, *i.e.* it is better than the mean of the preoperative tests. The critical ratios **indicate** that the losses must be considered as real.

	Stimuli					
	256 ~	512 ~	1024 ~	2048 ~	4096 ~	8192 ~
Limen	0.0	0.0	0.0	0.0	0.0	0.0
σ_M	1.86	1.67	1.32	1.01	1.67	2.42
n	7	6	7	7	7	7
Mar. 18		38.7	66.9	60.9	73.1	72.7
Mar. 19		51.7	36.4	20.4	22.1	20.7
Mar. 21	−6.1	4.2	11.4	20.4	34.6	13.2
Mar. 22	3.9	9.2	11.4	12.9	19.6	8.2
Mar. 23	6.4	11.7	6.4	5.4	12.1	10.7
Mar. 24	1.4	9.2	6.4	0.4	−0.4	0.7
Loss	1.4	8.6	6.4			
σ_M	2.72	1.58	0.0			
n	4	4	2			
Critical ratio	0.47	3.73	4.84			

successive tests (3/27-30 inclusive). Head orientations were present on the very first test. On the second test the "attentive body-set," which well-trained animals possess, reappeared. On the fourth test "some incipient CR behavior" was observed and that "spontaneous recovery of the function may occur."

On 4/2 (eighth post-operative day) the fifth test was made, and the L-R habit now appeared in full strength for all frequencies. There still was considerable attenuation in hearing on 2048 ~ anl 4096 ~. Unlike the previous animal, Dog 13, normal hearing returns to Dog 14 at all frequencies (cf. Table III). When all five post-operative tests are averaged, a real loss is statistically indicated on only one pitch, *i.e.* to 4096 ~. This, however, does not take into account the progressive recovery of function. By the last test (April 11), the acuity returns to the normal level on all pitches.

The results procured with these two animals support the results obtained from Dogs 7 and 8 as well as the previously reported findings.[9] Unless the critical auditory areas are removed completely from both hemispheres, permanent disruption of the L-R habit does not occur at any pitch.

TABLE III

RESULTS OF DOG 14

	Stimuli					
	256 ~	512 ~	1024 ~	2048 ~	4096 ~	8192 ~
Limen*	0.0	0.0	0.0	0.0	0.0	0.0
σ_M	0.90	1.50	1.50	1.88	1.38	1.15
n	8	9	9	9	9	9
Apr. 2	2.2	−8.3	−11.9	22.2	16.7	2.8
Apr. 3			8.1	12.2	9.2	2.8
Apr. 4	−5.3	−3.3	0.6	4.7	9.2	−2.2
Apr. 6	−0.3	4.2	5.6	4.7	4.2	2.8
Apr. 11	2.2	−0.8	−6.9	−12.8	−0.8	0.3
Loss	−0.3	−2.05	− 0.9	6.2	7.7	1.3
σ_M	1.77	2.61	2.98	3.21	2.31	0.79
n	4	4	5	5	5	5
Critical ratio	0.18	0.68	0.27	1.77	2.86	.93

* For an explanation see Table II.

More significant is the marked temporary impairment of acuity that occurs in both of the last animals. As shown in Fig. 2, the cortical areas extirpated were not identical in those two cases. Nor are the curves of attenuation in hearing the same for both specimens (cf. Tables II and III). In Dog 13, the severest losses first occur on the highest pitches with somewhat decreasing magnitude to the lower end of the pitch-range (excepting 256 ~, which was completely abolished for the first two tests). In Dog 14 the greatest losses occur on 2048 ~ and 4096 ~, with decreasing impairment at both ends of the pitch range. These results are supported by those obtained from Dogs 7 and 10. The fact that the intensity of the pure tones had to be increased post-operatively in these two animals indicated an attenuation in hearing. Likewise the amount of impairment was not the same for all pitches (cf. Table I).

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[9] Cf. Girden, *op. cit.*, this JOURNAL, 52, 1939, 1-15.

These initial severe hearing-losses, however, are followed by a spontaneous recovery of function which cannot be attributed to retraining. Little shock-reënforcement was used, and then only for the purpose of motivation. The sudden reappearance of the L-R habit for all pure tones also minimizes this possibility. The establishment of this discrimination, particularly for pure tones, requires periods of consistent shock-reënforcement training which usually last for many weeks. The longer period of depression in Dog 14 is at present inexplicable. It is unfortunate no tests were made in Dog 14 from the fifth to eighth post-operative days. Undoubtedly similar large differentia in acuity would have been noted as in Dog 13, because there is, on the first positive test (the fifth test), a difference of 34 db. between 1024 ~ and 2048 ~.

DISCUSSION

Without presenting a minute description of the neuroanatomy, many details of which are still unknown, the following facts are relevant. The acoustic nerve, after its exit from the cochlea, divides into crossed and uncrossed components, such that each medial geniculate body receives impulses from both cochleas. That is, the left acoustic nerve contributes to both the left and right medial geniculate bodies. The fibers of the right acoustic nerve distribute in a similar manner. An important consideration for the understanding of the physiological basis of auditory localization is the work of Kemp and Robinson. They have shown that impulses originating in both ears (cochleas) pass up both the crossed and uncrossed auditory tracts at the same time. Another way of stating this fact is as follows. In the left lateral lemniscus (containing uncrossed tracts from the left ear and crossed tracts from the right ear), impulses from both cochleas go towards the higher nerve centers simultaneously.[10] No crossed pathways occur at the thalamus; all radiations from the left geniculate body project to the auditory cortex of the left hemisphere, and the fibers from the right geniculate body terminate on the cortex of the right hemisphere.[11]

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[10] E. H. Kemp and E. H. Robinson, Electrical responses of the brain stem to bilateral auditory stimulation, *Amer. J. Physiol.*, 120, 1937, 316-322.

[11] F. Bremer and R. S. Dow, The cerebral acoustic area of the cat, *J. Neurophysiol.*, 2, 1939, 308-319.

Since each geniculate body receives impulses from both cochleas, each cortical area thus receives impulses from both ears. The corpus callosum is presumably the only means of direct communication between the two cortical auditory areas.

There is considerable evidence to show that, following this anatomic organization, there is a comparable functional arrangement such that hearing is bilaterally represented.[12] This organization is more than the simple participation of both cerebral cortices. Since the L-R habit is maintained in the hemidecorticated animal, there must be complete representation of both cochleas on each cortex.[13] There is every indication that the cortical acoustic mechanism involved in the L-R auditory localization habit is not indiscriminately spread over the cortex as a whole, as maintained by some authorities.[14] To the contrary, it is probable that the primary auditory functions are mediated by a proscribed cortical area.

The auditory cortex seems to play a passive rôle in L-R behavior, the real determinant having a peripheral locus. That is, the differentiating factor is some constituent component of the stimulus impinging upon the ears: phase, intensity, and time differences at the two ears. How are they represented physiologically? Phase and time differences can be considered identical in action.[15] This would mean that physically the sound reaches one ear before the other is stimulated, and as a consequence, physiologically, impulses would be initiated in one acoustic nerve before a comparable volley would be aroused in the opposite tract.

The third important factor, intensity, can likewise be shown to be a case of physiological time differences. Suppose the sound reaches both ears in exactly the same phase and at the same moment, differing only in being more intense at one ear (say, the right) than at the other ear (the left). This will arouse impulses (a) more quickly, *i.e.* they will have a shorter latency, and (b) a greater number of them will be in the right acoustic nerve than in heterolateral (left) tract. The change in latency appears to be effective only up to intensities of 30 db. above the threshhold.[16] If, in a particular case, only the second factor is operative, this will again be translated in terms of physiological time-differences at the second neuron. That is, the greater burst of impulses in the first neuron from the cochlea of the right ear will activate the second neuron before a comparable volley is initiated at the corresponding heterolateral level. In other words, auditory localization is maintained physiologically in the respective auditory tracts by the time differences which have originated at the two cochleas. This relationship will be maintained through the second, third, and fourth neurons to both cerebral cortices, one pathway maintaining a lead over the other to the motor outlet which will produce the L-R orientation.

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[12] Cf. W. J. Brogden, E. Girden, F. A. Mettler, and E. Culler, Acoustic value of the several components of the auditory system in cats, *Amer. J. Physiol.*, 116, 1936, 252-261.

[13] Cf. Girden, *op. cit.*, this JOURNAL, 52, 1939 1-15.

[14] I. P. Pavlov, *Conditioned Reflexes*, 1927, 1-430.

[15] Cf. R. S. Woodworth, *Experimental Psychology*, 1938, 518-538.

[16] S. S. Stevens and H. Davis, *Hearing, its Psychology and Physiology*, 1938, 385.

It has been mistakenly implied that there must be a one to one relationship between the frequency of the stimulus and the rate of discharge in the nerve. Thus Woodworth states, "Unless the impulses coming into the brain center from the two auditory nerves preserved the frequency of the stimulus, they could not preserve the binaural time-difference of the waves in a continuous tone, and this essential cue of direction would be lost."[17] Since the first report of the Wever and Bray phenomena in 1930, there has been much needless discussion concerning this relationship. Their original results indicated a concomitance between the acoustic frequencies and the electric phenomena, up to 4100 ~. This caused considerable concern, since there was an apparent contradiction between these findings and the all-or-none law and the known facts of refractory period.

More recent studies have shown that this effect holds only for the cochlear microphonics. As far as the cerebral acoustic pathways are concerned, "no synchronized impulses at frequencies above 100 per second have been detected from the auditory cortex or auditory radiations."[18] There need be no greater expectancy for a one to one relationship between the frequencies of the acoustic stimuli and the resultant auditory nerve impulses any more than one would expect the frequencies of the visual stimuli to be directly expressed into a numerically equal volley of nerve impulses in the optic nerve. All that would be required is that the *relative differences* be represented, whether the volley of nerve impulses be 5, 10, 50, or 100 per sec.

Since the discrimination persists following transection of the corpus callosum, or hemidecortication, there must be a common motor pathway from both auditory areas. The question that remains unanswered is the origin of and the pathways from the auditory cortex to the motor pathways.

It has been suggested by Culler that the auditory system, like the visual and somesthetic modalities, consists of a projection system such that the anatomic arrangement of the peripheral mechanism is maintained throughout the central nervous system up to and including the cortical terminus.

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[17] Woodworth, *op. cit.*, 537.

[18] Stevens and Davis, *op. cit.*, 402.

Evidence for this view and support for the consequent place theory of hearing has been offered for the cochlear mechanism and the medial geniculate bodies in the thalamus.[19] Detailed analyses of the relevant data and the theoretical implication thereof are available elsewhere.[20]

The present data offer support for this hypothesis. If the auditory cortex were completely equipotential and did not function according to some such principle, it would be difficult to explain the unequal *losses* on the *different* pitches following restricted lesions in the auditory area. Evidently, different parts of the acoustic cortex, in the first instance, function in the perception of different pitches. The differences produced between the two animals (cf. Tables II and III) are perhaps not as marked as those procured on the thalamic level by Ades.[21] Since the present study was a preliminary analysis, the lesions produced here are not as restricted, nor as different in the two animals (cf. Fig. 2). The present results, therefore, support the hypothesis that the primary principles of action of the cortical acoustic system is some form of a place theory, whereby different parts of the auditory area are activated by different pitches.

Certain differences between the present data and the findings reported at the cochlea and the thalamus suggest, however, that the cortical mechanism is more complicated than implied by a simple place theory. In the present experiment there were relatively no permanent impairments in acuity. In any case, they are not comparable to these produced at the thalamus. The initial sharp losses are eliminated within a week or so through a process of spontaneous recovery of function. This occurs, of course, only if some part of the primary auditory area remains intact.[22] Obviously

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[19] Cf. H. W. Ades, F. A. Mettler, and E. A. Culler, Effect of lesions in the medial geniculate bodies upon hearing in the cat, *Amer. J. Physiol.*, 125, 1939, 15-23; S. S. Stevens, H. Davis, and M. H. Lurie, The localization of pitch perception on the basilar membrane, *J. Gen. Psychol.*, 13, 1935, 297-315.

[20] Cf. Stevens and Davis, *op. cit.*, 356-436; Woodworth, *op. cit.*, 501-538; E. G. Weaver, The electrical responses of the ear, *Psychol. Bull.*, 36, 1939, 143-187.

[21] Ades, Mettler, and Culler, *op. cit.*, 15-23.

[22] It is also true that even if all of the auditory cortex is removed bilaterally, the animal does not become permanently deaf. Within two weeks, auditory function does return, but now at a *subcortical* (thalamic) level. As evidenced in the chronic decorticate dog, not only is auditory function present, but an auditory CR and even a tonal discrimination may be established. (Cf. E. Girden, F. A. Mettler, G. Finch, and E. Culler, Conditioned responses in a decorticate dog to acoustic, thermal, and tactile stimulation, *J. Comp. Psychol.*, 21, 1936, 367-385.)

Complete bilateral extirpation of the auditory cortex, however, does *disrupt* the CR as well as the L-R localization habit upon which it is based. While spontaneous recovery of auditory function occurs, after the usual period of post-operative rest, as shown by head-orientations to sound stimuli, the CR is permanently abolished. Retraining, of course, will reëstablish a *new* CR. But this is not the spontaneous recovery that does occur, in the present study, without retraining.

Similar results were present in the totally decorticate specimen. Conditioned a place theory under these circumstances is no longer tenable. This suggests the following hypothesis. At the cortex, unlike the more peripherally located components of the acoustic system, there is in operation another mechanism that for the present is best described as equipotentiality. If the primary projection system is disrupted by localized lesions, normal functions would now be maintained in this emergency by the relative frequency or volleys of nerve impulses in the remaining functional auditory cortex.[23]

This would explain why it was not possible to eliminate the L-R habit for specific frequencies or produce severe permanent losses in hearing. When the primary projection system (place theory) is disturbed by localized lesions, pitch is then maintained by the respective volleys of impulses (frequency theory). Presumably, normal hearing should be maintained so long as some part of the primary acoustic area remains functionally intact.

This concept is not entirely foreign nor contradictory to other known facts of cortical functions.[24] A somewhat analogous situation occurs in the clinical condition of hemianopsia. Total blindness of corresponding halves of the visual field of both eyes should follow total destruction of the primary visual cortex of one hemisphere. Yet objects can be recognized within an area where stimulation, during perimetrical examination, is ineffective. According to these studies of Goldstein, "precise exploration shows that the patients are not limited to half of a field of vision, but that their field of vision is arranged around a center like normal, and that, likewise, the region of their clearest vision lies approximately at this center. . . . Apparently, that point in the outer world seems clearest which is reflected not on the border of the intact retina, where the old macula now lies, but on an area *within* the intact retina."[25] In other words, following disruption of the visual projection system dynamic reorganization takes

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responses established after the third operation were permanently abolished subsequent to the fourth and final operation, which was the last stage in complete decortication. After this last entry, the CR never spontaneously reappeared and only occurred afterwards following subsequent training. (Girden, Mettler, Finch, and Culler, *op. cit.*, 373 f.) In the present study, therefore, the fact that there was spontaneous recovery of auditory function is in itself not indicative of cortical activity. But, simultaneously, and as an integrated part, came the return of the CR and the L-R habit *without retraining*. This does indicate that the pattern, therefore, was still cortical and hence can be attributed to the functioning of the remaining auditory cortex, and not indicative of thalamic activity.

[23] Wever, *op. cit.*, 175 ff.

[24] Cf. K. S. Lashley (The mechanism of vision. XVI. The functioning of small remnants of the visual cortex, *J. Comp. Neur.*, 70, 1939, 45-67) who has recently shown that the discrimination of visual figures in the rat does not depend on a particular part of the striate areas. So long as any section of the geniculostriate system remains intact, even a few cells, appropriate reactions are made.

[25] K. Goldstein. *The Organism*, 1939, 47 f.

place in the remaining optic cortex so that normal functions are maintained. Likewise, in the present case, in the study of a different sense modality hearing, similar dynamic readjustments occur.

SUMMARY

Evidence is offered to show that there is a specific cortical auditory area, which when extirpated will abolish the L-R habit as well as the CRs upon which it is based. Restricted lesions within the limits of this area will only temporarily depress this function, spontaneous recovery occurring within a period of about five days. It is not possible to disrupt the L-R discrimination for specific pitches.

During the period of depression following restricted lesions, auditory acuity is also impaired. Complete recovery of function occurs after a longer period of time; approximately seven to eight days. The severe temporary impairments in acuity differ in the two animals from pitch to pitch.

To explain these facts, it is suggested that a primary projection system of the peripheral acoustic mechanism exists at the cortex, whereby particular pitches are mediated by different areas (place theory). But in emergencies, such as produced by ablation of restricted portions of the auditory area, normal function is maintained by a principle of equipotentiality. That is, the intact auditory cortex is still capable of mediating normal hearing according to the number of nerve impulses produced by the respective tones (frequency of volley theory).

CHANGES IN GENERAL BEHAVIOR DURING FLEXION CONDITIONING AND THEIR IMPORTANCE FOR THE LEARNING PROCESS

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The conditioned response method has often been thought of as the most refined and accurate technique available for the study of learning. It limits its observations to the minute analysis of one simple act. If precise recording devices are employed, it permits a quantitative study of the movements or responses produced to a degree of perfection as yet unapproached by any other method. Yet its greatest virtue is also its most serious deficiency. For by studying intensively the modifications which take place in one organ or part of the body, it has tended to overlook the activity of other parts not directly concerned. It has turned a spotlight upon a portion of the learning process, but has left the remainder largely unexplored. The alterations through which the whole living organism passes must be clearly observed and studied for a thorough understanding of the genesis of learning.

Zener, in a recent article, has attempted to make up for this deficiency by supplying descriptions of the changes in the general behavior of dogs during salivary conditioning.[1] He writes as follows, "The relative inadequacy of available experimental facts to differentiate decisively between the alternative theories of conditioning is due largely to the circumstance that the traditional method reflects the substitute stimulus conception held by the original Russian investigators, and is thus not well designed to reveal facts conflicting with it. Thus with the salivary response, although concomitant occurrence of overt behavior has been reported, the fact that it has not been described in any detail reflects the opinion that it is incidental and irrelevant to the response selected for study."[2]

With regard to the conditioned withdrawal reaction, much the same situation exists although there has been, perhaps, more notice of the "incidental" behavior which accompanies the training than in the case of the salivary reaction. As early as 1916, Watson wrote upon this subject, "The conditioned motor reflex is usually

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[1] K. Zener, The significance of behavior accompanying salivary secretion for theories of the conditioned response, this JOURNAL, 50, 1937, 384-403.

[2] *Ibid.*, 388.

sharp, quick, and widespread, the whole body as a rule being brought into the reaction at first. Gradually the reflex becomes more circumscribed."[3] Beritoff noted barking, howling, groaning, tossing, vocal, and other reactions of his dogs when in the apparatus,[4] while Bechterev has pointed out that "the association reflex is, to a certain extent, more or less general in character when it appears, and only gradually, hand in hand with the fixation of the association reflex, does it become more and more local, until, finally, it is expressed in one defensive movement of the organism stimulated."[5]

Among the later American students of this question, Liddell has observed that "in the early stages of training, the reactions to the shock and to the signal that precedes it are generalized and impulsive to a high degree. . . . With increasing training, a gradual process of concentration and individuation supervenes."[6] Supporting this general finding, Culler and his associates write "The localized, individuated response of a well-trained dog is as unlike his original excited, diffuse behavior as two acts can well be."[7]

In several different papers, Schlosberg, and Kappauf and Schlosberg have reported conditioned struggle behavior in rats with which they worked and have observed that this struggle decreased with training.[8] Both tail withdrawal and foreleg withdrawal were studied. Schlosberg noted further that one stock of rats struggled more than another and that the strugglers as a rule learned more quickly.[9] "Pre-training" or habituation served to eliminate some struggle and probably to retard the conditioning. In another paper, Kappauf and Schlosberg suggest, on the other hand, that the production of excitement is not conducive to the development of precise responses.[10]

Kellogg and Walker, studying the bilateral transfer of the conditioned flexion-response in dogs, were not so confident that the conditioned withdrawal was specifically an outgrowth of the earlier struggle-reaction.[11] Some dogs they noticed struggled a great deal, while others in the same situation struggled very little. This problem has been followed up by Kellogg and Wolf who have presented data which show that struggling or generalized behavior may exist quite independently of the conditioned flexion-withdrawal.[12] Their findings on this point are summed up in the following statement, "Some animals, although they develop the conditioned flexion-response, never struggle at all. Others may struggle almost continuously, probably in response to the confining experimental situation, but develop the conditioned flexion-CR as an entirely independent reaction. They may never eliminate struggle even if given hundreds of conditioning trials."[13]

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[3] J. B. Watson, The place of the conditioned-reflex in psychology, *Psychol. Rev.*, 23, 1916, 97.

[4] J. S. Beritoff, On the fundamental nervous processes in the cortex of the cerebral hemispheres. I. The principal stages of the development of the individual reflex: Its generalization and differentiation, *Brain,* 47, 1924, 109-148; On the fundamental processes in the cortex of the cerebral hemispheres. II. On the principal cortical elements in the arcs of the individual reflexes, *ibid.*, 47, 1924, 358-376.

[5] V. M. Bechterev, *General Principles of Human Reflexology,* 1933, 337.

[6] H. S. Liddell, The conditioned reflex, Chap. IX in F. A. Moss' *Comparative Psychology,* 1934, 272.

[7] E. Culler, G. Finch, E. Girden, and W. Brogden, Measurements of acuity by the conditioned-response technique, *J. Gen. Psychol.*, 12, 1935, 224.

[8] H. Schlosberg, Conditioned responses in the white rat, *J. Genet. Psychol.*, 45, 1934, 303-335; Conditioned responses in the white rat: II. Conditioned responses based upon shock to the foreleg, *ibid.*, 49, 1936, 107-138; The relationship between success and the laws of conditioning, *Psychol. Rev.*, 44, 1937, 379-394; W. E. Kappauf and H. Schlosberg, Conditioned responses in the white rat: III. Conditioning as a function of the length of the period of delay, *J. Genet. Psychol.*, 50, 1937, 27-45.

[9] Schlosberg, *op. cit.*, *J. Genet. Psychol.*, 45, 1934, 324.

[10] Kappauf and Schlosberg, *op. cit.*, *J. Genet. Psychol.*, 50, 1937, 43.

[11] W. N. Kellogg and E. L. Walker, "Ambiguous conditioning," a phenomenon of bilateral transfer, *J. Comp. Psychol.*, 26, 1938, 63-77.

PROBLEM

Most of the data supplied by earlier investigators has been based on general observation during conditioned-response training. It is supported principally by descriptive protocols which have necessarily been qualitative in nature. This has been an unavoidable result of the fact that methods were not available for recording the reactions of any part of the organism except the particular segment selected for study.

The present experiment was designed specifically to overcome this difficulty. It makes use of a technique for recording simultaneously the reactions of all four limbs of the *S* in addition to his respiration. This procedure makes possible a more accurate and detailed picture of the general behavior which takes place than it has heretofore been possible to obtain.

Is the specific or adaptive CR actually demonstrated to be an outgrowth of the earlier generalized activity, when this generalized activity is in part recorded and hence subject to more exact analysis? Does some other principle furnish a better description of the changes which take place? Is any single description applicable to all cases? Answers to these and similar questions should now be subject to quantitative study. The technique seeks thus to obviate the early difficulty of too concentrated an emphasis upon a portion of the organism, which was formerly inherent in the conditioned reflex method. It represents an attempt to quantify the random behavior of the *S* and to reduce it to some measurable value.

EXPERIMENTAL CONDITIONS

Subjects. Six unselected mongrel dogs supplied by an animal dealer were used as *Ss*. These were all young but mature specimens, 20-30 lbs. in weight. Two were females and four were males. One of the females died suddenly when the experiment was half completed. Her data have been included in the grouped results, however, since they were found not to alter the general trends in the results of the other dogs, but served somewhat to smooth the graphs presented.

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[12] W. N. Kellogg and I. S. Wolf, "Hypotheses" and "random activity" during the conditioning of dogs, *J. Exper. Psychol.*, 1940 (in press).

[13] *Ibid.*

Procedure. The apparatus and buzz-shock conditioning technique were similar to those employed in previous studies and have been described in greater detail elsewhere.[14] During all the training *S* was confined in a conditioning stock in a small soundproofed room, where he could be observed through one-way-vision windows. The head of the animal was fastened in a neck-piece adjustable for both size and height, and a leather belt was passed loosely under the abdomen. Each of the four feet was strapped to a balanced lever which permitted a maximum vertical movement of approximately 4 in. A Sumner pneumograph was placed around the chest just back of the shoulders. Kymographic records of the movements of all four feet, the respiration, together with the stimuli and time, were obtained in the experimenter's room outside the soundproofed room.

The conditioned stimulus was a pure tone of 1000 ~ with an intensity of 50-55 db. above the human threshold at the position of the dog's ears. It was sounded for 2 sec. on each trial. The unconditioned stimulus was a make-break D.C. shock of 0.2-sec. duration, which overlapped the buzz for the last 0.2 sec. of its 2-sec. period. The shock was delivered through stainless-steel electrodes taped to the clipped surfaces of the animal's right-hind foot just above the toes. It was adjusted for every *S* before each daily session, so as to be just strong enough to produce the maximal unconditioned flexion response of 4 in. permitted by the apparatus. If *S* made a conditioned lift of about 2½ in. near the end of the buzz-period, this automatically operated a small mercury switch which eliminated the shock-stimulation for that trial.

A day's training consisted of 20 stimulations, spaced 15 to 120 sec. apart. Sixteen of these trials were pairings of buzz and shock, while one trial in every group of 5 was a buzz-alone stimulation. The order or position of the no-shock trials was randomly varied within each group of 5. A complete training series included 400 trials (20 days) of practice. Between the 200th and 201st conditioning trials of the present experiment, an interval of from 2 to 4 mo. was allowed to elapse.

Measurements from records. The data discussed in this report are based upon measurements taken from the kymographic records. They include the relative frequency of CRs within each daily group of 20 stimulations, the amplitude or height of lift, and the latency. The principal criteria of measurement which were adopted are given below. They apply to the responses of each of the four feet.

(1) All measurable changes in contour of the record-line describing the movement of the foot were considered as responses. To be classified as *conditioned flexion-responses* they had to be movements in an upward direction. Furthermore they had to occur either (a) within the 1.8-sec. buzz-period preceding the shock or (b) within the 2.0 sec. of buzz-alone during the no-shock trials.

(2) In some trials, activity (such as struggling) was in progress before the beginning of the buzz. If this activity did not change with the presentation of the stimulus, the trial was tabulated as doubtful or uncertain with reference to CRs. It could not be determined for trials of this nature, whether the recorded behavior was or was not influenced by the conditioned stimulus.

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[14] Kellogg and Walker, An analysis of the bilateral transfer of conditioning in dogs, in terms of the frequency, amplitude, and latency of the responses, *J. Gen. Psychol.*, 18, 1938, 253-265; *op. cit.*, *J. Comp. Psychol.*, 26, 1938, 63-77; W. N. Kellogg, R. C. Davis, and V. B. Scott, Refinements in technique for the conditioning of motor reflexes in dogs, *J. Exper. Psychol.*, 24, 1939, 318-331.

(3) The amplitude of the conditioned lift was the maximal height or rise in the line describing the movement of the limb, during the buzz-period.

(4) The latency of the CR was the time between the beginning of the buzz and the break in the recording line for the foot indicating the beginning of a lift. It was measured to the nearest 0.1 sec.

RESULTS

Conditioned responses in different limbs. The most adaptive type of CR from the point of view of the experimenter was a simple lift of the RR member beginning just before the time for the shock and just high enough to escape the shock. Yet at various stages during their training one or another of the *S*s gave "adaptive CRs"—that is, CRs without struggle or violent activity—in all four feet, in three feet, in two, or in only one of the feet, which might or might not be the shocked member.

Figs. 1, 2, 3, and 4 show instances of CRs involving from one to four feet even though the right-rear foot only was shocked. In all these figures, the top line, R, is the respiration, LF, RF, LR, and RR are the tracings made by the left-front, right-front, left-rear, and right-rear members, and B, S, and T are the buzz, shock, and time-lines. Fig. 1 shows 'adaptive' responses to the buzz in all four feet during each stimulation. In reactions of this sort the different limbs were either flexed alternately in rapid succession, or else *S* supported his weight by means of the collar of the stock and the abdominal strap. Fig. 2 is similar except that the left front foot is entirely quiet. In Fig. 3, *S* has lifted only the rear members during the buzz period, while Fig. 4 shows flexion—CR of the RR limb which was high enough to escape the shock-stimulus. Conditioned respiration is also evident, especially in the responses reproduced in Figs. 1, 3, and 4. It shows itself by a change in the conformation of the breathing curve during the period of the conditioned stimulus.

Frequency of CRs. Composite graphs of the CRs made by all four limbs separately are shown in Fig. 5. The heavy solid line gives the frequency of CRs for the right-rear foot, the heavy broken line is for the left-rear, the light solid line is for the right-front, while the light broken line shows the frequency for the left-front foot. The break in all the curves after trial 200 indicates the time interval of 2 to 4 mo. which was interpolated in the middle of the training. Since one animal died during the course of the experiment, the graphs for trials 1-200 include the data from six dogs, while those for trials 201-400 are based on the results of five. Each point on the frequency curve represents the percentage of CRs given on each day of training. Doubtful or uncertain trials were eliminated from the figures by subtracting their number from the maximum possible in computing the final percentage. When results for several feet or for the same foot on different animals were grouped (as in Fig. 5) this was done by adding the total number of possible responses with doubtfuls omitted. Every point in the first half of the curves is therefore computed on the basis of 120 trials (less doubtfuls)—*i.e.* 20 daily trials for each

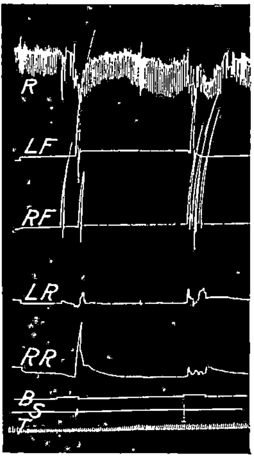

FIG. 1. FLEXION CRs IN ALL FOUR FEET

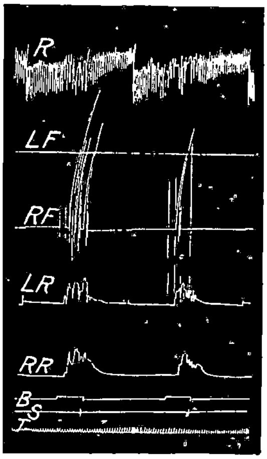

FIG. 2. FLEXION CRs IN THREE FEET

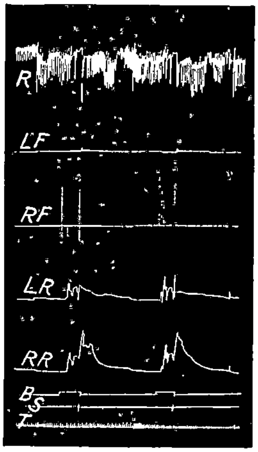

FIG. 3. FLEXION CRs IN TWO FEET

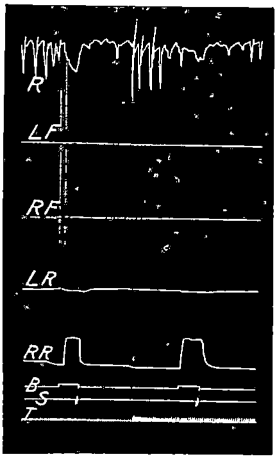

FIG. 4. A CR IN THE SHOCKED MEMBER ONLY

of six dogs—while those for the second half of the curves are based upon 100 stimulations (less doubtfuls).

The graph for the RR (shocked) foot is the typical conditioning curve which is obtained when frequency of flexion CRs is plotted. Although irregular, especially during the first 200 trials, the RR graph possesses something of the S-shaped contour which has been previously reported in conditioning experiments. The efficiency of learning increases as the curve rises from left to right.

This is not the case, however, in the curves for the three non-shocked feet. Since the lifts made by these members were superfluous or unnecessary, the efficiency of learning increases as the graphs drop or fall from left to right. The CRs of the three non-shocked members are in effect 'errors,' and may be considered as equivalent to the errors plotted in the orthodox trial-and-error experiment.

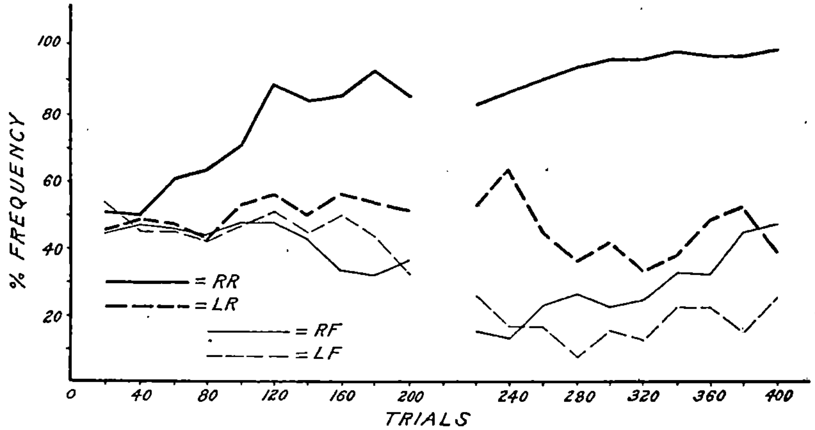

Fig. 5. Composite Relative Frequency Graphs of the Flexion Responses Made by all Four Feet During the Process of Developing an "Adaptive" CR in the RR Foot Only

Trials 1-200 were plotted from the data of six animals; trials 201-400 were plotted from the data of five. The break in the curves indicates an interpolated time interval of 2-4 months.

When the results of all the *Ss* are combined in this manner, it is clear that in the early trials the *Ss* *as a group* lifted all four feet with about equal frequency. As training progressed, the frequency of responsiveness of the shocked member increased at the same time that the frequency of responsiveness in the non-shocked members decreased. We have here different sorts of learning curves depicting the activity of different parts of

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the body which took place simultaneously in the same experimental situation. The *Ss* were clearly learning two things at the same time. The graph of the shocked (RR) member shows how they learned *what to do.* The graphs of the other three feet show how they learned *what not to do.*

A definite trend or progression in the frequency of responsiveness of the different feet is apparent especially during the latter part of the training. *Those members which were anatomically closer to the point of electrical stimulation responded with greater frequency than the more distant parts.* Thus the order of frequency of responsiveness was RR, LR, RF, and LF.

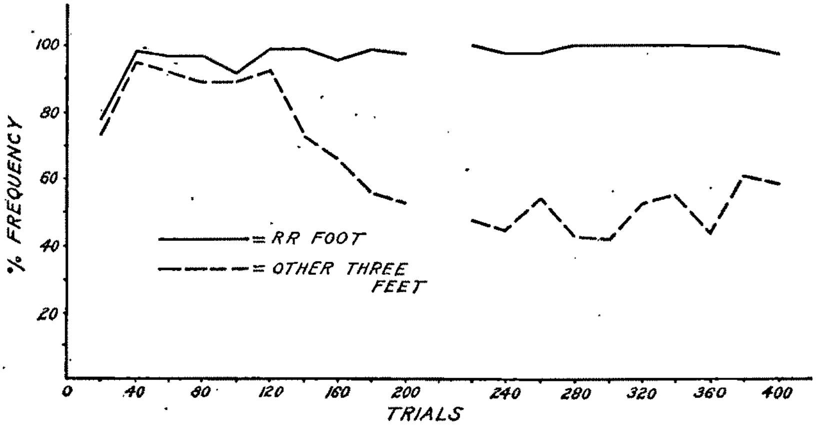

FIG. 6. FREQUENCY OF CRs FOR THE EXCITABLE *Ss*

The graphs show very little detrimental or forgetting-effect as a result of the interpolated time interval of 2-4 mo. After the time interval the curves seem to continue the trends established before it, with but minor variations. Such a result substantiates an earlier finding from this laboratory to the effect that the nature of the behavior after intervals up to 6 mo. is essentially a reproduction of that at the beginning of the intervals.[15]

The writers would like, at this point, to suggest a certain caution in the interpretation of the results shown in Figs. 1, 2, 3, 4, and 5. These results should *not* be taken to mean that in the case of all dogs, the first CRs to appear are present in all four feet, and that the superfluous feet are then successively eliminated. It is doubtful if such precision can be found in the case of any single *S*. The presence of CRs in the non-shocked members should be thought of rather as a matter of individual variation and fluctuation from *S* to *S*. While there are certain general trends, which show up when the data are grouped, there are also marked individual differences. Dog 17, for example, raised its right-front foot a much greater number of times than either of the other non-shocked feet. This foot was often pumped up and down hard and fast during the presentation of the conditioned stimulus. The amplitudes for the responses of this foot, even when the movements become fewer in number, remained greater than the amplitudes for the RR or for any other limb. Comparable individual peculiarities were also observed in the other *S*. These are to a considerable extent cancelled out in the group curves.

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[15] Kellogg and Wolf, The nature of the response retained after several varieties of conditioning in the same subjects, *J. Exper. Psychol.*, 24, 1939, 366-383.

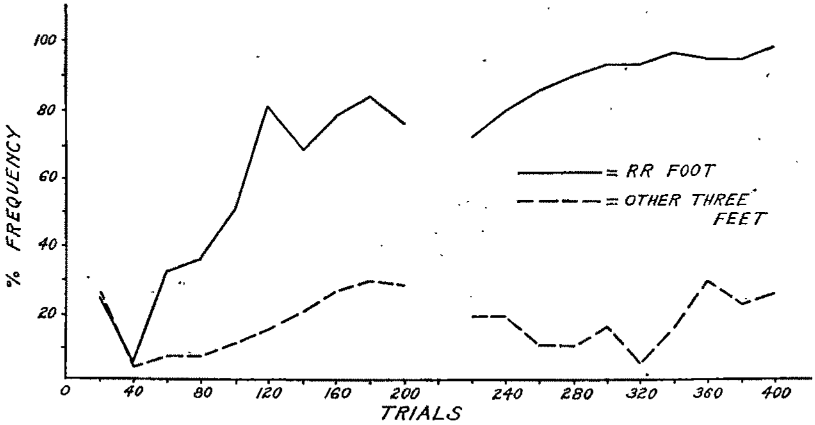

FIG. 7. FREQUENCY OF CRs FOR THE QUIET *S*s

Excitable and quiet animals. An examination of the separate performances of the different animals shows that three of the *S*s had a much greater propensity to lift the non-shocked members than the other three. They may well be characterized as 'excitable' animals. These *S*s displayed a great deal of so-called random behavior during their conditioning which was not present in the other dogs. This included struggling, barking, howling, excessive tail movements, a large number of limb movements between trials, biting of the stock in response to one or the other of the stimuli, and defecation and micturition. Since the other three animals displayed but little of this sort of behavior, they may properly be characterized as 'quiet' animals.

If now the frequency scores of the three excitable animals are grouped together and the frequency scores of the three quiet animals are similarly grouped, the results given in Figs. 6 and 7 are obtained. In each of these figures the solid line represents the frequency of the RR foot and the broken line is the composite frequency for the other three feet. Fig. 6 shows that the excitable animals began their training with a frequency of responsiveness in all four feet of over 70%, whereas the quiet animals (Fig. 7) began with a frequency of less than 20%. In the case of the excitable animals, the CRs in all four feet were near 100% for 120 trials. At this point a gradual reduction in the responsiveness of the non-shocked members began, while the shocked foot remained near the 100% level. The quiet animals, on the other hand, at no time lifted the three non-shocked feet with a composite frequency of greater than 30% and did not arrive at the 100% level with the shocked member until they had received almost 400 trials.

What the excitable animals had to learn was *what not to do.* Yet they never succeeded in reducing their non-shocked CR much below the 50% level. What the quiet animals had to learn was *what to do.* This they did in a gradual manner—the responses of the non-shocked feet remaining at a low level throughout their training. The quiet group of *S*s has an S-shaped conditioning curve for the RR (or shocked) foot. There is no evidence of this phenomenon in the case of the excitable animals.

Such results preclude any general inference to the effect that the adaptive CR regularly develops from the earlier conditioned struggle of the animal. A statement of this sort may hold for some *S*s, but it certainly does not apply to all.

Amplitude of CRs. Analogous curves prepared from the amplitude data gave strikingly similar, though not identical, results. This is not because one of these measures is a function of the other. The average amplitude of lift was obtained by totalling the number of inches lifted in each day's training, and dividing this total by the number of CRs made. Average amplitude as computed in this way is not affected by the frequency of CRs, but is the average height of the responses that do occur. This makes the two measures independent.

The excitable *S*s made flexion CRs which were of greater extent on the average than those of the quiet *S*s. The average height of lift in the non-shocked feet became gradually less for the excitable animals, although it never dropped much below 2 in., which was one-half the maximum distance permitted by the apparatus. CRs made by the quiet animals were of small amplitude at first although they gradually increased in amplitude—following an S-shaped contour—as training progressed. Curves derived from the amplitude data were, in fact, so similar to those already presented for frequency of CRs that it is unnecessary to reproduce them here.

Latency of CRs. With regard to latency, however, the differences in the behavior of the two groups tended to disappear. The latency of responsiveness in the quiet animals was a little greater than that of the excitable group but by an amount so small as to be unreliable. Some dogs were consistently slower or faster than others. In two instances there were slight trends apparent, but they were in opposite directions. There was no general tendency for the latency to increase or decrease within either group. In fact, the major finding of the present study with regard to latency is its extremely small change throughout a fairly long period of training.

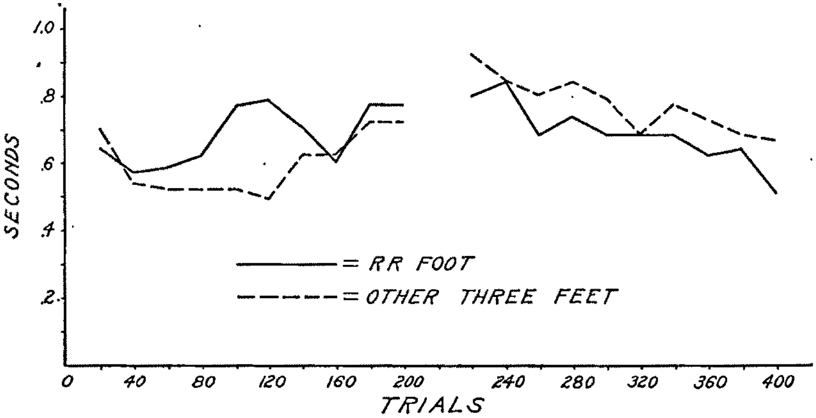

FIG. 8. COMPOSITE CURVES OF THE LATENCY OF CRS FOR ALL *S*s
The first half of these curves is based upon the data from 6 animals. The last half is based upon the data from five animals.

Fig. 8, in which the latencies are combined for all the *S*s, illustrates this fact. The values plotted represent the average latency for all CRs which occurred, for which unquestionable measurements to the nearest 0.1 sec. could be made. The last 200 trials as indicated in Fig. 8, show some slight evidence of a decreasing latency in the responses of all four feet, but this is largely counterbalanced by the increase which occurs within the first 200 trials. It will further be noted (for the last 200 trials) that the shocked member was lifted on the average a little earlier than the non-shocked members. Here again, however, the results are no more than suggestive and the actual differences are very small. The general constancy of the latency values obtained in this experiment is of greater significance than these minor variations. Its special interest lies in the fact that the study of the latency of the CRs during training, as reported in recent experiments, presents a conflicting and somewhat obscure picture.

Hilgard and his collaborators, for example, have found a general decrease in latency with training, yet an analysis of their data shows that this relationship does not hold for all the *Ss*.[16] Wickens has reported that 'involuntary' conditioning is negatively correlated with frequency, while 'voluntary' conditioning may be positively correlated.[17] Unfortunately, however, the correlation coefficient which he obtained for his 'voluntary' group was not statistically significant. In an earlier publication from the present laboratory, Kellogg and Walker suggested that latency is on the whole negatively related to the stage of training, but their data were so irregular as to leave room for a large element of error.[18] The comparative unreliability of latency as a measure of performance in the conditioning situation was discussed at that time.[19] From the further analysis of the latency of the CR in the present experiment, it can only be concluded that the *average* latency may vary little throughout a long conditioning series.

Considered as a whole, these somewhat confusing results from different laboratories point to the generalization that latency as thus far measured, must certainly be regarded as the most unpredictable of the three common measures of the CR (frequency, amplitude, and latency).

SUMMARY

This experiment represents an attempt to get at the genesis of the process of conditioning in a measurable or quantitative way. The flexion CR of the right rear foot of dogs was the response conditioned, but kymographic records were made of the reactions of all four feet and of the respiration throughout the entire experiment. By this method a better picture of the general behavior of the organism was obtained than is possible when records are taken from the conditioned member alone.

Six unselected mongrel dogs were used as *Ss*. Five of them received 400 conditioning trials, and the sixth 200. The conditioned stimulus was a 1000 ~ buzz, 50-55 db. above the human threshold, which was sounded for 2 sec. The uncondi-

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[16] E. R. Hilgard, Conditioned eyelid reactions to a light stimulus based on the reflex wink to sound, *Psychol. Monog.*, 41, 1931, (no. 184), 1-50; E. R. Hilgard and D. G. Marquis, Acquisition, extinction, and retention of the conditioned lid response to light in dogs, *J. Comp. Psychol.*, 19, 1935, 29-58; E. R. Hilgard and A. A. Campbell, The course of acquisition and retention of conditioned eyelid responses in man, *J. Exper. Psychol.*, 19, 1936, 227-247.

[17] D. D. Wickens, A study of voluntary and involuntary finger conditioning, *J. Exper. Psychol.*, 25, 1939, 127-140.

[18] W. N. Kellogg and E. L. Walker, An analysis of the bilateral transfer of conditioning in dogs, in terms of the frequency, amplitude, and latency of the responses, *J. Gen. Psychol.*, 18, 1938, 253-265.

[19] *Ibid.*, 261.

tioned stimulus was a D.C. shock of 0.2-sec. duration, delivered to the right rear foot at the termination of the buzz. A CR was credited to any limb which was lifted during the period of the conditioned stimulus.

(1) One or another of the *S*s made CRs in all four feet at once, in three, two, or in any one of the feet alone, during the course of the training.

(2) The frequency of CRs in the shocked feet increased with training for the group as a whole, while the frequency of the CRs in the non-shocked feet decreased with training.

(3) Since the responses of the non-shocked feet were superfluous, unnecessary, or 'random,' their elimination is not unlike the elimination of errors in the typical trial-and-error experiment.

(4) Learning curves of the three non-shocked feet show how the *S*s learned *what not to do*. The learning curve of the shocked foot shows how the *S*s learned *what to do*.

(5) Those limbs which were anatomically closer to the point of electrical stimulation responded with greater frequency than the more distant parts. Thus the order of frequency of the CRs was RR, LR, RF, and LF.

(6) An examination of the individual records of the separate *S*s shows that three were active or excitable while the remaining three were considerably more quiet during training. The data from the 'excitable' and the 'quiet' animals were therefore grouped together for comparative purposes.

(7) The excitable *S*s made CRs in all four feet with a frequency of almost 100% from the start of training. Their learning problem reduced itself primarily to the elimination of the unnecessary or 'random' movements. They had to learn *what not to do*.

(8) The quiet *S*s, on the other hand, made very few responses with any of the feet at the start of training. They had to learn *what to do* by building up a CR in the RR member. The learning curve for the foot conditioned in the case of these *S*s possessed the typical S-shaped contour which is characteristic of conditioning.

(9) The conditioned flexion-response cannot be regarded as an adaptive reaction which is refined from out of the original raw material of generalized conditioning. Such a description will apply only to some cases. It does not hold for all.

(10) A treatment of results both in terms of frequency and amplitude of CR has supported this generalization. Yet frequency and amplitude as used in the present experiment are independent measures.

(11) The latency of the CRs from both shocked and non-shocked feet was roughly constant throughout the whole period of training. This is of special significance in view of the fact that some students of conditioning have reported a decreasing latency with training while others have reported irregular or inconsistent trends.

CEREBRAL MECHANISMS IN CONDITIONING UNDER CURARE

By EDWARD GIRDEN, Brooklyn College

It has been long believed that the effect of curare is to leave the organism functionally intact except at the myoneural junction. This would mean that the sensory, cerebral, and motor components of the nervous system are still capable of normal activity, but that neural innervation of the striate musculature can no longer be effected. One could thus attempt to establish conditioned responses (CRs)[1] during this drugged condition (life being maintained by artificial respiration) and test for modifications in behavior subsequent to recovery. Such a study was reported by Harlow and Stagner and the data interpreted in favor of a motor theory of learning.[2] In short, many combinations of bell followed by shock in the curare-state failed to establish a conditioned response of striate musculature. On the other hand, pupillary conditioned reflexes could be established under the drug which persisted undisturbed afterwards.

More recently, Girden and Culler have shown that these findings are equivocal for several reasons, and do not actually provide a crucial test of the motor theory.[3] Other phenomena also appeared which have led to several new considerations. It was found that, with doses of curare large enough to produce the usual symptoms of paralysis, a single isolated muscle such as the M. semitendinosus is still capable of reponding to *sensory* stimulation. This curare-response which then results is decidedly unlike the normal reaction, and it is possible that not only is there a quantitative reduction in the magnitude of the response under the drug but also concomitant qualitative changes.[4]

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[1] The following abbreviations are used in this report: CR = conditioned response; CS = conditioned, substitute stimulus (bell); LSM = left semitendinosus muscle; RSM = right semitendinosus muscle. Successive curare and normal periods are indicated respectively by *1st Curare, 1st Normal, 2nd Curare,* etc.

[2] H. F. Harlow and R. Stagner, Effects of complete striate muscle paralysis upon the learning process, *J. Exper. Psychol.,* 16, 1933, 283-294.

[3] E. Girden and E. Culler, Conditioned responses in curarized striate muscle in dogs, *J. Comp. Psychol.,* 23, 1937, 261-274.

[4] There is independent evidence to show that, contrary to the traditionally accepted

These modified responses were capable of being conditioned to a bell while the animal was in the drugged-condition, and were evidenced *so long as and only when* the animal was in the curare-state. When the animal recovered from the drug, the CR was inhibited. That it had not been subject to extinction in the interim was manifest, for upon re-curarization the CR reappeared undiminished in strength. Tests made at the time indicated that there was a cortical inhibition under the drug, which was interpreted to mean: Curare CR = Thalamic CR; whereas Normal CR = Cortical CR. This hypothesis was of course applied only to the particular experimental situation which employed the use of striate musculature. Culler and his associates have since made more exact electrical measurements at the motor cortex and the spinal motor roots to determine the threshold sensitivity for the semitendinosus muscle for both normal and curare conditions.[5] Their results support the tentative hypothesis suggested by the previous functional data, and Culler concludes that "Somewhere between cortex and ventral root passes a plane of cleavage: a surface whose existence is demonstrated by our data even though its location and contour cannot be definitely stated. To one side of the boundary (cephalad) is a zone of depression; to the other (caudad) is a non-depressed (possibly sensitized) area."[6]

If, therefore, curare produces a condition of *functional decortication,* we have two systems capable of independent modifications. Whenever the cortical system is active, the subcortical is inhibited; if the cortical system is depressed, the subcortical pathways may be evoked.[7] If this hypothesis is correct, then *actual* decortication by operation should provide a situation in which this reciprocal amnesic condition can no longer be obtained. In such preparations the Curare CRs should appear in the normal state, and learning developed in the non-curarized situation should manifest itself in the drugged animal. The curare, however, may also inhibit the thalamic level, and, as suggested by Culler, "it is not impossible that

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view, curare produces physiologically changes besides the block at the myoneural junction (cf. J. S. Light and W. H. Gantt, Essential part of reflex arc for establishment of conditioned reflex, *J. Comp. Psychol.,* 21, 1936, 19-36). In fact, the effect of curare upon the nervous system is not restricted to the motor-end-plate (cf. W. B. Cannon and A. Rosenblueth, The transmission of impulses through a sympathetic ganglion, *Amer. J. Physiol.,* 119, 1937, 221-235).

[5] E. Culler, J. D. Coakley, P. S. Shurrager, and H. W. Ades, Differential effects of curare upon higher and lower levels of the central nervous system, this JOURNAL, 52, 1939, 266-273

[6] *Ibid.,* 272 f.

[7] A somewhat analogous condition occurs in the effect of suggestion upon pupillary CR in human subjects, in which conscious 'sets' modify the unconscious pupillary CR. These data will be reported in a separate paper.

the plane of cleavage passes somewhere *below* the thalamus."[8] Negative results with decorticate animals would, therefore, be inconclusive, whereas positive findings would support the original hypothesis.

Specimens prepared for conditioning of the semitendinosus muscle can be used only for a few days. The decortication must therefore be performed first, with a sufficient period of post-operative rest to permit the recovery of function. What degree of decortication should be used? In order to explore the question systematically, the simplest situation was obtained for the present investigation; namely, bilateral extirpation of the cortical auditory areas.

All auditory conditioned reflexes (involving paw-flexion) are abolished when the sylvian gyri are removed bilaterally in a single operation.[9] Auditory function, however, has been attributed to the surrounding area.[10] Both the sylvian (areas 78 and 78b)[11] and the large surrounding ectosylvian gyri (areas 68, 68b, and 38b) were, therefore, removed completely from both hemispheres in a single operation. We can thus be certain that any subsequently established auditory conditioned responses must be subcortical in pathway so far as the conditioned stimulus (bell) is concerned.

Procedure. The cortical auditory areas were removed in a single aseptic operation performed under Nembutal anesthesia. After a lapse of at least two weeks the semitendinosus muscles were then exposed under ether anesthesia. After recovery curare was injected intravenously. Rarely was more than a minute or two required for the injection of the drug and the placing of the animal in the experimental cradle for testing or training. Either before or immediately after injection of the curare, a tracheal catheter was inserted.[12] Artificial respiration was maintained by means of a Harvard respirator. This process of curarization and recovery could be repeated indefinitely without mortality.

The time for complete recuperation is manifestly a function of a number of factors. In the first study, young animals, practically puppies, were used. In the present study, the dogs were from 1–2 yr. of age and weighed from 10–15 lb. The potency of different samples of curare is unpredictable.[13] In the present inquiry, complete recovery occurred about two hours after the intravenous injection of approximately 1 cc. of a 1% solution.

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[8] Culler, Coakley, Shurrager, and Ades, *op. cit.,* 273.

[9] E. Girden, Cerebral determinants of auditory localization, this JOURNAL, 52, 1939, 1-15.

[10] Cf. J. W. Papez, *Comparative Neurology,* 1929, 1-518. Also F. Bremer and R. S. Dow (The cerebral acoustic area of the cat, *J. Neurophysiol.,* 2, 1939, 308-309) who have shown that the cerebral acoustic area in cats includes parts of the ectosylvian as well as the sylvian gyri.

[11] After Papez, *op. cit.* .

[12] A brass tube, fit snugly over that end of the catheter which rested in the animal's mouth, protected the catheter from being bitten during the process of recovery. It was secured to the lower jaw by bandage, which helped keep the throat and mouth clear of the large quantities of saliva secreted during the drugged condition. The animal could thus be transported without fear of displacing the catheter.

[13] The artificial preparation of the active ingredient in the laboratory may eliminate

The technique of conditioning the animals has already been described.[14] A bell was sounded for about 3 sec., terminating with the simultaneous application of a momentary shock to the paw of the desired hind limb. Animals 15 and 16 were trained under curare and tested after return to normal. Animal 17 was first conditioned in the normal state and tested in the subsequent drugged condition. After the animals were dispatched, the brains were placed in 10% formalin solution for further study.

Results. Animal 15. Twenty days after the cortical operation, both semitendinosus muscles were exposed under anesthesia, the catheter inserted, and the animal permitted to recover from the ether.

1st curare. The drug was injected at 3.00 P.M., artificial respiration initiated and the animal placed in the test-cradle. Conditioning scores with bell-shock combinations were as follows: 0/5, 0/5, 3/5, 3/6.[15] When incipient signs of recovery appeared, training was halted (3.40 P.M.). The response noted was the typical curare CR. Complete recovery occurred by 5.00 P.M. and artificial respiration was then halted, and the catheter removed.

1st normal. At 6.00 P.M. the animal was returned to the cradle and four tests with bell alone were made. (On all test-trials, the CS was given *alone* without shock reënforcement.) On the second trial there was general diffuse struggle behavior, as a consequence of which both semitendinosus muscles contracted. No responses whatsoever were made on the other three trials, the animal remaining completely inert. The reaction on the second trial was therefore interpreted to be the result of spontaneous activity and not conditioning to the bell.

2nd curare. After injection of the drug at 6.19 P.M. several preliminary tests with CSs alone failed to evoke the CRs. Conditioning was then continued until 7.00 P.M. with the following results: 3/5, 3/5, 5/5, 5/5, 5/5. The animal appeared to be even slightly deeper under the drug than in the first session.

2nd normal. Tests for the CR after recovery were made at 8.11 P.M. The following are the protocols of successive trials with CS alone:

(1) Both semitendinosus muscles contract; the left semitendinosus muscle (LSM) reacted first; then the right semitendinosus muscle (RSM). These reactions were followed by diffuse behavior. (2) A small twitch of LSM was followed by a slight movement of the rear part of the body. (3) Definite 'pull' as distinguished from 'twitch,' occurred in LSM. This started at the tip and spread through the body of the muscle, persisting for a second or two. (4) CR was the same as in the third trial, although the response was not as strong. (5) Again CR of LSM appeared, but limited this time to tip of muscle. (6) LSM CR this time was more like the characteristic curare-twitch.

3rd curare. The injection was made at 8.27 P.M. and four tests with CSs produced the following results.

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this difficulty. Cf. K. Folkers and R. T. Major, Isolation of erythroidine, an alkaloid of curare action, from erythrina americana mill, *J. Amer. Chem. Soc.*, 59, 1937, 1580.

[14] Girden and Culler, *op. cit.*, 262 ff.

[15] The numerators denote the number of CRs, whereas the denominators indicate the total number of trials. In this session there were a total of 6 CRs out of 21 trials.

(1) CR of LSM was followed by a similar action of RSM, in typical curare-action. (2) CR again appeared as before. (3) Response in RSM was sharp, followed by a twitch of the tip of LSM. (4) Repetition of CR as noted in third trial.

The CR did not appear in the first normal period apparently because of insufficient training in the previous curare session. This interpretation is supported by the fact that the CR failed to carry over from the first to the second curare sessions. With more training in the latter, the CR now appeared in the subsequent, *i.e.* the second, normal period. Furthermore, the CR now persisted into the third curare session without further training. During the second normal period, there was some spontaneous activity which was not unusual under these circumstances. Great care was taken to avoid successive presentations of the CSs in a regular time-order. One could also anticipate the periods of activity and avoid test trials during these intervals.

While it is true that no tests preliminary to conditioning were made to determine the nature of the unconditioned reactions to the bell, we do know that the end result was true conditioning. The CR in the second normal session can be attributed only to the training in the previous, second, curare period, since there was no CR (or unconditioned response to the bell-alone) in the first normal session. As shown with the next two animals, the bell before conditioning produces no action of the semitendinosus muscles.

Animal 16. Twenty-five days after the cortical extirpation, the animal was taken to the experimental situation. Both semitendinosus muscles were exposed and the animal placed in the test cradle. The following protocols of 5 preliminary trials with CSs alone are relevant.

(1) A fine "shiver" occurred, but no discernable action of the semitendinosus, or any other muscles, noted. (2) This time the animal remained completely inert. (3) A little movement of the left hind limb-as-a-whole occurred. This produced a slight contraction of the LSM. (4) Left hind limb begins to twitch spasmodically before the bell was sounded. This action continued undisturbed while the CS was sounded. Neither semitendinosus muscle contracted. (5) Same as on previous trial.

Throughout the above test there was a continuous, slight shivering of the body (fine muscle-contractions) not unusual under the circumstances. The behavior during the third trial can be best understood as a natural consequence of exposure of the muscles, since a similar action occurred on the fourth trial before the CS was presented. The animal was surprisingly quiet in the situation.

1st curare. Some spontaneous contractions of the semitendinosus muscles, indicative of the curare-effect, appeared after curarization at 2.37 P.M. With the animal relaxed, two trials with CSs were negative; no reactions of these muscles were evoked. CR training was now initiated with the following results: 0/5, 2/5, 3/5, 3/5, 5/5. The period was terminated at 3.05 P.M.

1st normal. Entirely recovered at 4.00 P.M. the test was given in the usual fashion with successive trials of CSs alone. The CR was shown to be unmistakably present as the following protocols reveal.

(1) Immediate contraction of LSM was followed by a general body twitch. (2) Separate contractions of LSM and a sharp jerk of the left hind limb distinct from the single muscle action were produced. (3) Less intense but clearly observed, was a twitch of LSM. (4) Delayed contraction of LSM appeared about ½ sec. before termination of the CS. The form of the response was this time less of a twitch and more of a 'slow pull.'

The presence of a general body tremor tended somewhat to disguise the CR of LSM. Every indication, however, pointed to the conclusion that these reactions were not artifacts but real conditioned responses to the bell. The test was completed at 4.20 P.M.

2nd curare. Under the drug at 4.23 P.M. the CR was again present. Further training was given, however, to reënforce the CR: 6/10, 6/10, 7/10, 6/10, 9/10, 10/12. Training was halted at 5.04 P.M.

2nd normal. At 6.14 P.M. the animal was back in the cradle, entirely recovered and ready for testing. The results of successive well-spaced trials with CS alone were as follows.

(1) Some possible contraction of RSM occurred. General body tremor was inhibited by CS. (2) Same as first trial, but head was also raised to CS. (3) Clearcut CR of LSM appeared about ½ sec. before the bell was terminated. Body tremor previously noted was again present undisturbed before, during and after the CS-interval. (4) CR of LSM again occurred as in the previous trial; body tremors, this time, possibly reduced by CS. (5) There was a possible CR of LSM; otherwise the results were much like the first trial. (6) Definite CR of LSM occurred with possible accompanying reaction of RSM. (7) No responses of any kind observed.

3rd curare. Tests with CS alone under the drug at 6.26 P.M. showed that the CR was still present, although somewhat reduced in magnitude.

We thus see that no original, unconditioned reactions of the semitendinosus muscles were produced by the bell in the untrained animal. After conditioning under curare, the CRs appeared in the subsequent first and second normal sessions. That there was some tendency for the CRs to become weaker is not surprising. A total of 77 training-combinations (bell-shock) had been applied during the first two curare sessions. After the second curare period no further training was given, although a total of 15 presentations of bell alone (without shock) were made in the two normal sessions and the final curare period. Clearly, the reduction in the magnitude of the CRs was a function of extinction.

Animal 17. Fourteen days after the cortical operation, the animal was taken to the test cradle. (2.10 P.M.).

1st normal. Five preliminary trials with CS alone were negative. On the first trial, the head oriented from left to right, but the rest of the body remained inert. On the last 4 trials no reaction whatsoever could be detected. The animal was now given 100 combinations of bell-shock to left hind paw, which constituted the first conditioning session. On the following day, two more training periods were had, 125 and 165 trials respectively. By the end of the third session (concluded at 5.35 P.M.) the CRs in this intact left hind limb had developed to 100% level of performance. The semitendinosus muscles were now exposed. An additional 110 combinations of bell-shock to left hind paw were presented (6.35–7.42 P.M.) to insure satisfactory retention of the CRs in the exposed LSM.

1st curare. Drug was administered at 7.45 P.M. and the usual procedure was followed. Preliminary application of shock to paws of respective hind limbs produced typical unconditioned curare-twitch reactions. The critical tests with CS alone therefore begun at once, and the following observations were recorded.

(1) Both LSM and RSM contracted in "jiggle" fashion; that is, several successive jerks occurred simultaneously in both muscles. (2) There was a CR of LSM alone, as in the first trial. A single test with shock-alone to left hind paw was interposed at this time. The unconditioned response of LSM was now much weaker than in the preliminary tests made before the two bell-trials. (3, 4) Two more tests with bell-alone were now negative. Shock-alone again applied failed, this time, to produce any muscle-reaction. (5) In a little while, the muscle-response to shock-alone reappeared. Shortly after, the bell-alone evoked the CR of LSM (8.04 P.M.), somewhat limited in magnitude and restricted to a small section of the muscle. (6) At 8.06 P.M. CR of LSM was again produced by bell-alone. This time the response was full strength, typical curare-twitch, as noted before the interval of depression. On this last trial there also followed a weaker CR of RSM. At 8.25 P.M. the first symptoms of recovery were manifested: stronger heart action, and some occasional breathing reflexes.

Successive trials had been well spaced and presented in irregular time-order. No activity occurred, other than that reported above. Even the 'ripple' effects, noted in some animals under curare, were absent. It is clear that the failure of the CRs to appear on two of the trials with CS was due to a transition period of deeper curarization. When this happens, a rest must be taken until the return of curare-function is indicated by the reappearance of the shock-response. Once this occurred, the CR to bell returned full strength. Aside from the two negative trials during the interval of depression, the CR to bell was 4/4 (100%).

In the present animal we have the converse condition obtained in the previous specimens, Animals 15 and 16. In the latter two cases the CRs developed under curare persisted through the following normal period. In the present instance, Animal 17, the CRs established in the normal animal persisted in the following curare session. None of these results can be attributed to incipient recovery. In Animal 17 the striate musculature were still completely inert, and the first signs of recovery did not appear until at least 20 minutes after the conclusion of the test. In no case were tests under the drug ever continued until voluntary striate muscle activity appeared. To the contrary, testing was always halted just as soon as any signs of recovery occurred. Even under such circumstances normal CR can not be developed.[16]

Discussion. It is clear from the present results that conditioned responses of the semitendinosus muscle to an auditory stimulus in the normal animal are mediated by cortical pathways, whereas the CRs developed under curare involves subcortical systems. When a section of the cortical pathway is literally extirpated, the block between the normal and curare states is disrupted; the CRs established in either one of the two conditions will now be manifested in the other state.

Some interesting questions now arise. Will the amnesia also be disrupted if the motor area is removed? Will this also happen if the somesthetic sensory area (essential to the conduction of the sensory impulses produced by the shock-stimulus) is extirpated? Can this reciprocal block be broken instead by the removal of cortical areas *not directly concerned* in this learning? Is the conditioning under curare completely subcortical in locus? Although the cortical system (auditory area) for the CS (bell) was extirpated in the present study, it is possible that the motor component was still, somehow, cortical in locus. Totally decorticate dogs are capable of developing conditioned responses[17] as are spinal preparations.[18] Can curare have a similar dissociative effect in totally decorticate animals, whereby a block is established between the thalamic (now the 'normal') level and some still lower brain center? All these questions are easily subject to experimental test.

Another point deserves consideration. It is accepted that these dissociations do not occur in the case of the pupillary CRs. However developed, in normal or curare state, they will appear in the other situation.[19] This fact does not contradict our hypothesis. Since Marquis and Hilgard have shown that extirpation of the occipital lobes does not disturb previously established visual conditioned lid responses, the latter were obviously subcortical in locus.[20] When the pathways for the CRs are cortical (as in the case of auditory paw-flexion conditionings) extirpation at this level

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[16] H. Harlow (The effect of curare on the learning process, *Psychol. Bull.*, 34, 1937, 744) writes "Even when bodily movements were not entirely abolished the capacity for formation of conditioned responses [in the intact animal] was lost."

[17] E. Girden, F. A. Mettler, G. Finch, and E. Culler, Conditioned responses in a decorticate dog to acoustic, thermal and tactile stimulation, *J. Comp. Psychol.*, 21 1936, 367-385.

[18] P. S. Shurrager and E. A. Culler, Phenomena allied to conditioning in the spinal dog, *Amer. J. Physiol.*, 123, 1938, 186-187.

[19] Cf. Harlow and Stagner, *op. cit.*

[20] D. G. Marquis and E. R. Hilgard, Conditioned lid responses to light in dogs after removal of the visual cortex, *J. Comp. Psychol.*, 22, 1936, 157-178.

(cortical auditory areas) disrupts the CRs.[21] Curarization would, therefore, have no more effect upon pupillary conditioned reflexes than it does upon auditory CRs developed after extirpation of the cortical auditory areas. (Observation of the pupillary CRs in the intact animal is possible since the functioning of smooth muscle is unaffected by curare.)

Culler has suggested that the curare produces a condition analogous to "dual personality."[22] This term is usually applied to more complicated abnormal conditions in human beings, disturbances involving changes in character traits. A more appropriate analogy, that could be defended operationally, is the concept herein employed—*amnesia*. While the term dual personality was applied only in a descriptive fashion without any explanatory implication, the term amnesia is less susceptible to misuse or misinterpretation.

Intact animals conditioned under curare give every indication after recovery of having been unconscious.[23] It is extremely difficult to believe that conscious processes are involved in the development of conditioned reflexes in spinal preparations.[24] The interpretation of conditioning in totally decorticate animals is more difficult. Granting the presence of conscious behavior in the latter condition, not even the slightest indication of such activity is manifested in normal animals after recovery from curare. There is perhaps no value to be derived from raising this question of conscious behavior. However, in so far as the striate musculature is involved (which would also serve to express autonomic upset) these animals gave absolutely no indication of being aware of what had happened in the curare state. In this sense of the word, they were totally unconscious at the time.

The present data have some bearing on this question. In the partial decorticate, the amnesic condition is disrupted so that the curare CR now appears in the subsequent normal period. But the animal still appears to be unaware of what had occurred under the drug. That is, the animal does not manifest the diffuse struggle behavior ordinarily present during the first sages of conditioning.[25] Some of the protocols do indicate struggle behavior, but it is hard to say whether this is spontaneous activity or not. If it is a manifestation of diffuse CR behavior, its magnitude and frequency is far below the degree normally present.

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[21] Cf. Girden, *op. cit.*, 1-15; and Girden, Mettler, Finch, and Culler, *op. cit.*, 372 f.

[22] Culler, *op. cit.*, 267.

[23] This interpretation is supported by our own previous results as well as the data of the other investigators. The experimental technique, conditioning under the drug, and post-curare behavior have now been recorded on a 16-mm. film, entitled "The Dissociative Effects of Curare."

[24] Shurrager and Culler, *op. cit.*, 186 f.

[25] Cf. E. Girden, Conditioning and problem-solving behavior, this JOURNAL, 51, 1938, 677-678. On the other hand, the absence of such behavior may have some bearing on the question of the motor theory of learning.

It seems as if the animal is still unaware of what had happened under the drug, the semitendinosus muscle CR occurring much like the motor automatisms reported in hysterics. Whether the curare state is one of complete unconsciousness or the recovery period represents a condition of amnesia, is difficult to determine without introspective evidence. It does, however, seem possible, by studying specimens with varying degrees of decortication, to determine whether total organized (conscious?) bodily responses can eventually be produced in a post-curare situation after training under the drug. It would be highly desirable, of course, to make use of photographic techniques, and test for possible internal autonomic changes as well.

ACTIVITY OF THE HUMAN NON-PREGNANT UTERUS

By EDMUND JACOBSON, JULIUS E. LACKNER, and M. B. SINYKIN, Chicago, Illinois

Aside from pregnancy, very little is known concerning the activity of the human uterus. Bags for the registration of contractions during labor were first introduced by Schatz in 1872;[1] but in the non-pregnant human uterus the first mechanical study of contractions was made by Knaus, employing a manometer in 1929 and a bag in 1933.[2] In 1934, Moir working under conditions which he considered more comfortable for the patient concludes that "the investigations thus far have shown that it is always possible to record contractions in the non-pregnant uterus."[3] Bags have been used in recent investigations by one of us, particularly with reference to effects of hormones.[4]

The discovery that electricity shifts when striped muscle contracts was made by Matteucci almost one hundred years ago.[5] He placed two wires in a frog's muscle along the length of the fibers. His galvanometer showed that the wire closest to the point of contraction became for the instant negative, as compared with the other wire. Countless investigators have confirmed this finding. In 1907 Piper applied it to man.[6] In 1930 one of us showed how even microscopic contraction in human skeletal muscle can be measured quantitatively in electrical terms.[7] These methods are now applicable to the human uterus.

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[1] F. Schatz, Beiträge zur physiologischen Geburtskunde, *Arch. f. Gyn.*, 3, 1872, 58-144.

[2] H. Knaus, Eine neue Methode zur Bestimmung des Ovulationstermines, *Zentbl. f. Gyn.*, 53, 1929, 2193-2203; G. K. F. Schultze, Die Reaktion der nicht schwangeren menschlichen Gebärmutter auf Hypophysenhinterlappenextrakte, *ibid.*, 55, 1931, 3042-3052; H. Knaus, Die periodische Frucht- und Unfruchtbarkeit des Weibes, *ibid.*, 57, 1933, 1393-1408.

[3] C. Moir, Recording the contractions of the human pregnant and non-pregnant uterus, *Edinburgh Med. J. Trans. Obst. Soc.*, 93, 1934, 93-120.

[4] J. E. Lackner, L. Krohn, and S. Soskin, The etiology and treatment of primary dysmenorrhea, *Amer. J. Obst. & Gyn.*, 34, 1937, 248-265; F. H. Falls, J. E. Lackner, and L. Krohn, Effect of progestin and estrogenic substance on human uterine contractions, *J. Amer. Med. Assn.*, 106, 1936, 271-274; L. Krohn, J. E. Lackner, and S. Soskin, The effect of ovarian harmones on the human (nonpuerperal) uterus, *Amer. J. Obst. & Gyn.*, 34, 1937, 379-391.

[5] Ch. Matteucci, Sur le courant électrique des muscles des animaux vivants ou récemment tués, *Compt. rend. de l'acad. sci.*, 16, 1843, 197-214.

[6] H. Piper, Über den willkürlichen Muskeltetanus, *Arch. f. d. ges. Physiol.*, 119, 1907, 301-338.

[7] E. Jacobson, Electrical measurements of neuromuscular states during mental ac-

The first studies of electrical changes upon contraction in smooth muscle were made also in the laboratory of duBois Reymond, where Matteucci worked. Until the work of Fuchs (1910),[8] unstriped muscles were, however, generally supposed to be electrically inactive. Four years later, Foá noted a series of electrical waves indicating the discontinuous, tetanic kind of contraction in the pregnant uterus of the guinea pig.[9] He claimed that no electrical action-currents accompany certain spontaneous contractions. We challenge this claim of Foá since he used only a string galvanometer, the kind currently used in electrocardiography, which is not sufficiently sensitive to rule out the presence of low but significant action-potentials.

Uterine contraction-voltages have been studied in the guinea pig, cat, rat, and other mammals.[10] Most often the investigator has been interested in the electrical phenomena in their relation to the contraction of smooth-muscle fibers rather than as a means of recording the activity of the uterus considered as an organ. We shall not exhaustively review here the growing literature on electrical potentials in smooth muscle, including the uterus, but certain salient points should be mentioned. Authors who have registered both the electrical and mechanical phenomena during spontaneous activity agree in stressing the approximately simultaneous occurrence of the two processes (Orbeli and Brücke, 1910;[11] Alvarez and Mahoney, 1922;[12] Funke, 1922;[13] Richter, 1924;[14] Hasama, 1930;[15] Vozza, 1933[16]). When studying the electrical responses of smooth muscle in the uterus and other organs in cats, elicited upon stimulating the sympathetic nerves, however, Rosenblueth and co-workers (1933) distinguish an initial from a delayed response.[17] The initial response precedes the contraction, with a latency sometimes on the order of 1/10 sec.; the delayed response occurs during contraction, with a latency of the order of ca. 1/3-1/2 sec. In the uterus (pregnant) only the 'delayed' potential was identifiable. The authors believe that the initial potential is strictly comparable to the spike in the 'action current' of

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tivities: IV. Evidence of contraction of specific muscles during imagination, *Amer. J. Physiol.*, 95, 1930, 703-712.

[8] R. F. Fuchs, Die elektrischen Erscheinungen am glatten Muskel, *Arch. f. d. ges. Physiol.*, 136, 1910, 65-100.

[9] C. Foá, Elektrophysiologie der glatten Muskeln, *Zentbl. f. Physiol.*, (Ergänzhft.) 27, 1914, 319.

[10] The customary term is 'action-potentials.' We proposed instead "contraction-voltages' for the potentials in electrodes in muscle-tissue associated with contraction.

[11] L. Orbeli and E. T. von Brücke, (1) Beiträge zur Physiologie der autonom innervierten Muskulatur; (2) Die Aktionsströme der Uretermuskulatur während des Ablaufes spontaner Wellen, *Arch. f. d. ges. Physiol.*, 133, 1910, 341-364.

[12] W. C. Alvarez and L. J. Mahoney, Action currents in stomach and intestine, *Amer. J. Physiol.*, 58, 1922, 476-493.

[13] H. H. Funke, Courants d'action des muscles lisses. (1) Électrogastrogrammes et mécanogastrogrammes de Rana esculenta, *Arch. néerl. de physiol.*, 6, 1922, 198-220.

[14] C. P. Richter, Action currents from the stomach, *Amer. J. Physiol.*, 67, 1924, 612-633.

[15] Bun-ichi Hasama, Pharmakologische Studien über den bioelektrischen Strom am Uterus; zur Registrierung des normalen Aktionsstrombildes des Uterus, *Arch. f. exp. Path. u. Pharmak.*, 153, 1930, 129-139.

[16] F. Vozza, Richerche sull'elettrometrogramma, *Ann. di ostet. e gin.*, 55, 1933, 603-615.

[17] A. Rosenblueth, C. Leese, and E. Lambert, Electrical potentials in smooth muscle, *Amer. J. Physiol.*, 103, 1933, 659-680.

skeletal muscle which precedes contraction. Their failure to obtain it in the uterus is due, as they are inclined to think, to a voltage so small as not to be revealed by the apparatus employed. If so, we can assume that such initial potentials are included in the records to be presented here, because the apparatus which we have used is unique in its sensitiveness. We shall, however, make no attempt to distinguish possible 'initial' from 'delayed' potentials, for our interest is in the action of the uterus regarded as an organic whole.

According to Bozler (1938) the musculature of the uterus is, like that of the heart, a syncytium.[18] Using long thin strips of uterine muscle from guinea pigs, rabbits and cats, he demonstrates 'all-or-none' contractions following various electrical stimuli. In short, he takes issue with Rosenblueth and associates, for he finds that the electrical changes during a conducted response differ only quantitatively from those observed in nerve and striated muscle.

Aside from slight temporal differences, there is much evidence, as we have said, that the mechanical and electrical phenomena occur in parallel. Most particularly, Fletcher (1937) has found in the smooth muscle of a certain mollusk (the byssus of Mytilus edulis) proof that the parallelism there is quantitative.[19]

Contraction-voltages in the human uterus have been somewhat crudely studied by several investigators. The electrocardiograph was used by Theilhaber in a study of uterine hemorrhage in 1910.[20] One electrode was applied to the cervix; the other was inserted in the rectum up to the level of the fundus. He believed that he found many typical rises in the curve in younger women without abnormal bleeding. These rises were absent when the uterus had been extirpated. It seems safe to say, in the light of our own experience, that no electrode should be in the rectum and that some of the waves which Theilhaber recorded were probably picked up from the cervix, at least in part. Veit (1912) stated briefly that he obtained records of uterine contraction from the feet, but he does not indicate their character.[21] Bode (1931) also used the electrocardiograph,[22] but says nothing about electrodes; he was interested in the action of posterior pituitary hormone on the pregnant uterus. With the same type of apparatus, Falk and Nahon (1935) placed a curved silver rod in the uterine cavity, presumably touching the mucosa.[23] To test the theory that the non-pregnant human uterus shows spontaneous contractions, the authors undertook to secure electro-uterograms periodically throughout the menstrual cycle. They admit that the origin of the waves they secure is somewhat problematical. We agree; for an electrode in the uterine cavity exposed to chemical action of tissue-fluids may show potentials in consequence. In any event, silver is not a metal to use with any degree of safety in this respect.

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[18] E. Bozler, Electric stimulation and conduction of excitation in smooth muscle, *ibid.*, 122, 1938, 614-623.

[19] C. M. Fletcher, Relation between mechanical and electrical activity of molluscan unstriated muscle, *J. Physiol.*, 91, 1937, 172-185.

[20] A. Theilhaber, Insufficientia Uteri, atonia Uteri, hyperplasia Uteri und Uterusblutungen, *Monatsch. f. Geburtsh. u. Gyn.*, 31, 1910, 727-740.

[21] J. Veit, Das Elektrometrogramm, *Zentbl. f. Gyn.*, 36, 1912, 161-162.

[22] O. Bode, Die Wirkung des Hypophysenhinterlappenhormons auf den schwangeren Uterus im Elektrohysterogram, *ibid.*, 55, 1931, 2887.

[23] H. C. Falk and R. Nahon, Electro-uterography, *Amer. J. Obst. & Gyn.*, 30, 1935, 403-408.

Through the use of thermionic amplification, extremely sensitive methods for the study of uterine contraction voltages are now open to the physiological psychologist. Our attempts to make an introductory survey concerning the electrical and mechanical activity of the human non-pregnant uterus have been partially described; but it is believed that a résumé, along with a fuller account of other studies, may prove of interest.

Methods. In many of our experiments, contraction-voltages were recorded from the uterus and from the abdominal striated muscles simultaneously.

For this purpose, two string galvanometers were available. The string's tension was set at 3-4 mm. per mv. in order to secure a flat characteristic for frequencies up to 200 cycles at least. By means of a switchboard any one of the three amplifiers could be thrown in circuit with either of the galvanometers. These assemblies have been described previously by Jacobson,[24] but modifications as here employed have not. Amplifier-1 is of the resistance-capacity type, and was most often used in the study of the uterus because of its ability to record lower as well as higher frequencies. It lacks the voltage sensitivity of Amplifier-2 and of Amplifier-3. The latter two are a twin pair, transformer-coupled, capable of recording higher frequencies from about 30 to 4000 with a flat characteristic. At the maximum, we have employed them in the present study to secure an over-all sensitivity a little less than 1 cm. per μv, a.c. Accordingly it is possible to secure amplification about 1000 times that of the string galvanometer alone.

Electrodes should penetrate directly into the uterine tissue, if chance variations of heat are to be averted. We, therefore, use platinum-iridium wires with points sharpened for easy penetration as described previously by Jacobson.[25] The wire is pushed into the tissue of the cervix or fundus about 5 mm. up to a hilt of insulating rubber or bakelite. Concentric electrodes were employed in one study only; namely, on a virgin dog's uterus. We preferred platinum-wire electrodes in order to record more extensively from muscle fibers than would be possible with the concentric type.

Pneumatic records were procured by means of a balloon in the uterine cavity. The passing of air from within this balloon took place through a rubber tube connected with a modified Marey tambour. Movements of the tambour facing were conducted by means of an inelastic thread to an extremely delicate system.[26] In effect, the passage of small amounts of air into or out of the balloon was recorded by means of a lever moving freely so that its position made a shadow on the photographic record. By-pass tubes with stop-cock arrangements made it possible to inflate the balloon after it was in the uterus, using an air-syringe; while the pressure in the system could be tested with an oil-manometer. As a rule, when used most sensitively, the pressure was approximately equal to a column of water 95 cm. in height.

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[24] E. Jacobson, Electrical measurements of neuromuscular states during mental activities. I. Imagination of movement involving skeletal muscle, *Amer. J. Physiol.*, 91, 1930, 567-608; VII. Imagination, recollection and abstract thinking involving the speech musculature, *ibid.*, 97, 1931, 200-209.

[25] Jacobson, Measurement of the action-potentials in the peripheral nerves of man without anesthetic, *Proc. Soc. Exp. Biol. & Med.*, 30, 1933, 713-715.

[26] Cf. Jacobson, Electrical measurements of neuromuscular states during mental activities. IV. Evidence of contraction of specific muscles during imagination, *Amer. J. Physiol.*, 95, 1930, 703-712.

The patient lies on an extremely comfortable couch in a shielded room adjacent to the space which contains the galvanometers and most of the other instruments. Amplifiers and standardizing devices are concealed in the couch. Both the leads and the air-tubes pass from *S* to connections through the wall. In this manner *S* is generally kept relatively free from disturbance through movement and speech of the experimenters.

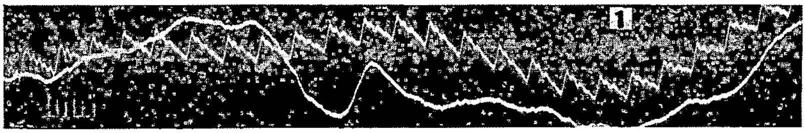

Fig. 1. (EKU-B) A Uterine Contraction-Cycle Recorded Electrically (Lower Curve on Right) and Mechanically

Microvoltage for the cycle ca. 300. The control test (input of amplifier short-circuited) is almost a straight line (not shown). Deflection is upward when the electrode in corpus tissue becomes electronegative; 1 cm. = 65 µv. Upward deflection of mechanical tracing denotes uterine contraction. Time-unit (left lower corner) = 0.2 sec.

Electrodes were inserted into the uterus in three types of location; (a) both in the corpus tissue; (b) one in the corpus and the other in the cervix tissue; and (c) both in the cervix, anterior or posterior lip. The corresponding records are called Electro-uterograms, Type A, B, and C respectively, and may be abbreviated as EKU-A, EKU-B, and EKU-C. For our purposes, insertion of both electrodes in the cervix is the most convenient and is attended by the least discomfort.

In order to record entirely within the corpus (EKU-A), a modified tracheal catheter is employed. The platinum-iridium wires contained within were insulated with bakelite varnish throughout their length, excepting at the tips. They were freely movable longitudinally, and could be made to protrude from the end of the catheter to a distance determinable upon a scale marked in mm. on the wire near a metal connection which served as a handle for the device.

In securing tracings of Type EKU-B, we employed either a similar device containing one wire only or else a metal catheter insulated externally, but with a central tube for the passage of one wire surrounded by a passageway for air currents leading to and from the bag which was inserted into the uterine cavity. The central wire could be withdrawn into the tube during the insertion, but could be protruded when the instrument came into place.

For tracings of Type EKU-C, no special device was needed. The two platinum-iridium wires were inserted directly into the exposed lips of the cervix.

Results. Twenty-nine *Ss* were available for the present study, of whom 15 were free from noteworthy pathology. The account contained in the present report will be limited chiefly to the findings in the relatively normal women.

Marked uterine contractions are recorded mechanically and electrically in Fig. 1. What appears to be a complete cycle of contraction is shown. The duration in this instance is approximately 20 sec.; though the time varies widely, even in successive contractions. This *S* was a multipara of 24 yr., who complained of 'nervousness' prior to menstrual periods. She mentioned also occasional pain in the lower portion of the abdomen bilaterally passing to the legs intermittently since her last delivery. Her uterus was movable, but retroverted and was held in proper position by a pessary. This recording was made about three weeks after her previous period and two weeks before the following. Tracings made on the same occasion from the skeletal muscles of the abdomen near the mid-line changed from a level of about 1 μv. at first (maximal peak; *i.e.* the voltage of the maximal action-potential recorded per unit of time, 0.2 sec.), then to 6, and later to 12 during a recording period of about 12 min. It might be concluded that this *S* was in a state of neuromuscular hypertension during this period,[27] which coincided with her complaint of premenstrual nervousness and, to a certain extent, objectively confirmed that complaint.

Measurements of the states of activity in skeletal muscles, denoting whether the individual appears to be in a hypertensive or in a relatively normal condition at the time, are made in μv. However, it is well to emphasize that there is no hard and fast line between the normal and the nervous hypertensive patient expressible in these units. Indeed, the actual microvoltage determined in a particular muscle at a particular moment will vary with the type of amplifier as well as with the character, position, and dimensions of the electrodes employed. Under such variable conditions, prolonged experience with the amplifier and the electrode-assembly employed is pre-requisite for reliable interpretation.

Less marked uterine contractions are traced electrically in Fig. 2a. This *S* likewise complained of 'nervousness' prior to menstrual periods. At times for three years there had been also some pain in the bladder and lower portion of the abdomen. She was a multipara of 45 yr. with irregular menses. The last period had ended six weeks previously. The left ovary and tube, as well as part of the cervix, had been removed in 1930. A small fibroid tumor was palpated in the right cornu of her uterus.

This case affords another opportunity to determine objectively the nervous state of the patient—at least at the time of the test. Fig. 2a affords a brief example of the sort of moderate contraction-voltages recorded at irregular intervals, often less than 1 sec. Fig. 2b clearly shows that when a separate set of electrodes is inserted into the (skeletal) abdominal muscle, a record is secured of contraction-voltages, which is independent of the uterine record (2a). The uterine voltages shown in Fig. 2a are not very high. Similarly the records of the abdominal neuromuscular state show no excessive voltages such as one of us has shown commonly can be recorded during highly nervous states.[28] Our *S* contracts and relaxes the abdominal muscles promptly upon signal, a performance which is not characteristic of individuals at the time of high emotion. Accordingly we find in this instance no objective record of nervous hypertension, either in the abdominal skeletal muscles or in the uterus. Such negative findings suggest the doubt that this patient was really in a state of 'premenstrual nervousness' at the time of the study.

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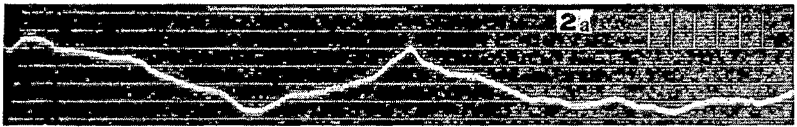
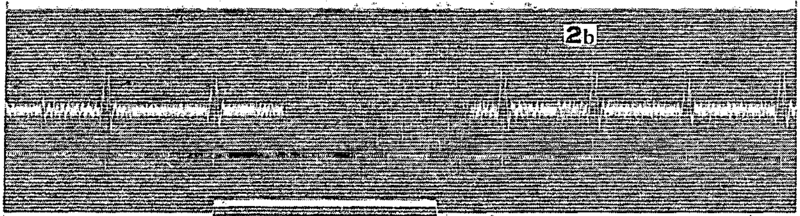
[27] Jacobson, *Progressive Relaxation,* (rev. ed.), 1938, 6-27, 309-326.

[28] Jacobson, Electrical measurements concerning muscular contraction (tonus) and the cultivation of relation in man Studies on arm flexors, *Amer. J. Physiol.,* 107, 1934, 230-248.





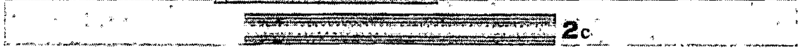


FIG. 2a. (EKU-B) Moderate contraction-voltages. Signal-line at top of record marks patient pulling in abdominal muscles as requested. No great influence on uterine contraction-potentials is shown; but when later repeated, continuously for 6 sec., the electrical deflections showed increased amplitude, lasting some seconds after the abdominal muscle-contractions ceased. The short-circuit recording is an almost straight line (not shown). 1 cm. = 75 μv. (Deflection is upward when the electrode in corpus tissue becomes electropositive.)

FIG. 2b. Record of contraction-voltages in two electrodes inserted in abdominal muscles several cm. apart, about 5-6 cm. above pubic hair. Note the regularly recurring electrocardiogram. Note also that otherwise the abdominal muscles show low contraction-voltages, except upon signal (bottom of record). While *S* contracts these muscles upon request, contraction-voltages increase, corresponding in time with the signal-mark in Fig. 2a. *S* relaxes promptly upon a second signal-sound (end of the signal-line). 3.5 mm. = 1 μv.

FIG. 2c. Short-circuit control test. Compare with Fig. 2b.

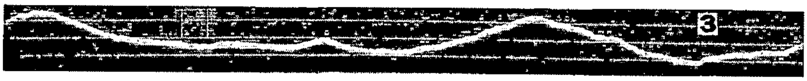
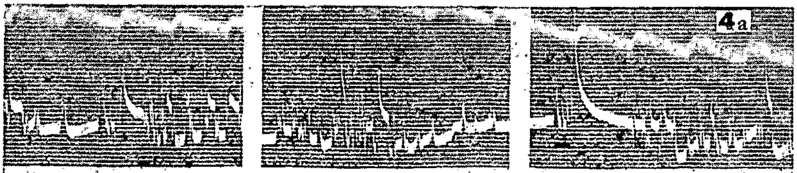


FIG. 3. (EKU-C) Records similar to EKU-B, except that the contraction-voltages are lower. 1 cm. = 75 μv. Time lines = 0.2 sec.

From the same *S* a record is presented (Fig. 3) to illustrate that contraction-voltages can be recorded most conveniently with both electrodes in the lips of the cervix. Curves sometimes resemble those secured with one electrode in the body of the uterus and one in the cervix (Type EKU-B). The voltages, however, are lower in the case of Type EKU-C.

Fig. 4 presents records from a fourth *S* who, regarded from a clinical standpoint, was obviously of 'nervous' type. Her age was 38 yr., she had borne two children, and had suffered one miscarriage. The record was made on the 21st day of the menstrual cycle (a 28-day rhythm). *S* complained of pain in the lower abdomen and of vaginal discharge; the uterus was retroflexed and slightly enlarged, and there was a 'second-degree' prolapse.

The electrical recordings of Fig. 4a indicate marked contractions in the uterine



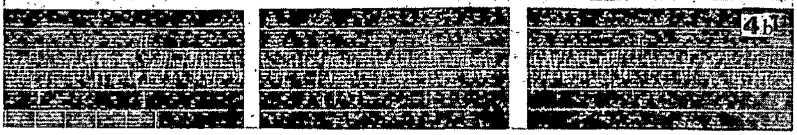
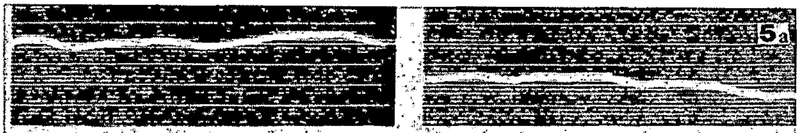
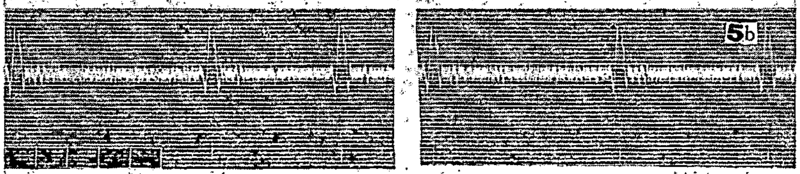


FIG. 4a. The upper tracing is a mechanical record, the lower the EKU-B in a 'nervous' *S*. Each vertical interspace indicates a lapse of ca. 18 sec. The entire record represents ca. 1 min. In the mechanical tracing appear rhythmic variations due to arterial pulsation. Pressure in the air system is ca. 95 cm. of water. Upward deflection signifies uterine contractions (notable in the middle part of record). The electrical tracing, like the mechanical, indicates marked uterine contractions. 1 cm. = 100 µv. The control test (short-circuiting the input terminals) is approx. a straight line, indicating that the results are not artifacts.

FIG. 4b. Contraction-voltages in abdominal skeletal muscles, occurring approx. simultaneously with the tracings shown in Fig. 4a. 3.5 mm. = 1 µv. Electrocardiograms barely discernible. Short-circuit control test similar to Fig. 2c.





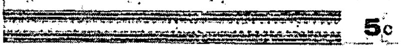


FIG. 5a. (EKU-B) In the 17-sec. interval between the two photographs (white space), the negative potential in the electrode in the fundus declines about 20 µv.(= 1 cm.). Shallow wavelets are superimposed ca. ½ — 2 per sec. The uterus shows a low or moderate activity.

FIG. 5b. Electrodes in abdominal wall, 4 cm. below umbilical level, each about 2.5 cm. from midline. Six complexes of cardiac origin appear. Between these complexes the string tracing is fairly quiet. Comparing such intervening sections with Fig. 5c, the abdominal muscles here recorded are almost completely relaxed (cf. Fig. 5c). Records 5a and 5b are taken at approximately the same time.

FIG. 5c. Control test, showing slight fluctuation of the string tracing when a short-circuit exists across the input of the amplifier. 1 µv, a.c. = 7 mm.

musculature.[29] A similar interpretation can be made from the mechanical (upper) tracings. At the same time, contraction-potentials from the abdominal musculature (Fig. 4b) indicate that this *S*, for the moment at least, was in a somewhat tense state.

By way of a contrasting illustration, recordings from another *S* are shown in Fig. 5 *S* (age 25 yr.) ended her first pregnancy six weeks previously and had been nursing her child. The uterus was undergoing normal involution. When the electrodes were inserted in the cervix, the EKU-C illustrated continual tonic contraction (not here shown). (Tonus in the intact human uterus had not been electrically recorded prior to the present studies.)

Further records from the same *S* illustrated that at times, at least, tracings of Type EKU-C are similar in general character to those secured by the more difficult methods, EKU-A and EKU-B.

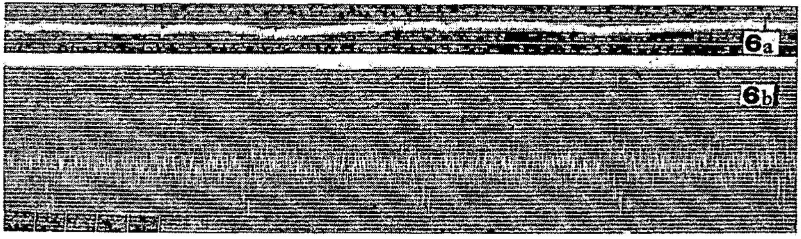


FIG. 6a. (EKU-B) Connections and *S* same as for Fig. 2a.

FIG. 6b. Connections and *S* same as for Fig. 2b. Electrodes in the abdominal muscles evince a peak microvoltage of about 6 or 7 μv. between heart-beats, whereas in Fig. 2b their microvoltage was approximately zero. The uterine musculature as recorded in Fig. 6a, however, is more quiet than in Fig. 2a.

Fig. 5 illustrates a slight or moderate degree of uterine contraction, as electrically recorded (5a), coincident with approximately complete abdominal muscle relaxation (5b). With right fist vigorously and persistently clenched for some seconds, contraction-potentials increased markedly in the abdominal musculature. *S* failed to relax upon signal, but rather became more tense, illustrating something found by one of us in previous studies; namely, that many persons require training to relax before they are prepared to do so effectively upon signal or on their own intent.[30] During one period of such failure to relax tensions in the abdominal skeletal muscles, contraction-potentials in the uterine musculature increased at first but later proceeded to decrease, although the abdominal tension was still increasing. That the uterus can on occasion become relatively quiet even while the skeletal abdominal muscles are relatively tense is illustrated in Fig. 6, from this same *S*.

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[29] Artifacts in the tracings of Type EKU-A and EKU-B cannot be ruled out with certainty since it is impossible to determine with complete assurance that an electrode in the corpus has not slipped out at any moment. In this respect, tracings of EKU-C are more dependable, since it is possible at the end of the experiment to determine the location of the electrodes by direct inspection.

[30] Jacobson, Electrical measurements concerning muscular contraction (tonus) and the cultivation of relaxation in man; Relaxation-times of individuals, *Amer. J. Physiol.*, 108, 1934, 573-580.

If the figures presented above indicate that uterine contraction-potentials proceed with rhythm and magnitudes not predictable from the examination of skeletal muscles alone, *e.g.* the abdominal muscles, we are not warranted in drawing the conclusion that the magnitude of abdominal muscle-contraction fails to influence that of the uterine musculature. A careful study of the relationship between such magnitudes in the smooth and skeletal muscles involved would require a systematic study of this problem alone and would require also the use of *S*s trained to relax previously or possessed of such natural ability. In the absence of thorough-going studies, we leave open the question of the influence of the abdominal-musculature tension upon uterine contraction. In some of our studies it seemed evident that the induction of persistent abdominal tension tended to induce a simultaneous increase in uterine contraction, while, in other instances, no such relationship was clearly observed.

Significant is the fact that it is now possible to study objectively the nervous patient with pelvic symptoms, since electrical tracings can now be made simultaneously from the skeletal and the uterine musculature.[31]

SUMMARY

(1) A method is set forth for a study of the activity, in pathological and in normal states, of the intact uterus *in situ*.

(2) An additional procedure has been described for the measurement of skeletal neuromuscular states. A combination of these methods makes it possible to secure an objective record of the 'tense' or 'nervous' state of an individual during a particular test in conjunction with a synchronous record of the electrical and mechanical activity of the uterus.

(3) Uterine tonus, registered during the present investigation for the first time in the intact organ, varies considerably in magnitude from time to time and from *S* to *S*. Whether the uterus ever becomes entirely free from contraction could not here be determined because we had no *S*s trained to relax.

(4) Spontaneous contractions were recorded repeatedly from the uterus, even with no bag in the fundus. Accordingly, the objection sometimes raised to conclusions from previous investigations that spontaneous contractions were characteristically due to the presence of a foreign body is not confirmed. We cannot, however, confirm or deny Moir's statement that the normal uterus always shows spontaneous contractions. This problem would require a larger series of cases, employing at least a number of individuals previously trained to relax.

(5) The uterus was recorded as active or quiet whether or not the ab-

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[31] Cf. E. Jacobson, J. E. Lackner, and M. B. Sinykin, Electrical and mechanical activity of the human non-pregnant uterus, *Amer. J. Obst. & Gyn.*, 38, 1939, 1008-1021.

dominal neuromusculature was relatively active or quiet. Although four combinations were found to occur, we discovered no conclusive evidence against the plausible view that conditions under which abdominal musculature is tense tend to promote uterine activity; what evidence we have on this subject would tend to favor this view. It is certain, however, that recorded tensions in the abdominal and uterine muscles do not always vary in correspondence.

STUDIES ON CONTOUR: STROBOSTEREOSCOPIC PHENOMENA

By HEINZ WERNER, Wayne County Training School

It has been demonstrated in an earlier study that under certain circumstances a solid form will not be seen if its presentation is immediately followed by that of a second figure.[1] In the demonstration of this phenomenon, two visual figures were used: an 'inner figure,' a solid black disk (15 mm. in diameter) on a white background; and a 'frame figure,' a black ring (inner diameter 15 mm.) on a white background. The black disk will not be seen providing (a) that the two figures are observed repeatedly in succession; and (b) that they are presented at an optimal rate of succession, *i.e.* the intervals between figures and between the pairs of figures are of a certain optimal length.

Since it is of theoretical significance whether this and allied phenomena can be obtained during binocular interaction, a strobostereoscopic experiment has been devised.

METHOD AND PROCEDURE

The strobostereoscope,[2] used previously in studying binocular depth perception, was adapted for the present study. The apparatus permits the projection of each figure to one eye only; for example, the ring to the left and the disk to the right eye.

RESULTS

Table I shows the dependence of the disappearance of the disk upon rate of succession. In this experiment the disk appeared always as the first member of the pair. The figures in the columns indicate, for each rate of succession, the percentages of statements of complete disappearance of the disk. The table indicates that the phenomenon of the disappearance of the disk—previously observed under ordinary visual conditions—continues to occur even when each eye is receiving only one of the alternating stimuli. It should be noted that the optimum rate of succession is different for different *O*s.

If the order of the succession is reversed so that the ring appears as the first member of the pair, the disk does not disappear within the limits of speed mentioned above. If black and white are reversed in the figures

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* Accepted for publication December 14, 1939.

[1] Heinz Werner, Studies on contour: Qualitative analysis, this JOURNAL, 47, 1935, 40-64.

[2] Werner, Dynamics in binocular depth perception, *Psychol. Monog.*, 48, 1937, (no. 218), 1-127.

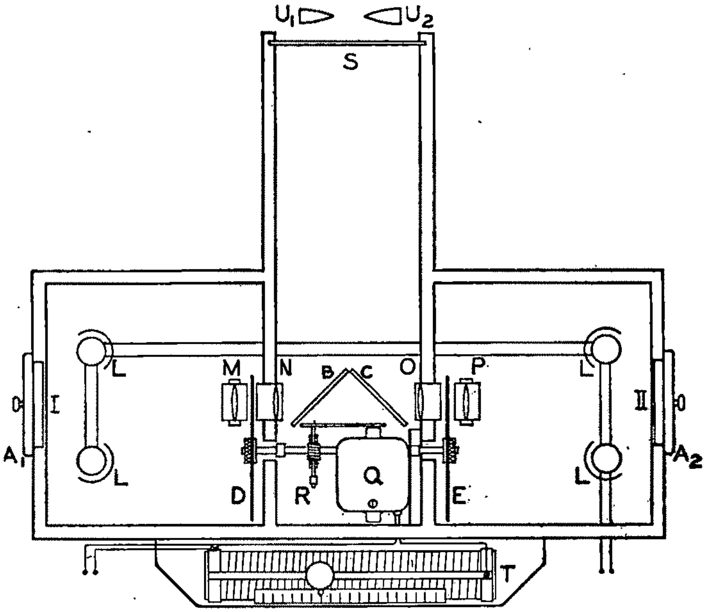

FIG. 1. STROBOSTEREOSCOPE

The Pictures I and II, held in the frames A_1 and A_2, after receiving illumination from the lamps (L), are projected through the lenses (M, N, and O, P) and are reflected from the mirrors (B and C) to the opaque glass (S). The projections are interrupted by episcotister disks (D and E). The disks are operated by means of motor (Q) and gear (R). The speed of rotation is varied by the rheostat (T). *O* looks through the curved prisms (U_1, U_2) at the opaque glass where the images appear.

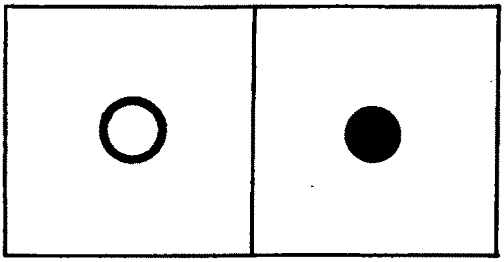

FIG. 2. FORMS USED IN THE STROBOSTEROSCOPIC EXPERIMENT

In the course of each trial the pair of figures is presented several times. On each square of opaque glass where the images will appear, is a tiny dot which serves as a fixation-point. The *Os* were trained to keep the two points constantly fused during every trial. Four *Os* took part in the experiment. There were 20 trials for each series.

the phenomenon is not altered. At the optimum rate the white disk becomes invisible, the white ring alone appearing at regular intervals.

The phenomenon of disappearance cannot be observed if the disk and the inner diameter of the ring are relatively small with respect to the width of the ring. If, for instance, the diameter of the disk is 2 mm, and the inner and outer diameter of the ring 2 and 15 mm respectively, the disk will remain visible.

TABLE I

PERCENTAGE OF DISAPPEARANCE OF DISK RELATIVE TO RATE OF SUCCESSION AND TIME OF ROTATION

O	Rate of succession (in ms.)* and time of rotation (in sec.)†								
	50	75	100	125	150	175	200	225	250
	0.2	0.3	0.4	0.5	0.6	0.7	0.8	0.9	1.0
1	0	20	70	70	80	100	40	10	0
2	0	0	25	70	70	55	50	0	0
3	0	0	10	50	100	80	60	50	0
4	0	0	0	40	70	70	100	40	20

* Interval between disk and ring. † Interval between disk and disk.

TABLE II

THE OPTIMAL EXPOSURE-TIME (IN MILLISECONDS) UNDER STROBOSTEREOSCOPIC AND ORDINARY BINOCULAR CONDITIONS

O	Optimal exposure-time (in ms.) under	
	strobostereoscopic conditions	binocular conditions
1	175	150
2	125–150	125
3	150	125
4	200	180

A comparison of the optimal rate of succession under the strobostereoscopic conditions of the present experiment and under ordinary binocular conditions is given in Table II.

The optimum rate of succession at which the disk disappears is greater for all *O*s under strobostereoscopical conditions than in ordinary binocular vision.

DISCUSSION

The present study has demonstrated that the disappearance of a disk succeeded by a ring can be observed whether both figures are seen binocularly, or one figure is presented to the right, the other to the left eye. The facts brought out in this study seem to have significance for the general theory of interaction within the visual field.

About the time the first study on contour was published, two papers on apparently allied phenomena appeared. Fry exposed in a dark visual field a small bright white area *b*; after a lapse of time two adjacent areas *a* and *a′* were exposed. He determined the duration of exposure of *a* and *a′* necessary to produce a sensation of brightness equal to that produced by *b*.[3] Keeping the duration of exposure of *b* constant at 4 ms. he found that the time values of *a* and *a′* were considerably shorter than 4 ms., varying with the interval between the two exposures as follows.

Interval (in ms.)	7.5	15	25	50	75	100	125
Duration of *a* and *a′* (in ms.)	2.6	1.8	1.3	0.8	0.4	0.2	0.15

By this and other experiments Fry proved that the second flash has a depressive effect upon the first. In interpreting his results, Fry states, "What seems to happen is that the response of the retina to the first stimulus is considerably delayed and prolonged and overlaps in time the response to the second stimulus and is inhibited by it by some kind of interaction between retinocortical pathways at synapses either at the retina, or at the basal ganglia or at the cortex."[4] Piéron[5] published several experiments with forms similar to those which we used in Experiments 15 and 16 of the first study,[6] obtaining essentially the same effect as we have described. An oblong figure followed by two adjacent bands was more or less inhibited. Fry does not attempt to decide whether the interaction occurs at the peripheral or central level, whereas Piéron is inclined to interpret the inhibition as an interaction at the peripheral level, that is, in the retina.

Some of the facts published in our first study suggest, however, that higher integrating centers are involved in producing the phenomenon. It may be recalled that *O*'s attitude played an important rôle. The *O*s were able to enhance or to reduce the phenomenon intentionally. Most pertinent to the present question is Experiment 21 with ambiguous frame figures.[7] If the frame figure is perceived so that the inner figure fits into it, the latter will disappear. If *O* perceives the frame figure in such a way that no configurational relation exists between it and the inner figure, the latter continues to be seen. These results indicate that all the facts cannot be explained on the basis of Piéron's theory. They suggest that the phenomenon is not based upon any point-by-point stimulation and interaction at the retinal level. Perceptual processes on a higher level of integration (*i.e.* contour processes) obviously play a fundamental rôle.

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[3] G. A. Fry, Depression of the activity aroused by a flash of light by applying a second flash immediately afterwards to adjacent areas of the retina, *Amer. J. Physiol.*, 108, 1934, 701-707.

[4] *Op. cit.*, 706.

[5] Henri Piéron, Le processus du métacontraste, *J. Psychol. Normale Pathol.*, 31, 1935, 5-24.

[6] *Op. cit.*, this JOURNAL, 51 ff.

[7] *Ibid.*, 55 ff.

The results of the present study supply further evidence in support of this view. They demonstrate that the primary interaction cannot occur peripherally, since the strobostereoscopic phenomenon is based on interaction between different stimuli presented to the two retinal fields. Of course, the question could be raised whether one is not dealing here with a primitive phenomenon of binocular rivalry, of suppression of the activity of one eye in favor of the other.

It is true, as we have frequently observed, that binocular rivalry does occur not only under simultaneous stimulation but also under alternate stimulation of the two eyes. There are several facts, however, which seem to exclude the interpretation of the phenomenon in terms of binocular rivalry.

(1) If the ring succeeds the disk, rivalry may occur, but only at a rate of succession much greater than the optimum rate. Under these circumstances the inner figure is frequently visible.

(2) If suppression of one image in terms of binocular rivalry is the cause of our phenomenon, the order of presentation within the pair should have no effect on its occurrence. Actually, however, if the succession is reversed, that is, if the ring precedes the disk, the latter, as a rule, remains visible at the optimum rate.

(3) Again, suppression by rivalry does not explain why the disk does not vanish when its diameter is very small in relation to the width of the ring. On the contrary, one should expect that the disappearance of the disk would be even more easily and firmly established when the discrepancy between the two figures is greater.

(4) Finally, the *O*s frequently observe that the ring expands when it appears alone. This observation, previously noted under ordinary binocular conditions, seems to confirm our assumption that *O*'s response to the stimulus preceding the ring, though not on the level of form perception, is dynamically involved in the organization of the ring.

SUMMARY

A black disk and a black ring of the same inner diameter as the disk are projected strobostereoscopically, one to the left and one to the right eye. At an optimum rate of succession the disk disappears. This experimental observation supports the view that the phenomenon of depression or inhibition described in the first study on contour, does not originate at the retinal level but is the result of a perceptual process at a higher level of integration.

DISTRIBUTION OF EFFORT IN MEMORIZING PROSE AND POETRY

By ARTHUR P. BUMSTEAD, Berkeley, California

More than fifty years ago Ebbinghaus discovered that less effort was required for memorizing when units were small and when learning was spread or distributed. Though these results with nonsense syllables have since been confirmed by many investigators, there is no general agreement among psychologists regarding the most economical way of memorizing poetry or prose. Some psychologists contend that the greatest economy is secured when a passage to be memorized is broken into parts and learned by repeated readings over long intervals. They hold to this view because the actual learning or study-time in hours and minutes is short. Other psychologists, contrariwise, regard that method as wasteful because of the long time elapsing between the first and last reading.

TABLE I

MILTON'S *Paradise Lost:* NUMBER OF READINGS, STUDY TIME, AND ELAPSED-TIME REQUIRED FOR LEARNING SELECTIONS OF DIFFERENT LENGTHS AT VARIOUS TIME-INTERVALS BETWEEN SINGLE READINGS

Measure	Length of selection in lines	Intervals between readings									
		0	1 hr.	2 hr.	4 hr.	8 hr.	12 hr.	1 d.	2 d.	4 d.	7–8 d.
No. of readings	350	—	—	—	—	—	—	—	—	—	16
	50	49	43	34	28	24	—	19	16	15	14
	25	29	—	—	—	—	21	—	17	—	—
	5	24	18	—	—	—	13	—	8.5	—	7
	1	—	5.3	—	—	—	—	—	4.5	—	3.5
Study-time (in min.)	350	—	—	—	—	—	—	—	—	—	400
	50	170	140	115	95	75	—	60	48	50	46
	25	47	—	—	—	—	29	—	23	—	—
	5	6	4	—	—	—	3	—	2	—	2
	1	—	1	—	—	—	—	—	1	—	1
Elapsed-time (in days)	350	—	—	—	—	—	—	—	—	—	112
	50	1	4	6	7	8	—	19	32	60	98
	25	1	—	—	—	—	11	—	34	—	—
	5	1	2	—	—	—	7	—	17	—	49
	1	—	1	—	—	—	—	—	9	—	25

How are study-time and elapsed-time affected by changes (1) in the length of the intervals between readings and (2) in the size of the units learned? In order to answer these questions specifically, the writer performed upon himself the experiments reported here.

About 1000 lines from Milton's *Paradise Lost* were memorized at various times between 1915 and 1935, and about 1400 lines from the *Bible* during the years following. The results are shown in Tables I and II.

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* Accepted for publication September 16, 1939.

Learning of a typical selection from *Paradise Lost* will be described in detail. One day *S* read a 50-line passage from beginning to end at his usual rate. He repeated the process every day until he could recite the selection to himself without referring to the text. After a few readings he could recall an isolated line here and there, and after a few more he could repeat sections up to 10 or 15 lines in length. He read the entire passage through every day, however, until he could recite the entire 50 lines. Table I indicates that the time spent in actual study of the text was 60 min. As there were 19 readings spaced one day apart the elapsed-time was 19 days.

Nine 50-line selections were memorized. The lengths of the intervals between single readings were variable, ranging from 0 to 8 days, but all other factors were held fairly constant. There was a pronounced tendency for decreased study-time and increased elapsed-time to be associated with decreases in the intervals between readings. For instance, 43 readings spaced 1-hr. apart (in the daytime) were needed for learning one passage. The study-time was 140 min. and the elapsed-time was 4 days. When a piece of the same length was learned by readings spaced 8 days apart the study-time was reduced from 140 min. to 46 min.; and the elapsed-time was increased from 4 days to 98 days. Similar results were obtained when the selections were 1 line, 5 lines, and 25 lines in length.

TABLE II

THE *Bible*: NUMBER OF READINGS, STUDY TIME, AND ELAPSED-TIME REQUIRED FOR LEARNING SELECTIONS OF DIFFERENT LENGTHS AT VARIOUS TIME-INTERVALS BETWEEN SINGLE READINGS

Measure	Length of selection in lines	Intervals between readings	
		1 hr.	7 d.
No. of readings	1200	—	50
	200	30	10
Study-time (in min.)	1200	—	1085
	200	240	60
Elapsed-time (in days)	1200	—	350
	200	3	70

Selections from the *Bible* (Table II) were memorized by a slightly different procedure. The first 14 chapters of *Job*, comprising 1200 lines were memorized in 50 study-periods spaced 1 week apart. At the first period the chapters were read through once, the operation taking 35 min. After 10 readings, one at each period, the first two chapters, containing 200 lines, were written down on paper from memory; they were omitted from subsequent readings. After 9 more readings the third and fourth chapters, also containing 200 lines, were committed to memory; they too were recorded and omitted from later readings. As time went on more and more chapters or substantial parts of chapters were written down, and the learning periods became correspondingly shorter. The 50th period, which was the final one, was only 4 min. long.

The story of Samson and Delilah as related in the 16th chapter of *Judges* was memorized as a result of 30 readings spaced at 1-hr. intervals. After 5 readings it was noted that one of the 31 verses could be recalled; after the 10th reading 5 verses, and after the 20th reading 17 verses could be repeated. After the 30th complete reading the whole chapter could be recited, and it was written down.

For learning 200 lines from *Job* by readings spaced one week apart the study-time was 1 hr., and the elapsed time was 70 days. For learning a comparable passage from *Judges* by readings spaced 1 hr. apart the study-time was 4 hr., and the elapsed-time was 3 days.

It must be pointed out that lines from the *Bible* and *Paradise Lost* are not comparable in length nor in difficulty.

If a selection were divided into parts which were learned consecutively, how would study-time and elapsed-time be affected? We saw in Table I that 23 min. of study-time were needed for learning a 25-line selection by readings spaced 2 days apart, and the elapsed-time was 34 days. It seemed reasonable to suppose that a second passage of the same length could be memorized by the same method in the next 34 days, and that the study-time would again be approximately 23 min. Then the study-time and elapsed-time for learning 50 lines divided into two parts of 25 lines would be respectively 46 min. and 68 days, comparing with 48 min. of study-time and 32 days of elapsed-time required when a 50-line selection was learned as a whole by readings every second day. Similarly, if a 50-line passage were divided into 10 parts of 5 lines which were learned consecutively the study-time would be reduced to 20 min. approximately, while the elapsed-time would be almost 6 mo.

When the experimenter had used the data in Tables I and II for making the computations in Table III he reached the conclusion that when a selection is divided into parts which are memorized consecutively, the smaller the parts the shorter will be the study-time and the longer will be the elapsed-time required for the learning.

If a selection is divided into parts which are memorized concurrently rather than consecutively, how are study-time and elapsed-time affected? In order to answer this question the writer memorized three more 200-line narratives from the *Bible*. *Shadrach and Company in the Burning Fiery Furnace* was divided into 2 parts of 100 lines each; *Samson's Riddle* was divided into 4 parts of 50 lines each; and *Belshazzar's Feast, or the Handwriting on the Wall,* was divided into 8 parts of 25 lines. There was always an interval of 1 hr. between any two readings of each part, and learning of all the parts of each narrative was under way at the same time.

Part A of the first of these narratives was read once an hour on the hour until it had been memorized and written on paper. Part B was learned concurrently, but the readings were on the half-hour. No part was read again after it had been recorded. The actual study-time for this narrative was about 2 hr. 25 min. The elapsed-time, disregarding periods devoted to sleep at night, was 24 hr. It is estimated that if the parts had been learned consecutively the elapsed-time would have been 48 hr., and the study-time would have been about 2 hr. 25 min. as before.

For the narrative divided into 4 parts, the study-time was 2 hr., and the elapsed-time was 20 hr. When the narrative was divided into 8 parts, the study-time was 1 hr. 25 min., and the elapsed-time was 15 hr. It will be recalled that

TABLE III

COMPARISON OF ACTUAL TIME REQUIRED FOR MEMORIZING PASSAGES AS A WHOLE WITH ESTIMATED TIME REQUIRED FOR MEMORIZING BY PARTS LEARNED CONSECUTIVELY

Interval between readings	Lines	Study-time (in min.)	Elapsed-time (in days)
0	50	170	1
	25	47	1
	2×25 (est.)	94	2
	5	6	1
	10× 5 (est.)	60	10
2 days	50	48	32
	25	23	34
	2×25 (est.)	46	68
	5	2	17
	10× 5 (est.)	20	170
7-8 days	50	46	98
	5	2	49
	10× 5 (est.)	20	490
7–8 days	350	400	112
	50	46	98
	7×50 (est.)	322	686
	5	2	49
	70× 5 (est.)	140	3430

when the story of Samson and Delilah was memorized as a whole by hourly readings, the study-time was 4 hr., and the elapsed-time was 30 hr.

Comparative results obtained from learning 200-line passages by various methods are summarized in Table IV, together with estimates of the time which would have been required if the parts had been learned consecutively.

TABLE IV

COMPARATIVE RESULTS OBTAINED BY LEARNING 200-LINE NARRATIVES AS A WHOLE; BY PARTS CONCURRENTLY; AND ESTIMATED, BY PARTS CONSECUTIVELY

(Each part was read at hourly intervals)

Method of Learning	Study-time (in hr.)	Elapsed-time (in hr.)
Learning as a whole (200 lines)	4.0	30.0
Learning in 2 parts of 100 lines, concurrently	2.4	24.0
Learning in 4 parts of 50 lines, concurrently	2.0	20.0
Learning in 8 parts of 25 lines, concurrently	1.4	15.0
Learning in 2 parts of 100 lines consecutively (est.)	2.4	48.0
Learning in 4 parts of 50 lines consecutively (est.)	2.0	60.0
Learning in 8 parts of 25 lines, consecutively (est.)	1.4	120.0

Table III may be interpreted to mean that when a passage is divided into parts which are memorized concurrently the smaller the parts the shorter will be the study-time and the shorter will be the elapsed-time.

Between 1915 and 1939 the writer memorized about 3000 lines from *Paradise Lost* and the *Bible*. The selections ranged in length from 1 line to 1200 lines; the intervals between single readings varied from 0 to 8 days; the study-periods ranged in length from a few seconds to 35 min.; memorizing was by wholes, by parts learned consecutively, and by parts learned concurrently. He arrived at the following conclusions regarding his own memorizing when selections are learned by the method of repeated readings.

(1) The longer the interval between readings, the shorter the study-time and the longer the elapsed-time.

(2) When a passage is divided into parts which are learned consecutively, the smaller the parts the shorter the study-time and the longer the elapsed-time.

(3) When a passage is divided into parts which are learned concurrently, the smaller the parts the shorter the study-time and the shorter the elapsed-time.

ALTERNATIVE INTERPRETATIONS OF RESULTS OBTAINED ON A VARIATION OF THE PETERSON MENTAL MAZE

By C. W. CRANNELL, University of California

In most learning experiments the experimenter (*E*) sets a criterion of 'good' behavior on the part of his *Ss* and scores all activity which deviates from this criterion as 'bad.' To be sure, *E* rarely, if ever, uses such terms as 'good' and 'bad,' in describing the behavior of his *Ss*, but speaks instead of 'correct choices,' 'errors,' etc., defining these as objectively as possible. Such objective definitions arbitrarily make 'errors' out of all behavior which does not lead in a manner prescribed by *E* to a condition in which *S*'s responses coincide with the criterion set by *E*.

In the vast majority of animal experiments the situation is arranged so that trying out different responses is 'bad,' while sticking to those which have proved successful in a single instance is 'good.' For example, if a rat has avoided a given blind in a maze on one trial and enters it on the next, this response is scored as an error. It may be, of course, necessary to do so for the sake of an objective record. *E* should not forget, however, that he is scoring two very divergent types of behavior as if they were the same thing: (1) a 'bad error,' failure to retain the 'correct' response at that part of the maze; (2) a 'good error,' an attempt to discover a better way out of the maze.

It may be possible to obtain some knowledge of the presence of such differences and of their importance as factors in learning by studying set-ups in which doing different things is 'good' from *E*'s point of view. The experiment here reported seems to the writer to illustrate how *E*'s methods of treating the data and his establishment of a particular kind of criterion of performance may determine the results he reports.

The recent introduction of Woodworth's *Experimental Psychology*,[1] which contains a very complete account of Peterson's mental maze experiment,[2] as a text in courses at the University of California has made it advisable to devise a variation of the mental maze which could be used in the laboratory on *Ss* with knowledge of the original form. It was decided to use alternate forms of the maze, with ten pairs of letters in each. In Maze I the pairs were P–Z, G–L, R–Q, A–V, B–X, T–E, H–N, D–U, Y–F, and J–M, with the first letter in each pair as written above correct. In Maze II there were the same pairs of letters in the same order, but with the second letter in each pair correct, making a sort of 'mirror image' of the first maze.

The lists of letters were written on the opposite sides of cards which were furnished to the student who served as *E*. The *Ss*, other students, were given mimeographed instructions copied verbatim from Peterson's article and sent from the room to read them, while the *Es* were instructed as to the operation of the experiment.

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* Accepted for publication January 5, 1940. This study was directed by Professor Warner Brown.

[1] R. S. Woodworth, *Experimental Psychology*, 1938, 153 f.

[2] J. Peterson, Learning when frequency and recency factors are negative, *J. Exper. Psychol.*, 5, 1922, 270-300.

E's instructions. The following instructions were given *E*. "You will begin with Maze I and present the first pair of letters, P–Z. Write down the letters in the order presented, P–Z if the correct letter is given first, and Z–P if the wrong one is given first. Correct or wrong first is to be determined by whether you draw a red card (correct first), or a black card (wrong first) from the deck which has been given you. Underline *S*'s response. If the response is Z, which is wrong, repeat the letters P–Z in the order as determined by drawing another card, and continue to do so until *S* responds with P. Then proceed to the pair, G–L. If *S* responds with G, proceed to R–Q, but if he responds with L *turn the card over* and start at the beginning of Maze II with Z–P. Now you have to present Z–P in random order as determined by the deck of cards until *S* says Z, then proceed to L–G, in which L is now correct and G leads back to the start of Maze I. Whenever *S* makes an error on either of the mazes, except at the first pair, P–Z or Z–P, you will always turn the card and start in again at the beginning of the other maze.

"When *S* finally makes the correct response on the last pair of letters, you are to say, 'That is the goal. Now you must run through the maze again.' You tell *S* how many errors he made on his first trial, by counting up the number of P–Z, Z–P pairs. *Then you turn the card to the other maze and begin the second trial.* You continue until *S* makes three perfect trials in succession. For example, on Maze II, then Maze I, and then on Maze II again."

The main features of the procedure as given in the instructions to the *E*s as outlined above are: (1) a return to the start of the alternate maze upon every error except an error on the first pair of letters; (2) a return to the start of the alternate maze upon having reached the goal in the first trial; and (3) the reaching of a criterion of three errorless trials in succession, the middle one of which would be on the form of the maze which was alternate to the other two criterion trials.

Twenty-one useable records were obtained after rejecting five because of failure to complete learning within the time allotted and several because of mistakes made by the *E*s.

It was thought by the writer that those *S*s who had achieved 'insight' in this problem would know that upon every return to the start the maze changes, while those who had merely learned a series of letters which somehow led farther, and had not grasped the principle of alternation would try to use the same responses over again. It might be reasoned that those *S*s who became cognizant of 'something queer' about the start and therefore varied their responses during learning would make the 'insightful' response upon starting the maze the second time, but those who ignored this feature of the maze would stick to their stereotyped solution.

Accordingly the *S*s were divided into two groups: 'insightful' *S*s, who, having reached the goal on one maze for the first time (*i.e.* completed trial 1), responded with the letter correct for the *other maze* on the first two pairs of letters on the second trial; and 'insightless' *S*s, who responded with the letter correct for the maze they had just finished.

Table I shows the results for these two groups of *S*s. The 'insightful' *S*s take much longer to reach the first errorless run (last 10 pairs of letters in trial 1), as measured by the number of pairs of letters presented. They take longer to reach the second errorless run, but are faster in reaching the start of the first criterion trial. In fact, ten of the 'insightful' *S*s made no more errors after reaching the goal for the first time (the remaining four *S*s progressed down the correct maze for from four to nine pairs of letters, then apparently failed to retain the correct response and made one error).

The columns, B, C and D, of Table I show how the activity of the *Ss* was distributed during the first trial. The 'insightful' *Ss* spent relatively more time in progression along the mazes than they did in fumbling with the first two pairs. 'Progression' is here defined as reaching the third pair of letters or beyond, while merely reaching the second pair is classified with the repetitive errors, since this pair had to be reached, and incorrectly responded to, if the *Ss* were fixated on using only one form of the maze. The 'insightless' *Ss* spent a greater proportion of time on repetitive errors than did the 'insightful' *Ss*, and they also spent relatively little time in progression along the alternate maze.

TABLE I

PAIRS OF LETTERS PRESENTED UP TO VARIOUS STAGES IN LEARNING

	S	Pairs to start of first errorless run (Trial 1)	Distribution of activity during first trial			Pairs to start of second errorless run (Trial 2)	Pairs to start of first criterion run
			Repetitive errors	Progression along			
				Alternate maze	Same maze		
		(A)	(B)	(C)	(D)	(E)	(F)
'Insightful'	1	338	174	78	86	348	338
	2	276	139	67	70	291	291
	3	244	107	87	50	254	244
	4	228	67	81	80	243	273
	5	197	91	60	46	207	197
	6	182	94	56	32	192	182
	7	173	58	55	60	183	173
	8	150	68	31	51	160	150
	9	141	57	6	78	189	189
	10	90	32	34	24	100	90
	11	88	16	45	27	98	88
	12	69	22	27	20	79	69
	13	67	13	28	26	77	67
	14	66	21	22	23	94	94
	Mean	164.9	68.4	48.4	48.2	181.1	174.6
'Insightless'	15	222	87	6	129	234	351
	16	161	93	32	36	192	252
	17	85	49	43	3	96	131
	18	72	35	0	37	92	216
	19	71	52	9	10	174	199
	20	50	28	10	12	85	124
	21	37	13	0	24	50	179
	Mean	97.1	51.0	14.3	35.9	131.9	207.7
Mean proportions			B/A	C/A	D/A		
'Insightful' Ss			0.378	0.314	0.308		
'Insightless' Ss			0.516	0.144	0.340		

These scores were also changed into proportions of total pairs presented on trial 1. The means of these proportions are shown in Table 1 and appear to confirm the analysis above. Another method of checking this finding, the computation of the mean length of a progression along the maze on which the goal was first reached as compared with the alternate maze, yielded a similar result.

Insofar as data from such small groups can be trusted, it would appear that those *S*s who reacted 'correctly' to the arbitrary condition set by *E* for the second trial were those who were already aware of the existence of alternate routes through the maze. Introspective reports showed this to be the case. Those *S*s who had selected only one possibility as a route were unable to make this "correct" response readily, nor did they seem to learn very much about the possibility of an alternate route during the second trial.

The present writer believes that the results are indicative of something more important than their intrinsic meaningfulness. When preparing the set-up for use in the laboratory the writer predicted that those *S*s who first caught on to the alternate maze aspect (*i.e.* who showed 'insightful' behavior early in learning) would be the first to reach the goal. The results obtained show that the reverse is true. If, instead of reversing the maze for the start of the second trial, the *S*s had been allowed to use the same side, it seems obvious, although not yet demonstrated experimentally, that the 'single-track' *S*s would have completed the experiment much sooner than those who paid attention to both routes. Theirs would then have been considered the 'good' performance.

It is only by the addition of an arbitrary condition of 'insightful' performance, which the writer chooses to regard as a 'good' performance, that the possibility of alternative interpretations of the results becomes evident. It would seem that not only the development of apparatus and procedure, but also the criterion of success—and therefore, to a great extent, the results of many learning experiments,—may be dependent upon the theoretical bias and valuative concepts of the *E*.

THE FORGETTING OF PLEASANT AND UNPLEASANT EXPERIENCES

By L. I. O'KELLY, University of Colorado, and
L. C. STECKLE, Denison University

The comparative memory-value of pleasant and unpleasant experiences has repeatedly been investigated.[1] The *raison d'être* of the present study is to report the results obtained over longer recall-periods than have previously been used. Stagner used a period of 15 days,[2] Meltzer of 6 weeks,[3] and the present study a period of 10 weeks.

The results reported here were obtained from 96 *Ss*—about equally divided as to sex. All were students in elementary psychology. On the first day of classes following the Christmas recess (*i.e.* on January 5, 1939), the *Ss* were asked to write all the pleasant and unpleasant experiences they had had during the holidays. One group was instructed to list the pleasant experiences and then when this was done, to list the unpleasant ones. The order of affective listing was reversed for the second group. Ten weeks later, without warning, the groups were again asked to recall the pleasant and unpleasant events of the Christmas vacation.

In the tabulation and comparison of the results of the two recall-periods, the events reported at the second but not at the first recall were omitted from consideration.

The 96 *Ss* reported a total of 1179 experiences at the first recall. Of these, 741 (62%) were pleasant and 438 (37%) were unpleasant.[4]

The second recall, 10 weeks later, gave a total of 529 experiences which had been recalled before. Apparently 650 (55%) of the originally reported events had been forgotten.[5] Of the 529 remembered experiences, 354 (67%) were labeled pleasant, and 175 (33%) unpleasant. After 10 weeks then, 387 (52%) of the original pleasant events and 263 (60%) of the original unpleasant events

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* Accepted for publication October 3, 1939. This study was made at Ohio State University during January and March, 1939.

[1] H. Meltzer, The present status of experimental studies on the relationship of feeling to memory, *Psychol. Rev.*, 37, 1930, 124-139; Individual differences in forgetting pleasant and unpleasant experiences, *J. Educ. Psychol.*, 21, 1930, 399-409; A. T. Jersild, Memory for the pleasant as compared with the unpleasant, *J. Exper. Psychol.*, 14, 1931, 284-288; Ross Stagner, The redintegration of pleasant and unpleasant experiences, this JOURNAL, 43, 1931, 463-468; M. M. White and M. M. Ratliff, The relation of affective tone to learning and recalling words, *ibid.*, 46, 1934, 92-98; M. M. White, Some factors influencing recall of pleasant and unpleasant words, *ibid.*, 48, 1936, 134-139; M. M. Ratliff, The varying function of affectively toned olfactory, visual, and auditory cues in recall, *ibid.*, 51, 1938, 695-699.

[2] Stagner, *op. cit.*, this JOURNAL, 43, 1931, 465.

[3] Meltzer, *op. cit.*, *J. Educ. Psychol.*, 21, 1930, 401.

[4] Our results stand in remarkably close agreement with Meltzer's. He found that out of the total of 2231 experiences 1393 (62.4%) were pleasant and 838 (37.4%) were unpleasant. (*Op. cit.*, 402.)

[5] After 6 weeks Meltzer (*op. cit.*, 404) reports 46.9% forgotten.

had been forgotten. Or, conversely, 354 (48%) of the original pleasant events and 175 (40%) of the original unpleasant events were recalled.

The results in Table I (if they are comparable) indicate that the relative amount of pleasant and unpleasant experiences recalled tend to approach each other as time goes on, with pleasant events apparently being forgotten somewhat more rapidly than unpleasant. The difference at 10 weeks (8%) is, however, still significant, being ten times as large as its standard error (0.72).

TABLE I

Percentages of *P* and *U* Experiences Recalled

	Original (this study)	Original (Meltzer)	15 days (Stagner)	6 weeks (Meltzer)	10 weeks (this study)
P	62	62.4	54.4	53.0	48
U	37	37.4	43.4	39.8	40

An analysis of the individual results again showed the presence of the dichotomy, pessimist-optimist, reported by Meltzer.[6] Of the 96 *Ss*, 60 (62.5%) retained a greater proportion of pleasant than unpleasant experiences, 30 (31%) retained a greater proportion of unpleasant than pleasant experiences, while 6 (6%) were classified as indifferent, retaining an equal proportion of both affective divisions.

A further examination of these two classes gives other interesting results. An attempt was made to determine wherein lay the differences obtained between the pessimists and the optimists. The formulae, $P_1 - P_2$ and $U_1 - U_2$ were applied to every *S* in each of these groups. From this computation two columns of figures were obtained for each group (optimists and pessimists). These results appear in Table II.

TABLE II

Average Experiences Forgotten by Optomists and Pessimists

Pessimists		Optomists	
Mean P_1-P_2	4.8	Mean P_1-P_2	3.9
Mean U_1-U_2	1.4	Mean U_1-U_2	3.5
diff.	3.4	diff.	0.4
$\sigma_{\text{diff.}}$	0.5	$\sigma_{\text{diff.}}$	1.3
$D/\sigma_{\text{diff.}}$	6.8	$D/\sigma_{\text{diff.}}$	0.3

From this it would appear that while the optimists tended to forget both pleasant and unpleasant experiences about equally, the pessimists forgot pleasant events significantly more rapidly.

In order to investigate the possibility that the optimist-pessimist dichotomy might be a manifestation of one of the commonly recognized contrasted personality types (extravert-introvert, etc.), the Bernreuter Personality Inventory was given to all *Ss*.[7] Correlations between number of experiences recalled, both P and U, with the measures in the inventory, ranged from —0.01 to —0.27. This does not mean, of course, that no personality differences exist, characteristic of the pessimists and optimists, differentiated on statistical grounds. Rather, it would appear that our results suggest the possibility of the existence of another variable in personality measurement.

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[6] *Ibid.*, 409.

[7] R. H. Bernreuter, *The Personality Inventory*, 1931.

If the differential forgetting of pleasant and unpleasant experiences is due to an increased repression of the unpleasant, and not to a difference in the number of experiences of each type impressed, the results of this experiment indicate that only a part of the general experimental population evidences the repression. In this finding the results of Meltzer's study have been confirmed,[8] and their statistical reliability has been demonstrated.

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[8] Meltzer, *op. cit.*, 407-409.

APPARATUS

TWO DEVICES FOR MEASURING DRIVING SPEED ON THE HIGHWAY

By C. H. LAWSHE, JR., Purdue University

With the increasing realization of the importance of the human element in traffic and highway safety, a growing need has been felt for methods by means of which the behavior of drivers can be measured. We early discovered, in a highway research undertaken in July, 1938, that the then existing methods and devices for measuring driving speed had their limitations. Though adequate for many purposes, most of them were subject to errors of reaction-time on the part of the investigator and were not easily adapted for use at night. Moreover, they required that the investigator station himself in positions in which he could be seen by the oncoming driver. The devices described here obviate these difficulties.[1] Both are oscillographs; they differ only in that one utilizes a cathode ray tube and the other an optical lever for recording the oscillogram.

THE CATHODE RAY RECORDER

The basic part of the cathode ray recorder is contained in an instrument case (Fig. 1 B), 10 x 10 x 27 in. It is composed of a cathode ray oscillograph, exposed and unexposed film-magazines, a motor and gear assembly for driving the photographic film, and an electrically driven tuning fork.

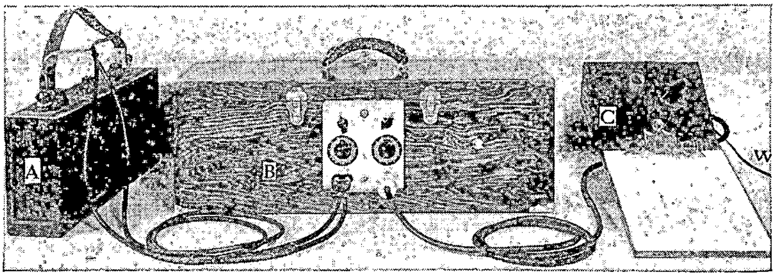

FIG. 1. THE CATHODE RAY RECORDER
'A' is the 6-v. storage battery; 'B,' the instrument case containing the oscillograph, tuning fork, and mechanism for driving the film; and 'C' is the combined data and control board. The wire (w) is the lead to the pressure switches.

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[1] These devices were developed in a study sponsored by the Joint Highway Research Project and the Division of Education and Applied Psychology of Purdue University. The research was directed by Dr. Joseph Tiffin. For assistance my thanks are due Professors F. B. Knight of the Division of Education and Applied Psychology, R. H. George and H. J. Heim of the School of Electrical Engineering, H. M. Jacklin of the School of Mechanical Engineering, and M. B. Steer of the Speech Clinic.

Oscillograph. A type 913 (1 in.) cathode ray tube is used in an adaptation of the basic circuit (Fig. 3) with a Mallory No. 554 Vibra-Pak and a 6-v. storage battery power supply. Oscillations on the screen of the tube are obtained with an interrupted D.C. potential of approximately 90 v., on the vertical deflecting plates of the tube, produced by a 100 ~ electrically driven tuning fork in conjunction with an ordinary door-bell transformer used as a step-up.

Photographic record. A photographic record of the wave pattern is obtained by sweeping horizontally across the screen of the tube a strip of 35-mm. Eastman Recording Paper (No. 1) by means of a 6-v. horn motor which is controlled by either of two switches; one on the oscillograph control-panel and one on the combined data- and control-board (Fig. 1 C). Light tight film-magazines with a 200-ft. capacity are employed for both the exposed and unexposed recording paper in order to make daylight loading possible.

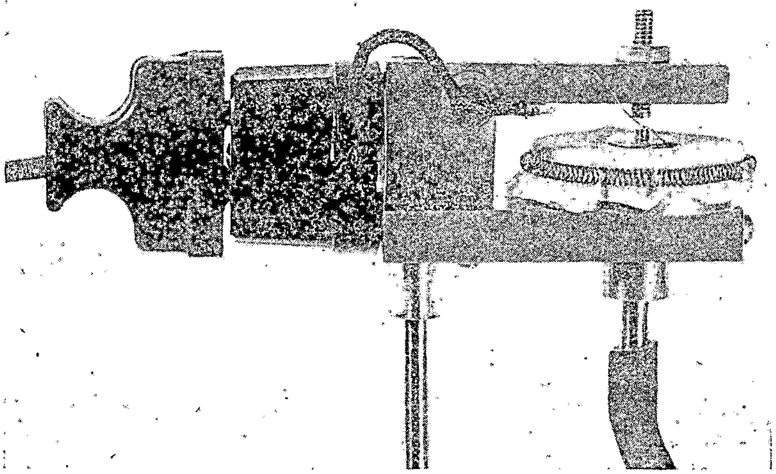

FIG. 2. THE TAMBOUR PRESSURE-SWITCHES

Detector-equipment. Small rubber tubing (3/32 x 3/8 in.) stretched halfway across the pavement is anchored in place by means of stakes, small turnbuckles, and soft iron stovepipe wire threaded through its center. These rubber tubes are quite inconspicuous to the driver, since they look like expansion-joints, and are used to actuate tambour pressure-switches (Fig. 2) at the side of the road. The switches are connected in the circuit (Fig. 3) in such a manner that pressure-impulses resulting from the passing of a car over the rubber tubes operate the switches which in turn shunt out the 15,000-Ω resistor in the deflecting plate-circuit of the cathode ray tube, the result being a momentary increase in the amplitude of the curve on the tube-screen. With the recording paper in motion, the result of this action is an oscillogram quite similar to the one in Fig. 4 which was made with the optical lever-recorder.

Operation. Two rubber tubes are stretched across the pavement at a known

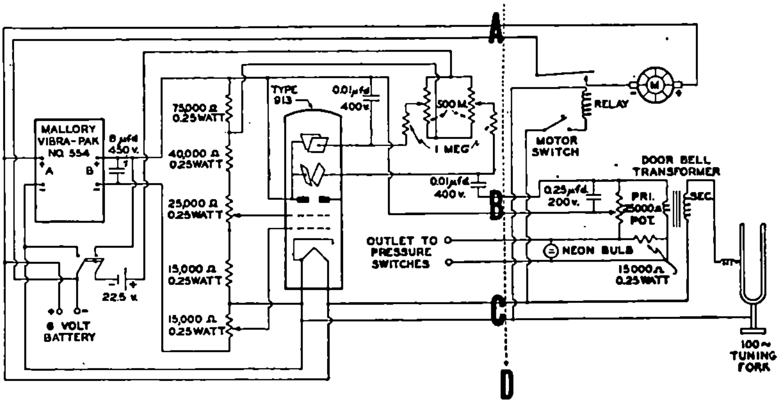

FIG. 3. CIRCUIT-DIAGRAM OF THE CATHODE RAY RECORDER

For the optical lever-recorder, only the portion of the circuit to the right of the broken line 'D' is used. The two pairs of leads 'A' and 'C' are connected directly to the 6-v battery and the pair marked 'B' is attached to the headphone unit.

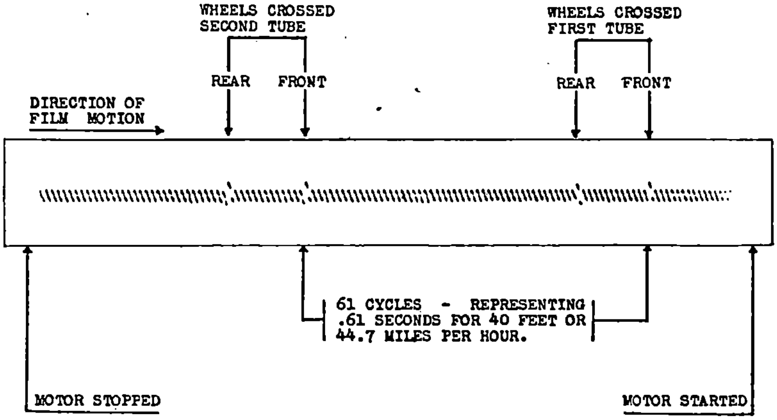

FIG. 4. AN OSCILLOGRAM MADE WITH THE OPTICAL LEVER-RECORDER

Oscillograms made with the cathode ray recorder are quite similar.

distance apart, preferably 40 ft. (the minimum separation that will accommodate the maximum legal length for vehicles in the state of Indiana). Pressure-switches are attached to these tubes and cables are laid to some inconspicuous point sufficiently far from the highway so as not to distract the motorist. The cables are led to the control-board where a neon lamp is shunted into the circuit. This lamp glows continually as long as the motor-mechanism is off. When a car approaches the detector-equipment, the investigator turns on the motor-switch which extinguishes the neon lamp and starts the oscillographic record. As the front and back wheels pass over the rubber tubes the pressure-switches are closed and deflections are made in the oscillogram. The neon lamp flashes on every time the pressure-switches are closed, hence, the investigator can readily see whether the switch-points are contacting. After a car has passed the detector-equipment, the apparatus is turned off and the investigator awaits the approach of the next car.

Reading the records. Records are read, after the paper has been developed, by counting the cycles between the deflections resulting from the passing of the front wheels over the first and second rubber tubes. Since a 100 ~ tuning fork is used, the number of cycles intervening shows in 0.01 sec. the time required for traversing the distance between the tubes. Conversion to miles per hour is accomplished by means of tables prepared for the purpose.

Calibration. Two methods have been employed in calibrating the instrument. The first was a laboratory test designed to determine the effects of varying amounts of clearance between the contact-points of the pressure-switches. A 16-lb. shot was held at the top of an inclined trough by a release-mechanism. Two rubber tubes were placed across the trough in such a manner that they would be traversed when the shot was released. The contact-points of the pressure-switch of the second tube traversed were set at 0.01 in.; those of the first tube were set at 0.005 in. in one series of experiments, and at 0.035 in. in a second series. (These values are the extremes at which the pressure-switches operate successfully.) As shown in Table I, there is virtually no difference between the arithmetic means of the readings at the two settings. The variability of the readings was, however, considerably greater when the points were farthest apart. It is, therefore, considered advisable to operate the switches with a minimum clearance between the contact-points.

TABLE I

Comparison of Measurements with Minimum and Maximum Clearance Between Switch-Points

	Clearance	
	0.005 in.	0.035 in.
No. measurements	25	24
Mean time (in 0.01 sec.)	89.04	89.27
S.D.	1.34	0.56

The second check was made in the field with the aid of a stopwatch. A section of smooth, level highway was chosen and a quarter-mile stretch was marked off by means of white stakes at the side of the road. The speed-recorder was set up in the center of this quarter-mile. A car was driven over this strip at as constant a speed as possible, and an investigator riding in the car clocked the time between the two stakes with the aid of sights. Records of the runs were taken simultaneously with the speed-recorder. While it is probable that this instrument is more accurate than timing with a stopwatch, a comparison of the speeds obtained by the two methods (Table II) shows an average difference for eight trials of 0.36 mile per hour.

The Optical Lever-Recorder

The optical lever-recorder is identical to the cathode ray recorder except that the cathode ray unit was removed from the instrument case (Fig. 1) and a mechanical unit (Fig. 5) was substituted. This assembly consists of a lamp housing containing a 6-v. 50 candle-power bulb, a Dorsey phonelescope[2] (incorporating the optical lever-principle) actuated by a standard headphone unit, and an appropriate lens system for focusing the light beam on the recording-paper. The circuit used is identical to that portion of the circuit-diagram at the right of the dotted line 'D' in Fig 3. The two pairs of leads marked 'A' and 'C' go directly to the 6-v. battery and the pair marked 'B' are attached to the headphone unit. The 6-v lamp is connected in the motor-circuit so that it operates only when the motor is in motion, thus preventing unnecessary heating of the lamp. Fig. 4 is an unretouched photograph of an oscillogram made with this unit. All other features of the equipment and its operation correspond to those of the cathode ray instrument.

The Multiple Recorder

The multiple speed-recorder is an elaboration of the apparatus already described and may utilize either the electronic or mechanical principles discussed above. It consists of the basic recording unit with the addition of more rubber tubes and pressure-switches. Six of these spaced at 100-ft. intervals have been used and the performance of the instrument indicates that many more could be added with equal success. One common wire is connected to all switches and a separate lead extends from each one to a selector-switch on the control-board.

TABLE II
Comparison of Speeds Obtained with a Stopwatch and with the Cathode Ray Recorder

Trial	Miles per hour		Difference
	Watch	Instrument	
1	30.4	30.1	−0.3
2	30.4	30.7	0.3
3	30.4	30.7	0.3
4	41.3	40.9	−0.4
5	41.7	40.0	−1.7
6	49.2	49.8	0.6
7	50.3	50.5	0.9
8	50.0	50.5	0.5
Av. diff.			0.56

Operation. As a car approaches the wired area, the motor of the apparatus is started and the selector-switch is set so as to connect the instrument with the first pressure-switch. After the vehicle has crossed the first tube (as is indicated when the neon lamp flashes) the selector-switch is set for the second rubber tube, and so on. In this manner it is possible to follow the same driver through the entire 600 ft. and his average speed for each consecutive 100 ft. may be obtained. Since only one pressure-switch is connected with the oscillograph at a time, very few records need to be discarded because of interference from passing or trailing cars.

Use. Use of the multiple speed-recorder has been confined to date to the study of the deceleration-patterns of drivers in response to various types, sizes, and locations of highway signs. Fig. 6 shows a graph of the patterns of five drivers approaching

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[2] C. H. Stoelting Co., 424 North Homan Avenue, Chicago, Ill.

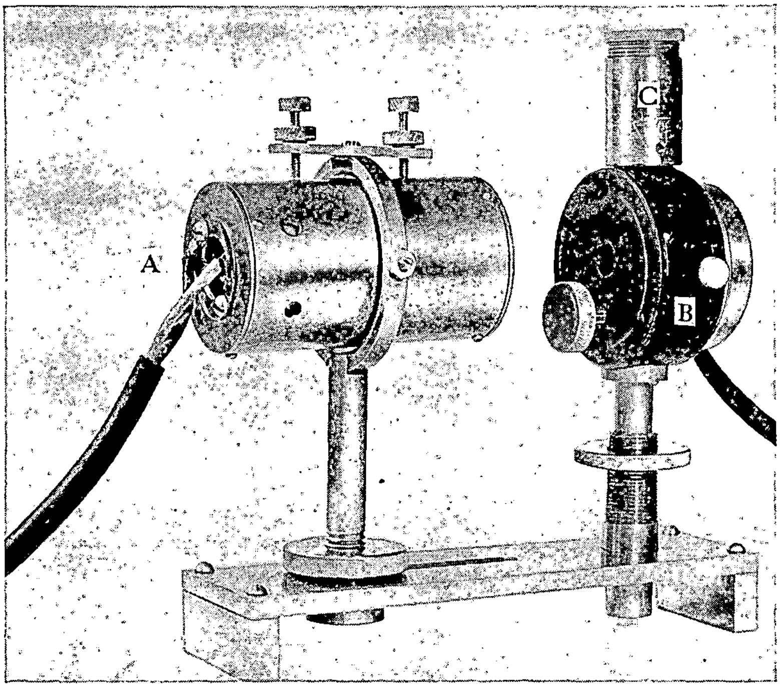

FIG. 5. THE OPTICAL LEVER-RECORDER
'A' is the 6-v. lamp-housing and 'B,' the phonelescope with headphone unit attached. 'C' is the lens system for focusing the light beam on the recording paper.

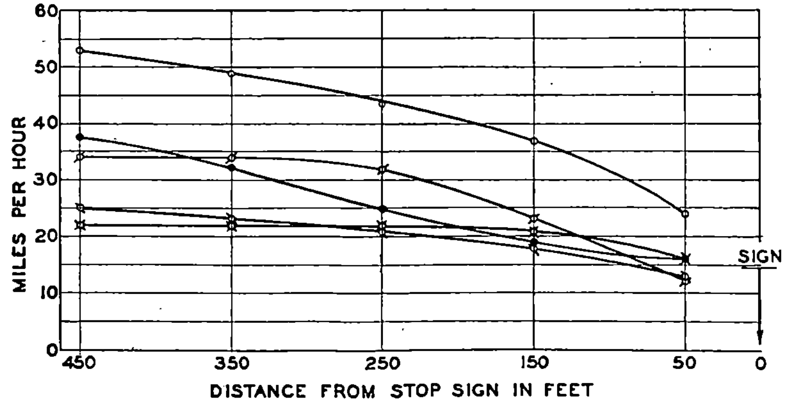

FIG. 6. DECELERATION-PATTERNS OF FIVE DRIVERS
All of the drivers were approaching a standard sized stop-sign (24 in.) at a state road intersection. These results were obtained with the cathode ray recorder.

a standard 24-in. stop-sign at a state road intersection. It is representative of the type of data that may be collected with the instrument. The apparatus may also be used in a number of situations such as those involving parked vehicles, pedestrian crossings, and advertising signs, where a continuous speed-pattern of drivers is desired in studying their behavior on the highway.

COMPARISON OF THE DEVICES

The electronic and mechanical recording devices described here have both been successfully used. It may be desirable, however, to point out, from our experience with them, the advantages and disadvantages of each.

The chief advantage of the cathode ray recorder is that its first cost is less. After extended use, however, the cathode ray tube tends to lose its photographic properties. There is also some evidence that at times the paper passing across the glass screen develops an electro-static potential which disturbs the action of the tube and results in a blurred record. The optical lever-recorder never fails to yield a readable record provided the instrument is properly focused. While records made with both instruments are adequate, those made with the optical lever-recorder are more clear-cut and easier to read. Records made with the optical lever-recorder are, moreover, much finer, hence, it is possible to operate the film at a slower speed—thus reducing operating costs.

A PROJECTOR-STAND TO FACILITATE PLOTTING

By L. C. GILBERT, University of California

To facilitate the labor of plotting eye-movements from motion-picture film an adjustable projector-stand has been designed, permitting the laboratory technician to sit comfortably at a table and work in a normal writing position. The stand rests upon a substantial work-table and consists of stable base (B) supporting on uprights a platform (C) which moves in grooves. The projector (P) rests upon the platform which can be lowered or raised by crank (A) to adjust the magnification to the reading distance of the eyes from the reading material during the photography. Directly in front of the projector lens the platform supports a mirror (M) at an angle of 45° in such a manner as to reflect the image downward to the movable plotting board (K) which rests flat on the table. For accurate reflection it is important that the mirror be of superior quality. The mirror stand (E) may be moved back and forth to adjust the distance between mirror and lens.

The film in the projector is manipulated from the table surface by the non-plotting hand by means of a chain and gear (G) attached to the projector sprocket (R). Plotted film passes into a container through a slot (S) in the platform base. For additional convenience there are three drawers built into the back of the base with pegs for the rolls of film and two drawers without pegs for plotting paper and pencils.

The movable plotting board rests flat on the table. The record from the elevated projector is reflected down to the plotting paper. The technician can then sit comfortably at the table and record the data in a normal working position, without the

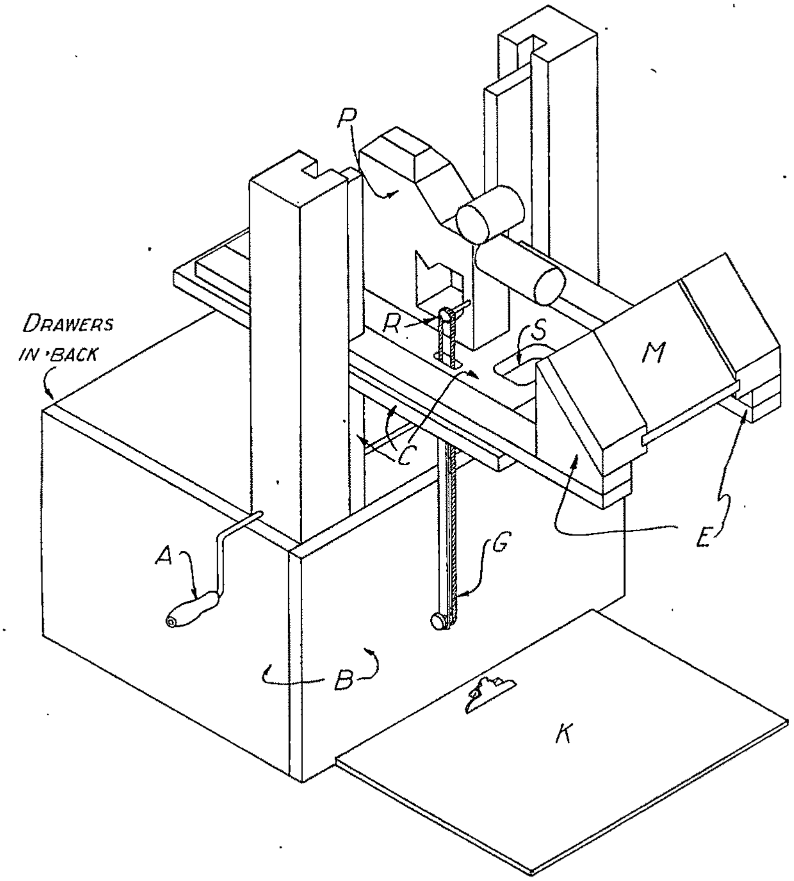

FIG. 1. PROJECTOR-STAND
A = crank; B = base; P = projector; M = mirror; E = mirror stand; S = film slot; R = projector sprocket; C = projector platform; G = chain and gear; K = plotting board.

necessity of shifting back and forth to turn the film and without the discomfort of plotting against a vertical board.

A SIMPLE AND ACCURATE METHOD OF CALIBRATING CHRONOSCOPES

By ROY M. DORCUS and GEORGE E. MOUNT, University of California at Los Angeles

One of the major problems in reaction-time work and in precise measurement of short time-intervals has been the calibration of the instruments. The old Hipp chronoscope was calibrated by means of the Wundt Fall Hammer or similar gravitational devices. It is well known that the friction of the bearing supports of the pendulum, the inertia of the contacts as well as the friction in the contacts resulted in some appreciable error. Substitutes of falling balls or balls rolling down inclined planes are not completely satisfactory, since the contacts that are made and broken by falling bodies give trouble especially for the shorter time-intervals. Friction of rolling balls on an inclined plane is difficult to figure and hence time measures from this source are likely to be inaccurate. The other method that has been used and which is highly accurate is photography. This involves very expensive equipment, including a string galvanometer or oscillograph, a high speed camera, and sensitive film. If the circuit to be timed is photographed simultaneously with an oscillating circuit, the degree of accuracy will be as reliable as the accuracy of the oscillating circuit.

The method proposed here is a modification of the photographic method. Many laboratories possess apparatus for making speech-records and if they do not, records can be made commercially for a small cost by almost any radio sales store. All the equipment that is needed in addition to the record cutter is a tuning fork or an oscillator of known frequency.

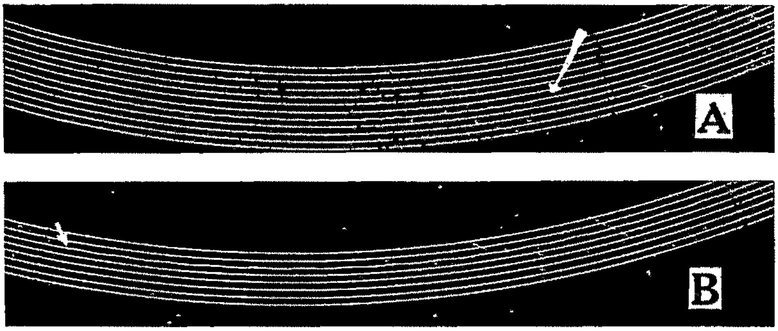

FIG. 1. SAMPLE RECORDS
A = 512 ~ tuning fork; B = 1000 ~ oscillator

The method of making the calibration with a tuning fork is as follows. (1) Set the recording equipment into motion. (2) Strike a fork and hold close to the microphone. (3) Close the amplifying circuit to the cutting head simultaneously with the starting circuit of the chronoscope. This can be accomplished with a single-throw double-pole switch. (4) Open the amplifying circuit simultaneously with the break circuit of the chronoscope by means of a double-pole single-throw switch. (5) Read the time on the chronoscope. (6) Count the oscillations cut on the record.

The procedure with an oscillator is the same as with a tuning fork except that the output of the oscillator may be fed directly into the cutting head.

It might be added that using a tuning fork is probably more reliable than using an oscillator since oscillators do not hold their calibrations accurately over long periods of time and since it is almost impossible to set them at a frequency which is exactly that specified on the calibration dials.

It is well-known that the beat count technique is useful in calibrating oscillating circuits at low frequencies when compared with a tuning fork but at the higher frequencies (over 10,000 ~) the accuracy of adjustment of the oscillators is such that it is almost impossible to set them at a point where the beats can be counted. These beats can be determined, however, if the fork and oscillator are recorded simultaneously on a disk. In making this record, the input current from the microphone should be equated with the current imposed on the cutting head from the oscillator. This is an inexpensive method of calibrating certain of the frequency-ranges of the oscillator.

We have presented in Fig. 1 photographs of a sample record of a fork and oscillator. The arrow in A marks the end of the vibration of a 512 ~ tuning fork; the arrow in B marks the beginning of a 1000 ~ oscillator. The rate at which the record is revolved will in part determine the ease of counting the oscillations. We have found that aluminum records are easier to read than acetate records. Since the cutting head responds to very high frequencies we did not expect any appreciable lag but this point was checked and no lag was detected at the 1000 ~ frequency.

A NEW DEVICE FOR MEASURING THE GALVANIC SKIN RESPONSE

By S. EDMUND STODDARD and CARL FISK, Blackfoot, Idaho

The device described here for measuring the galvanic skin response has been successfully used by the authors for three years. Its simplicity of construction and operation warrants, as we believe, its wider application and use.

The device, shown on its side with top removed in Fig. 1, consists of a light-proof box (A)—88 cm. long, 22 cm. wide, and 20 cm. high—which contains a 100-w. mazda lamp (B) and a photronic cell (C). The photronic cell is placed in the small box (F)—16 cm. long, 10 cm. wide, and 9 cm. high—, whose lid (L) is also removed, at any one of four positions. At the front of this small box an iris diaphragm (F) is mounted whose aperture ranges between 2.5–30 mm. in diameter. The mazda lamp (B), which is directly in front of the iris diaphragm, is fastened to a base that may be moved either forward or backward between the supports (E) by means of the metal rod (K). There are then three ways of varying the strength of the light falling upon the photronic cell: 1) the position of the cell within the smaller box; (2) the size of the iris diaphragm; and (3) the distance of the mazda lamp from the iris diaphragm. Of the two wires from the photronic cell, one runs to the galvanometer (D) and the other to one of a pair of copper electrodes (G). The second of these electrodes is connected to the galvanometer.

The placing of the electrodes on the skin is of great importance. We place them on opposite sides of the palm of one hand. Two pieces of felt are first dampened in a normal saline solution and placed in position. The electrodes are then laid upon the felt and held in place by means of a broad rubber band. The rubber band insures a constant pressure of the electrodes against the skin.

In this set-up we used a Weston photronic cell and a Leeds-Northrup galvanometer. The Weston cell is not injured by direct sunlight; it is non-microphonic and comes to its maximum value instantaneously—declining slightly after a few minutes to its final value which is approximately 1% below the maximum. Temperature changes have little effect on the current output though they do have relatively a large effect upon the voltage generated. The galvanometer has a sensitivity of 0.31μ a per mm. Its period is 2.7 sec. The external critical damping resistance is 2800 Ω, and the coil resistance is 1110 Ω.

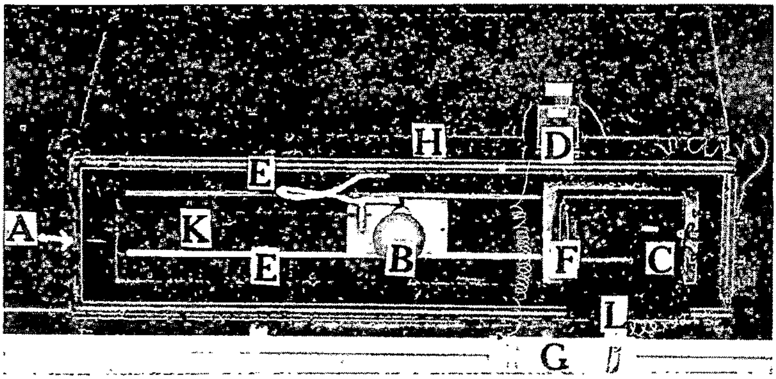

FIG. 1. THE PHOTRONIC APPARATUS FOR MEASURING THE GALVANIC SKIN RESPONSE

The results that we have obtained with this apparatus have been highly satisfactory.

APPARATUS NOTES

THE USE OF MUNSELL PAPERS IN TESTS DIAGNOSTIC OF COLOR WEAKNESS, COLOR BLINDNESS, AND VOCATIONAL COLOR CAPACITY

To meet shortcomings in current clinical tests of color vision, test-patterns need to be extended in two directions: (1) into more saturated color regions, in order to isolate the completely red-green blind; and (2) into weaker chromas, to detect border-line cases of high or low sensitivity to single critical colors, *i.e.* red, blue, violet, green, or yellow.[1]

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[*] Read at the annual discussions session of the Problems Committee of the Inter-Society Color Council, February 22, 1940, New York City. For apparatus and materials, the writer is indebted largely to grants-in-aid from the National Research Council and the National Society of the Sigma Xi; for other equipment and space, to the Psychological Laboratory of Cornell.

[1] Tests of foreign manufacture are not only difficult to obtain under war conditions, but impossible to copyright, hence subject to ruthless exploitation by commercial interests. Their use of easily memorized digits tempts applicants for whom normal color vision is a requisite to dishonesty (of this the writer has abundant proof). Cf. E. Murray, The Ishihara test for color-blindness, this JOURNAL, 47, 1935, 511-513. Use of numerals of unequal legibility, and the unhandy distance to which the plates are keyed for testing, invariably ignored by examiners, inject extraneous and variable vectors arising from refraction, astigmatism, acuity, retinal color zoning, and other ocular factors into test results.

Sample designs used in attacking this problem are of three types: *(1)* hue threshold; *(2)* hue discrimination; and *(3)* hue identification charts. In these designs Munsell papers are utilized, since they permit precise specification of the three dimensions employed in psychological color description, and are further standardized by spectrophotometric curves.[2]

(1) Hue threshold charts. A low-chroma hue (for red, blue, green, or violet sensitivity) is displayed through a circular aperture (1/16 to ¾ in. in diameter) in a matching gray ground. *E.g.* PB 5/2 through a ½ in. aperture in a 3½ in. square of N 5 (a middle gray). The chroma of the hue may be varied until *O*'s threshold at reading distance is roughly located for a given spot of color. Variation of distance is to be avoided, for reasons connected with refraction.

Gray backgrounds of the same value are used in preference to black or white as being less likely to involve hue shifts or changes in chroma in small color areas, though 'simultaneous contrast,' *i.e.* induction of negative hue or brightness by the ground. Exposure is made in a 3½ in. sq. gray kodak printing-frame with or without a protective layer of cellophane or celluloid. The frame permits ready shift of the color behind the aperture, and helps eliminate the shadow halo, especially if a celluloid cover can be used without alteration of hue or annoying reflections. *Query* (put to the *O* tested): Is a spot visible on the ground? If so, is it the same or different in hue or value (brightness)?[3]

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[2] Extended use of Munsell designations has been made by the ISCC committee on specification of colors in their recently completed task (begun in 1931) of standardizing color names for the U.S. Pharmacopoeia. The end set was a system of designations "sufficiently standardized to be acceptable to science, broad enough to be usable by science, art, and industry, commonplace enough to be understood by the general public." By utilizing the Munsell framework, 319 terms were found sufficient (with modifiers) for the entire psychological color solid, comprising millions of color tones. See D. B. Judd and K. L. Kelly, Method of designating colors, *J. Research N.B.S.,* 23, 1939, 355-386. This system is known as the ISCC-NBS method, in recognition of the joint contributions of the Inter-Society Color Council and the National Bureau of Standards.

The Munsell system was first brought out in 1905 and 1913, by Albert H. Munsell of the Massachusetts School of Art, after years of intensive work on Helmholtz' suggestion for tri-dimensional description. Since Munsell's death in 1918 the system has been further elaborated by A. E. O. Munsell and the Munsell Company. (See *Munsell Book of Color,* standard and abridged, with its 400 color specimens, and the *Munsell Manual of Color,* of 1929.) Equally spaced on a hue circuit of a 100 steps are five basic hues: red, yellow, green, blue, and purple. These, with the five intermediates, YR, GY, BG, PB, and RP, constitute the major hues, to each of which is allotted a 10-step span on the hue circuit, with the hue centered on step 5. Ten secondary intermediates, R YR, YR Y, etc., appear also on the published color charts (for simpler designations, *cf.* Judd and Kelly, *op. cit.,* 359, and D. Katzin and E. Murray, Instability in color naming, this JOURNAL, 46, 1934, 613). All 20 are available also in a number of values and chromas in separate sheets or smaller swatches, the number of variants in a constant hue chart ranging from 7 to 32 according to the permanent pigments obtainable.

Value (lightness) is graduated in 10 steps from black (zero) to white (10), on the central axis of the color solid. Values on the constant hue charts range from 2 to 8. Chroma (vividness or strength of color) is figured also on a 10-point scale. A given sample may therefore be designated by the symbols P 5/4, the numerator representing value, the denominator chroma or saturation. For greater precision a hue circuit digit (1-10) may be prefixed to the hue letter as 5R 6/10. Chromas 2, 4, 6, 8, 10, and sometimes 12 and 14, are now available for a number of the major hues; 4, 6, 8, or 10 for most of the secondary intermediates, and lately for some of the third intermediates, *e.g.* 2.5 and 7.5 R. (The Allcolor Company offers a number of supplementary chromas, though less accurately scaled, for the use of illustrators, advertisers, and interior decorators.) The neutral grays of the value axis are designated as N 1-10.

Reverse procedure displays a matching gray through an aperture in a square of low-chroma hue, thus ruling out reduction of color through *macula lutea* absorption (an end served also by enlarging the aperture).

(2) Hue discrimination charts: (a) One-spot. Low-chroma colors nearby on the hue circuit and of the same brightness are displayed one through an eyelet of another and vice versa. *E.g.* G 5/2 through BG 5/2. The difference between the two hues may be gradually increased or decreased till the approximate limen for discrimination of hue for a given individual in this region is located. [This method may also be used to test confusion-colors in the color-blind.]

(b) Two-spot. Two hues of low or medium chroma are displayed through 1¼ in. apertures (about 1 in. apart) in a 6 x 3½ in. rectangle of gray of the same value. *E.g.* G 5/4 and GBG 5/4 (green and bluish green) on N 5, in a larger kodak frame (painted gray or black). As in (*a*), chroma, value, or hue difference may be varied, or other regions of the color scale substituted. *Query:* Are two spots visible on the ground? If so are they the same or different in hue or value from the ground? From each other? Which is bluer or greener?[4]

The above charts, *(1)* and *(2)*, are designed primarily to spot divergences in *sensory* (biological) *equipment,* innate or acquired, producing color blindness or color-weakness and imposing inevitable limits on sensory performance and color aptitude. The patterns of *(3a)* and *(3b)*, on the other hand, presumably probe not only intrinsic and inalterable differences in sensitivity at various points in the hue circuit, but chance differences as well in color concepts and speech habits, such as the location and extent of the hue range accorded a given term (green or blue) as a result of early environment and training; or the ability to differentiate between hue and chroma or brightness changes (artificial and acquired distinctions).

(3) Hue identification and recognition charts: (a) Three-spot. Choice of two alternatives (in the violet to purple region). A standard and two comparison hues, of the same or slightly different brightness and chroma, are displayed diagonally in the square kodak frame through ¾ in. circular apertures in a matching gray. *E.g.* P 5/6 between PRP 5/6 and RP 5/6 on N 6. *Query:* Which of the two comparison spots is the closer match (*i.e.* nearer on the hue circuit) to the central standard?

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[3] Hue threshold tests, of the old Abney and Hess 'extinction of color' type, are indispensable prerequisites in determining color aptitude. Obviously, the limiting factor in an individual's discrimination of hues is his relative sensitivity to the several primaries, the overlapping of which gives us the various discriminable colors of the spectrum, to the number of about 150. Weaken one primary and the number of adjacent discriminable hues inevitably declines. Superimposed upon these biological optical limits (which may or may not be alterable by drugs or diet) will be the individual's learning curve in this field of interest.

[4] The stock psychophysical techniques (methods of limits and of constant stimuli) are applicable here. Where, however, interest pivots on color aptitude rather than on color theory, and where a sense organ as delicately adjusted as the eye is concerned, a dozen spaced and carefully conditioned observations will be worth far more than several hundred rapid-fire judgments (with an over-adapted *O*) rushed through to run up impressive statistical totals.

Which is bluer? [The blue-weak may hesitate between the two comparison colors. Trained color-blinds, on the other hand, guided merely by chroma differences in the blue component, may choose correctly; in which case the chroma of RP may be increased, that of the standard decreased, usually to the confusion of the red-green blind.]

This design may also be used with normal *O*s to test the accuracy of assignment of low-chroma hues on adjacent Munsell hue charts. *E.g.* many of the low-chroma Ys probably belong on the YGY page; the low-chroma Rs in the RPR group.

Similarly, in a higher zone of the value scale, tints of violet and purple may be compared for relative closeness of match. *E.g.* standard RPR 8/6 with PRP 8/2 and P 8/2 (lower chromas) on N 8 (a light gray). [Red weakness may lead to hesitation between the comparison violets. Blue weakness is less likely to lead to confusion.]

(b) Five-spot. Selection of match from four alternatives. The central standard in the sample is a bluish green, GBG 6/4. The four comparison colors are a second bluish green of lightly different chroma and value, GBG 5/6; a green, G 6/4; a greenish blue, BGB 6/6, and a blue-green BG 6/4. *Query:* Which of the four corner spots resembles most in hue the central standard spot? Response may be checked by four different positions of the test chart.

[Slightly divergent chromas and values are utilized in the five spots. Hence those of normal vision who have not learned to abstract hue from other variables may unwarily choose BG 6/4. A blue-weak individual may hesitate between the first two comparison colors, GBG and G.][5]

The sample procedures above described are all carried on under fixed or controllable conditions of illumination, surroundings, background, distance, size (visual angle), angle of stimulus-surface with line of vision and of lighting, and with constant pre-adaptation. A gray booth, Corning day-light roundels with high wattage bulbs yielding light of spectral energy distribution equivalent to 6500° K, illumination of 20 f.c., reading distance of 15 to 18 in., are among the constants.

All results needed for scientific norms should be checked with color vision thresholds obtained with spectrometer light or Wratten and Corning filters in a special limenopter with Aubert diaphragm or similar control of light intensity.

Preliminary tryouts included use of a Group Color Classification Test in which 36 five-inch samples of assorted hue, brightness, and chroma were displayed on a wall chart beneath 6 standards (*viz.* hues of middle to high chroma—Y, G, BG, B, P—along with a medium gray). Every sample was allocated to its proper hue by a check mark on a cross-section blank, designed for correction with a stencil. Color-blinds were found to be as effectively eliminated by this test as by the Holmgren wools or the Ishihara. Its use for more exact diagnosis, however, was blocked by difficulty in securing accurate hue, chroma, and value sequences for the six standards in the Hering, Heidelberg, or Bradley colored papers available. Recent extensions of the Munsell series, along with price reductions, raise hopes of the revision and more extended usefulness of this test procedure in preliminary diagnoses.[6]

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[5] The severity of the test may be increased by insertion of a time factor, *i.e.* presenting standard and 4-spot comparison chart in sequence. Results in any region, by either method, obviously bear on the industrial problem of 'color tolerances,' *i.e.* the amount of chromatic deviation permitted the manufacturer by the customer.

A second preliminary test turned on the ability to arrange 20 hues of low chroma and value (4/4) in an evenly stepped circle (*i.e.* the Twenty Hue Circle Test). In an abbreviated procedure, the individual tested passed merely on the perfection or distortion of a set arrangement in which, at several points, there was interchange of adjacent hues. Both procedures catch color-blinds (to whom the concept of a color circle is fairly meaningless). Owing, however, to the fact that lower chromas were not available for more than 10 of the Munsell hues, whereas far more than 20 are needed to provide sufficiently small hue steps, the test proved impracticable for isolating mild color weakness. Recent extension of the Munsell system may permit revival in more adequate form of this procedure.[7]

On the basis of the above tentative approaches, a clinical color test is in process of construction by the writer. Utilizing in the main the old and tried device of a design in terms of color spots of varying size, chroma, and hue on a white ground, it aims to eliminate reliance on test numerals of varying legibility and high memorability. Further, it aims to diagnose both complete and partial red-green blindness, shortening of the spectrum at the red or violet end, and other borderline deviations in the normal-visioned, the detection of which, with the growing use of color in art and industry, has assumed the proportions of a major problem.[8]

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[6] This test was administered in 1935 to a high school art class of 55, with a view to evolving a measure of amount of improvement in identification of hues induced by a semester's instruction. Initial difficulties, arising from the foreign parentage of many of the students, and their awkwardness in handling English prepositions, whereas the directions called for a check mark *opposite* the number representing the sample, and *under* the letter representing the standard, threatened to transform the exercise into an intelligence test, but were overcome. More than half of the class were girls, and color-normal; but among the boys two color-blinds and two color-weaks were discovered.

[7] The Student Charts developed by the late Walter J. Spry of the Munsell Company, published in 1936 and now in use in a number of colleges and art schools, designed to establish the three-dimensional concept of color in the student's mind, are built on similar lines. Diagrams of a 10-segment hue-circuit, a 9-step value axis, a 7-step chroma radius for R (covering 2 to 14 points), and an envelope of color chips (each about ½ by ⅝ in.) are provided at moderate cost. Ten or more constant-hue charts, each with its envelope of chips, and a silhouette indicating the characteristic outline of each completed chart, are now available. By mixing the contents of the envelopes for two or more adjacent hues, ability to discriminate hue from value or chroma might also be tested, especially when the present minor deviations from accuracy of the Munsell constant-hue charts are corrected. Timing of arrangements before and after a course of instruction, with due allowance for diverse finger agility, may gauge susceptibility to improvement in hue discrimination.

[8] While the tests above described are utilized by the writer mainly to locate critical color regions to be tapped in tests for color deviates, they are susceptible of other uses, *e.g.* in Psychology I laboratory exercises, where they may serve two purposes. First, they provide excellent material illustrative of the type scientific experiment, with exact control of conditions and quantitative variables. Secondly, they tap the natural undergraduate interest in individual differences and achievement, and suggest natural vocational trends or limitations—in an era when color has insinuated itself into a wide variety of occupations and professions. In the graduate laboratory, they assist in the selection of qualified *O*s for visual problems.

NOTES AND DISCUSSION

THE SIZE OF THE DIFFERENTIAL LIMEN FOR PITCH

The size of the differential limen for pitch has always been of especial interest because it is supposed to fix the minimal differentiation in the inner ear that a place theory of pitch, like Helmholtz's, must have. Helmholtz himself had only Seebeck's statement that the threshold is about 1/1000, and he figured that there ought thus to be about 64 discriminations per semitone, although there were only about 33 rods of Corti per semitone to act as resonators.[1] After Luft's determinations had come out in 1888, Külpe used them to conclude that there would be altogether about 11,000 discriminations in the total range of pitch. By this time Helmholtz had abandoned the rods of Corti in favor of the basilar membrane as a resonator, so Külpe merely remarked that there were more than enough hair cells in the inner ear to provide the necessary differentiation.[2] This figure—11,000 discriminably different pitches—is probably fixed in the mind of nearly every psychologist who took his Ph.D. before 1920; yet here are Shower and Biddulph, with the new thermionic technique for tonal stimulation, finding limens so large that the number of discriminable pitches, even at the intensity which gives the maximum, is only about 1500.[3] What has become of the other 9,500 pitches that Külpe had? The modern thermionic psychologists say that the old results were got with bad technique and without good experimental control, but why does *poor* control help the organism to make *more accurate* discriminations?

The conventional values that are usually cited for the chief investigations of the differential limen for pitch are given in Table I. Inspection of the table makes it appear that the limens, consistently small in the early days, got suddenly larger about 1900. Before we inquire into the reason, let us see what these investigations were like.

The early estimates of differential sensitivity generally stated the frequency ratio that could be just discriminated, and these fractions varied from 1/2000 (Sauveur, 1700) to 1/100 (Seebeck, 1846). They did not take into account the fact that the fraction might vary with the frequency.

The first adequately described experiment was made by Delezenne in 1827.[4] He had a string 1147 mm. long, tuned to 120 c.p.s. A bridge placed at the middle gave him two halves, each tuned to 240 c.p.s. He found that good *O*s under the best conditions could detect a difference in pitch if the bridge were moved 1 mm. toward one end. That means a limen of 0.42 c.p.s. Practically everyone could notice the difference of 0.84 c.p.s. when the bridge was moved 2 mm.

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[1] H. Helmholtz, *Die Lehre von den Tonempfindungen*, 1865, 219 (undoubtedly the same in 1st ed., 1863, which I have not seen). Helmholtz attributes the Weber fraction of 1/1000 to E. H. Weber himself, but Weber seems to have been referring to Seebeck's determination.

[2] O. Külpe, *Grundriss der Psychologie*, 1893, 108-111, 114; or Eng. trans., 104-107, 110 f.

[3] Cf. S. S. Stevens and H. Davis, *Hearing*, 1938, 152 f.

[4] C. É. J. Delezenne, Sur les valeurs numérique des notes de la gamme, *Recueil des travaux de la société des sciences, de l'agriculture et des arts de Lille*, 1827, 1-56, esp. 4-6. This frequency, usually cited as 60 or 120 c.p.s., is really 240 c.p.s., as E. B. Titchener, *Experimental Psychology*, II, ii, 1905, 235-237, makes clear; but Titchener, who did not see the original article of Delezenne, is wrong about thinking that the threshold should be 0.83 instead of 0.42.

Preyer used an elaborate set of reeds made by Appunn.[5] He confirmed Delezenne and Seebeck, and fixed the values for the twice and the thrice accented octaves. Thus far there was no use of psychophysical method: the limen was not a statistical quantity but the least difference observed under optimal conditions.

Luft used tuning forks with sliders to alter their pitch, and employed the method of limits.[6] Max Meyer decried the method of limits for this purpose, and with tuning forks determined the limens for Stumpf's ear by the method of right and wrong cases.[7] Actually he gave only the frequency of right cases; the limens in the table were calculated for him by Vance. These two studies introduce psychophysical method but are defective in that they rely on the manual actuation of the tuning forks and fail to control the intensity of the blow.

TABLE I

DIFFERENTIAL LIMENS FOR PITCH

These figures, which are all expressed in c.p.s., are the conventional values usually given for the investigators named. The conditions under which they were observed are discussed in the text.

Frequency	Delezenne 1827	Preyer 1876	Luft 1888	Meyer 1898	Stücker 1908	Vance 1914	Shower and Biddulph 1931
31, 32			.44				1.26
62–73			.15		.70	3.40	2.64
100–131			.16	.54	1.00	1.40	3.10
200–262	.42		.23	.25	1.30	1.40	2.64
400, 440				.28	1.40		
500–512		.30	.25			1.80	2.60
600				.24			
870					2.50		
1000–1200		.50	.22	.69		3.30	3.60
1740–2048			.36		7.70	5.70	3.80
3100, 4000					26.70		9.20
6200, 8000					304.00		23.20
11700							41.00

Stücker used forks up to 522 c.p.s., strings for 870 and 1740 c.p.s., and the Galton whistle above 3100 c.p.s.[8] Instead of trying to test maximal sensitivity, he interested himself in individual differences, with especial interest in the musical training of his *Os*. His results in the table are averages. There is no evidence that he used a psychophysical technique.

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[5] W. Preyer, *Die Grenzen der Tonwahrnehmung*, 1876, 26-37.

[6] E. Luft, Über die Unterschiedsempfindlichkeit für Tonhöhen, *Philos. Stud.*, 4, 1888, 511-540, esp. 528.

[7] M. Meyer, Über die Unterschiedsempfindlichkeit für Tonhöhen, *Zsch. f. Psychol.*, 16, 1898, 352-372, esp. 352-361.

[8] N. Stücker, Über die Unterschiedsempfindlichkeit für Tonhöhen in verschiedenen Tonregionen, *Zsch. f. Sinnesphysiol.*, 42, 1908, 392-408.

Vance was also concerned with individual differences and gives the data for 50 Iowa students.[9] He employed tuning forks, carefully and lightly struck, and he found his limens by the method of right and wrong cases with the *equal* category excluded. Table I gives his averages of the 50 *O*s.

Shower and Biddulph provide the classical results for the new technique with thermionic oscillators.[10] They objected to the hand-struck tuning fork because this method of actuation introduces overtones and transients that can become, instead of the fundamental, the basis of the judgment. Any abrupt interruption or initiation or change of a frequency necessarily introduces confusing transients. Shower and Biddulph chose, therefore, to vary the frequency gradually, changing the properties of the circuit according to a smooth sinusoidal function of frequency-change, mechanically controlled. They found that they got the smallest limens by varying the frequency at the rate of two changes per second, and they used this rate. They always began with equality and increased the difference until it was just noticeable.

TABLE II

EFFECT OF PRACTICE ON THE DIFFERENTIAL THRESHOLD FOR PITCH

Averages of Stücker's thresholds in c.p.s. for four groups with different training. N, number of Os, is small because only those Os are included who had limens for all 8 of the frequencies used in the table; otherwise the averages would not have been comparable.

Frequency c.p.s.	Colleagues N=6	Trained musicians N=8	Singers N=6	Unmusical Os N=4
73	.99	.25	.18	1.26
130	.90	.39	.32	1.40
261	.91	.43	.46	1.92
435	1.74	.57	.68	3.05
870	3.22	1.14	1.23	6.15
1740	10.61	2.76	3.07	15.25
3100	35.34	8.75	9.50	54.00
4176	127.37	29.25	38.50	109.00
Av.	23.13	5.44	6.77	24.00

So much for Table I. Now let us see what factors besides the fundamental frequency of the stimulus affect the size of the differential limen.

(1) Practice reduces the limen. From the very first the evidence has been that discrimination is best for the trained *O*, but Stücker's results make the fact sure. Table II shows how the limens are least for a group of trained musicians, larger for a group of singers, and largest of all for his unmusical observers. Thus it seems probable that Sauveur, Delezenne, Seebeck, Preyer, Luft (who used the members of his seminar in psychophysics as *O*s) and Meyer (whose *O* was Stumpf) ought to have gotten smaller limens than the averages of the less highly selected groups of Stücker and Vance, as indeed they did.

(2) Individual differences make the limen vary. Not all individual differences can be due to differences in practice and experience. The ear itself sometimes

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[9] T. F. Vance, Variation in pitch discrimination within the tonal range, *Psychol. Monog.*, 16, 1914, (no. 69), 115-149, esp. 126 f.

[10] E. G. Shower and R. Biddulph, Differential pitch sensitivity of the ear, *J. Acoust. Soc. Amer.*, 3, 1931, 275-287.

limits discrimination. Moreover, the mechanism of judgment, as it varies even under identical instructions, can affect the limen. Table III shows that difference between the largest and the smallest limen in each of Stücker's groups is much greater than the differences between the averages of the groups.

(3) The method of limits and the method of right and wrong cases do not yield comparable limens. The method of limits, as Luft used it, measured the average spread of the *equal* category. The method of right and wrong cases, as Meyer and Vance used it, eliminated the *equal* category, and took the semi-interquartile range of the psychometric function as the limen. Luft's limens are thus partly dependent upon the attitude of the *O* with respect to the meaning of the categories of judgment; Meyer's and Vance's are partly dependent upon the variability of the organism's adjustment to the stimulus. These are such different determinants that the correlation of limens got by the two methods ought not to be perfect and might not even be high. See also *(5)* and *(6)* below.

TABLE III

INDIVIDUAL DIFFERENCES IN DIFFERENTIAL THRESHOLDS FOR PITCH

Averages, maxima and minima of Stücker's thresholds in c.p.s. at 435 c.p.s. for four groups; cf. Table II. N, the number of Os, is larger than in Table II because all the cases for 435 c.p.s. can be used.

	Colleagues N=7	Trained musicians N=16	Singers N=19	Unmusical Os N=14
Av.	1.82	.56	1.93	4.79
Maximum	4.22	1.10	9.80	8.70
Minimum	.04	.10	.15	.30

(4) The limens of continuous change are larger than the limens for abrupt change.[11] It has even been suggested that they should be proportional to the rate of change but there are no good data. This factor is derived from the relation of the judgmental process to the differentiation of the sensory field. It is easier to perceive a difference when the impressions are separated by a 'contour' than when the one fades slowly, spatially or temporally, into the other. Shower and Biddulph compared gradual with abrupt change, and it seems probable that the larger limens

TABLE IV

EFFECT OF CONTINUOUS AND ABRUPT CHANGE UPON THE DIFFERENTIAL THRESHOLD FOR PITCH

Shower and Biddulph's comparison of the thresholds at 40 db for continuous (sinusoidal) change and abrupt change. All figures are c.p.s. They have been read with a lens from a printed graph and are not (for continuous change) the same observations as those given in Table I.

Change	Frequency							
	63	125	250	500	1000	2000	4000	8000
Continuous	3.01	3.22	2.50	2.90	4.20	6.00	11.20	20.00
Abrupt	.97	1.56	1.87	2.10	3.00	4.00	10.00	14.40

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[11] Cf. L. W. Stern, *Psychologie der Veränderungsauffassung,* 1898, esp. 208-243.

for continuous change at 2000 c.p.s. and above are due to this factor. See Table IV. Below 2000 c.p.s. much of the differences is presumably due, as they think, to transients and overtones. See *(7)* below.

(5) Fixed inequality between the standard and comparison stimuli favors smaller limens with the method of limits than with the method of right and wrong cases. Difference in the manufacture of forks or reeds may make it so that the nearest equivalence between the two stimuli is not nearly identity. The pattern of overtones or of transients may be different for the two instruments. If the *O* is to report *equal* in the method of limits, he has to make that judgment in the face of a difference. If the difference is clearly perceived, he abstracts from it only with difficulty. If it is not clear, it may nevertheless be effective. The result in the method of limits will be that *O* notes difference later in a series approaching equality, and earlier in a series that recedes from equality—thus making the limen less. In the method of right and wrong cases, as Meyer and Vance used it, the equality judgments are excluded and the question is not, as in the method of limits, as to whether there is still a difference, but as to which way the difference lies. Extraneous differences may then confuse the judgment but they will not aid it, as they do in the method of limits. This factor explains part of the discrepancy between the thresholds of Luft and of Vance but not the similarity between the determination of Luft and Meyer.

TABLE V

LEAST VALUES OF THE DIFFERENTIAL LIMEN FOR PITCH

The results of the best Os are averaged for Stücker and Vance. The data for abrupt change of frequency are given for Shower and Biddulph, as well as the other investigators. All values are in c.p.s.

Frequency	Luft Av.	Stücker Av. 2 best musical Os	Vance Av. 6 best Os	Shower and Biddulph Abrupt change
62–73	.15	.08	2.45	.97
125– 131	.16	.13	.81	1.56
250– 262	.23	.21	.68	1.87
435– 512	.25	.25	1.37	2.10
870–1024	.22	.50	1.98	3.00
1740–2048	.36	1.15	3.53	4.00

(6) Variable inequality between the standard and comparison stimuli also favors smaller limens with the method of limits than with the method of right and wrong cases. The striking of tuning forks by hand is subject to great variation, and the strike transients create an adventitious variability which favors smaller limens with the method of limits. The argument is that same as that of the preceding paragraph.

(7) The presence of overtones and transients tends to reduce the limen for those frequencies which are below the region of maximal sensitivity (ca. 2000 c.p.s.). It is obvious that extra frequencies, varying directly with the fundamental frequency and lying above it, would tend to become the basis of judgment when they lie within a more sensitive region. It is also possible that lower transients might work similarly for fundamental frequencies above 2000 c.p.s. Table IV, showing all through its range the influence of continuous change, shows also the great effect of transients and overtones, an effect diminishing from 62 to 2000 c.p.s.

(8) The weaker the tone the larger is the limen. Shower and Biddulph's work

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shows this relationship conclusively, but the effect is not great. The limen is nearly doubled only if the intensity is decreased from 40 to 10 db. This factor may enter into the difference between Vance's and Luft's results, since Vance seems to have tried to strike his forks gently, but it can not account for the main difference.

If we wish now in the light of our knowledge of these artifacts to lessen the discrepancies of Table I, we can largely eliminate the effects of practice and individual differences by taking the best *O*s for Stücker and Vance, as in Table V. We see at once that Luft's *O*s are no better than the best musical *O*s that Stücker had, so we may conclude that Stücker confirms Luft for thresholds got with hand-struck tuning forks. The limens for Vance's best *O*s remain higher, but that may be due to the fact that he used the method of right and wrong cases with hand-struck forks, instead of the method of limits, and furthermore that the two methods do not necessarily yield comparable limens. Since it is the procedure of the method of limits that gives the artifact here, Vance's results should presumably be preferred to Luft's. When we now compare Vance's best limens, got by abrupt change, with the limens that Shower and Biddulph got by abrupt change (Table V), we no longer find any great discrepancy. Vance, on the whole, finds smaller limens than Shower and Biddulph, a difference which may indicate that Vance had more transients than Shower and Biddulph.

There seems to be at the present time no way of determining the maximal sensitivity of the ear to difference of frequency without artifacts. For maximal sensitivity and comparable results, one should choose the most practised and most sensitive *O*s. The tones should be generated with thermionic oscillators and intensity should be controlled. If transients are not excluded the method of constant stimuli is preferable to the method of limits. In any case the method of limits should balance series in one direction by other series in the reverse direction, as Shower and Biddulph did not. The fundamental dilemma lies in the need to get rid of the transients that come from abrupt stimulation while still avoiding gradual transition from one stimulus to the other. The ideal is abrupt change without transients. Near 2000 c.p.s. abrupt change is presumably safe because the transients do not furnish good cues there. At low frequencies (*e.g.* 256 and 435 c.p.s.) maximal sensitivity probably lies somewhere between the indications given by abrupt change and by gradual transition. The 1500 discriminable frequencies given by the results of Shower and Biddulph are thus probably too few to express maximal sensitivity, but the 11,000 tones from Luft's data must be too many—very much too many.

Harvard University — EDWIN G. BORING

MORE ABOUT THE 'BABOON BOY' OF SOUTH AFRICA

The note by Dr. John P. Foley in the January number of this JOURNAL (*supra* 128-133) was published prematurely before Dr. Raymond A. Dart and others in South Africa had completed their investigation of Lucas. The evidence now available appears clearly to disprove him as a feral case.

The recent resurgence of interest in the Lucas case is due to the writer's interest in feral material. In responding to a question relating to the 'Wolf children' of India.[1] Dr. Arnold Gasell of Yale University sent me a newspaper clipping reporting the case of Lucas. I relayed it to Dr. R. R. Gates of the University of Bristol, England, and he in turn sent it to Dr. Dart of the University of the Witwatersrand, Johannesburg, South Africa, who uncovered the police evidence which, though hearsay, made the case worthy of further investigation.

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[1] Cf. P. C. Squires, "Wolf children" of India, this JOURNAL, 38, 1927, 313-315; W. N. Kellogg, More about the "wolf children" of India, *ibid.*, 43, 1931, 508-509; A further note of the "wolf children" of India, *ibid.*, 46, 1934, 149-150.

The early materials concerning the case of Lucas, accurately reported in Dr. Foley's note, were released to Dr. Foley and to others in keeping with Dr. Dart's generous policy of sharing his materials at all stages of the investigation with interested scientists. There was no thought on my part of publishing the materials until the investigation was complete.

Dr. Dart enlisted, in his investigation of Lucas, the assistance of Dr. E. G. Dru Drury, 120 High Street, Grahamstown, South Africa, and Dr. J. A. van Heerden of the Mental Hospital at Grahamstown—the institution where Lucas was treated in 1904. In a letter, recently received from Dr. Dart, copies of the reports of these colleagues were enclosed. The letter and reports are reproduced below.

UNIVERSITY OF THE WITWATERSRAND
JOHANNESBURG
Medical School

12th March, 1940

Professor R. M. Zingg
Department of Anthropology
University of Denver
Denver, Colo.
U.S.A.

Dear Dr. Zingg:

I enclose herewith a copy of Dr. Dru Drury's report on Lucas, the so-called "Baboon Boy," and also a copy of Dr. van Heerden's report.

With all the information now in our hands, it seems incontestable that the story, however circumstantial it appeared at first, proves a fiction.

With kind regards,

Yours sincerely,
(Signed) Raymond A. Dart
Professor of Anatomy

GRAHAMSTOWN MENTAL HOSPITAL

30th January, 1940

Professor Dart
Professor of Anatomy
University of the Witwatersrand
Johannesburg

Dear Professor Dart:

In company with Dr. Dru Drury I visited and examined Lukas—the so-called 'Baboon Boy'—on the 29th November, 1939. This native, whose age is computed to be now about 48 years, shows facial and anatomical characteristics of a cross breed between a native and a coloured or bushman.

His expression is fatuous and silly and this is aggravated by being partially edentulous. His demeanour is childish. He is clumsy and ungainly in all muscular movements, and stands with his feet splayed out. He salivates freely and, accordingly, "splutters" when he talks.

His language is English and he appears to know the significance of few, if any, words in Afrikaans or Native. He has a poor vocabulary in English but he is able to express himself fairly well.

He is reminiscent and appears to have a compulsion to relate how he acquired the scar on his cranium. He uses the expression "Koelie kicked me." The word 'Koel' is used in some parts of South Africa by the farming community in referring to an ostrich.

His intelligence is that of a low grade imbecile. While his coöperation can be readily gained for the moment, he is distractible and it was found impossible to complete any standardized intelligence tests on him. He is unable to count to 5. He shows no planning capacity and has a total disregard of form or contour as shown by his handling of the Sequin Board.

He has no knowledge of money values—he calls a penny, shilling, and sixpence all "Oom Paul." This may refer to a period following the Boer War when coins of the South African Republic had an enhanced value and were prized and sought after and it has to be assumed that he acquired this term and other expressions, which he repeats, from the children of his mistress.

I cannot accept the story that this native was reared by baboons.

Yours sincerely,

(Signed) J. A. van Heerden
Medical Superintendent

A/AS

SUMMARY OF DR. DRU DRURY'S INVESTIGATIONS OF LUCAS

The first sight of Luke, on 24th November, 1939, was unexpected by the general practitioner, if not by the alienist. As we drove up to the homestead on the farm Thorn Hill, which has been Luke's setting for 35 years, he was standing inside a fence which enclosed a garden.

His khaki shirt and grey flannel shorts were newly washed and so was Luke. He grinned, showing defective teeth, and raised his hand in a salute. The general effect of clean preparation recalled many formal visits of inspection to institutions where a good impression on the visitor is desirable.

It appeared that Luke had been ready and standing to attention for more than 2 hours. As we passed to the house, this misunderstanding and the need for discussion of finance led to further delay. The lady of the house instantly denied the identity of Luke with the Lucas photographed in the Mental Hospital in 1904 and loudly called on the rest of the party, who were dining in an adjacent room, to support her. When a working arrangement, if not cordiality, had been reached, Luke was still standing in the burning heat, and still smiling—the well trained orphan, though nearly 50 years of age.

Luke followed us to the gable end of the stable where field measurements, photographs, and plaster impressions of hand and foot were taken. Luke was unwilling to take off his clean two-piece suit and was sluggish in his movements. The orders were given him by a man he had known since 1904, who spoke in simple English. Similar muscular clumsiness was apparent at his visit to the X-ray Department at the Settlers Hospital on November 30th. Getting off and on to the table, or posing on all fours, Luke showed no agility of movement. On starting the examination, the orphanage boy effect gave place to that of the natural or village idiot boy. Given a white tennis shirt, Luke seemed pleased. Offered sweets, he did not fill a cheek pouch but put them in a pocket and ate a few deliberately and without greed.

His measurements do not differ materially from those of any half-breed. His forearms are very well developed and so are his back muscles. This may well be due to 30 years work as blacksmith's striker and bellows blower. It is related that he could keep up such work for the whole day and never asked leave to go and relieve himself. The results to his clothing were disastrous, unless he got an order to go to the bush.

His general build, prominent buttocks and long head suggest an admixture of Bushman or Hottentot blood, under his kafir skin.

For what it is worth one may quote a statement ascribed to Luke, and assented to by him in our hearing, that his mother ran away with a Hottentot named Kleinbooi. One cannot feel, however, that any statement of Luke's is worth credence. He did not volunteer any remark except that one of us was "a good doctor." This was on the second meeting at which also sweets were provided. None of his answers to leading questions would have survived a cross-examination.

It will be apparent that the front point of the semi-circular scar, and the beginning of the almost vertical depressed furrow in the skull are within an inch of the region of Broca's convolution in the fore brain, in the motor area for speech.

Three months of preliminary research had discovered his official record, all too scantily recorded by the late Dr. W. L. A. Leslie, and a photograph taken by W. S. Colegate. It has provided interviews with Mrs. Ethelreda Lewis and Mr. W. Muscott, postmaster at Port Alfred W., who are principally responsible for telling South Africa, England, and America, since 1928, about the history of Luke, as told by his deceased employer; and it has traced, with the help of Mr. F. R. Paver, editor of *The Star,* the earliest known reference to Luke in print, which appeared in *The Star* of 16th July, 1927.

There was an album published for publicity purposes. It contained a photograph and a few words summarising the baboon story, both probably due to Mr. H. Miles, "a member of the staff of the Railway Department Publicity Office." This album and picture postcards of "Luke, the Baboon Boy" priced at 1/6 used to be sold by Mr. T. Brandt, who was proprietor of the Grand Hotel, Port Alfred, during some years, from about 1928 to 1932. A copy of this photograph was obtained from the present proprietor, Mr. W. W. MacFarlane; it was his last copy.

Investigation of the police evidence supplied by Lt.-Col. C. J. T. Horak, Dept. Commr. of S.A. Police, Eastern Division, showed that actually no record bearing on the case exists, so far as is known. In 1921, that is, 17 years after Luke's admission to the Mental Hospital, the late Lance-Sergt. Charles Holsen, on a single occasion, related the story of the baboon troop to Const. W. J. Coetzer. When the latter came to Kafir Drift station, 5 years later, he heard the local story and recalled Holsen's account, which he had heard now 18 years before. It must be hard for any man, however trained in evidence, to separate Holsen's words from those of George Smith, who habitually shoed the police horses.

The value of Luke's "statement" of 152 words of narrative to (M) Const. G. G. Wright on 8th May, 1939, at the instance of the Deputy Commissioner of Police must be heavily discounted, in view of the mental processes of Luke.

Evidence as to the existence of the baboon story in 1904 is hard to obtain. Dr. Leslie's notes make no reference to it. They trace Lucas to Burghersdorp Gaol as an indoor pauper; "nothing being known about him or his people." Dr. T. D. Greenlees, the medical superintendent (now deceased) was on leave at the time. [Krisch—confirmed by Dr. C. G. Cassidy.]

The age of this acutely maniacal Kafir herd boy is given on the hospital records as 13 years, but the photograph suggests a boy of 10 years or less. He is identified with Luke by the scalp wound and X-ray evidence of what in 1904 was a badly united fracture of the right tibia. He was certified at Burghersdorp by Dr. Herbert Caiger and the late Dr. J. Tandy Bolger on 19th and 20th February, 1904, respectively, and the condition ascribed to his head injury. He was emaciated, dirty and destructive in his habits. He used English speech then and in the Mental Hospital.

On 20th February, 1904, a Basuto shepherd, aged 45, was also certified by Drs. Caiger and Bolger.

On 30th March, both patients were admitted to the Grahamstown Mental Hospital. In the hope that such a dual event, especially if the Baboon story were then in existence, might be recalled by him, I wrote to Dr. Caiger, now of Sheffield, Yorks. He has no recollection of the occurrence. With his help I am trying to trace one of the gaolers (Proffitt, Pearce, and Winnacott) who might have held office at that time.

The period of escort from Burghersdorp to Grahamstown alone remains to be traced. If that took place by train, Luke's history from February, 1904 up to date would be accounted for by documentary evidence.

The baboon story must then be shifted from Driefontein, Koonap, and placed in the neighbourhood of Burghersdorp, miles away, or it must be discarded as a baseless piece of folklore, as applied to Luke.

George Howell, 6th November, 1939, said: "I am 84 years old. I was in the native wards at Fort England in 1904. I knew Lucas when he was in the native wards. We did not hear anything about the baboon story when he was there. I reckon it was made up afterwards." [The latter was volunteered; not given in answer to questions.]

A. Hobley, now of Kroonstad, says that he cannot identify Lucas. "I remember the case but cannot remember under what name he was admitted." He had read the *Outspan* and other accounts "a few years ago." [The boy was admitted as "Lucas," although George Smith said that he had given him the name.]

Wallace Stephen Colegate, aged 73 years, who died a few weeks later, had photographed Luke in 1928 and 1930 for Mrs. Lewis and Mr. Muscott respectively. He, living at the Kowie, was accustomed to the story. He said: "I used to photograph cases as a routine [at Fort England]. There were then two blocks for native patients. Old Van der Merwe, the attendant, brought Lukas to me for a photograph and said 'This is the boy who grew up with baboons.' Lukas was in the hospital for some weeks [actually 10 weeks]. I am pretty sure he was certified [correct]. I very much doubt the truth of the story. No one at the Mental Hospital took it seriously." Colegate noted that leading questions put by Mr. Muscott, about raw food and insects, had on a later visit been incorporated in the legend. [Colegate's facts are correct. Native admission to the hospital began in 1875 and ended in 1908, when such cases were transferred to Fort Beaufort.]

George Edward Krisch was a mason at the Asylum in 1904 and struck Dr. Cassidy, who interviewed him at 2 Kyle Street, Main Road, Strand, Cape, as a fine type of man physically, intelligent and reliable.

Krisch stated correctly that Dr. Greenlees never saw the boy Luke and that Dr. Leslie took the notes, but he denied that he was certified. "The boy could walk erect, but frequently went on all fours and used to fossick among the bushes looking for crickets, which he ate. The staff learnt about the boy from the press and were all curious to see and observe him and soon concluded that the story about him was all mere nonsense. He spoke English and knew no native language. He seemed to Krisch to be a boy of 7 or 8 years of age." [Dr. C. G. Cassidy—interview.]

Krisch confirmed attendant Bruce's sudden death while on duty at Valkenberg Mental Hospital of heart failure, some time in 1925. This is confirmed by ex-head attendant J. Linckner, who assisted at his post-mortem. Dr. J. A. van Heerden confirmed this.

Mrs. Bruce was certified (by Drs. R. C. Mullins and G. C. Purvis) on 2nd and 3rd August, 1905, at the age of 27 as recurrent melancholia and was discharged 'relieved' 6 mo. later. It is not known whether there were any children. In any case no "Bruce" evidence is available as to the length of time Luke stayed with them in Belmont Valley. The Hospital records say "a week's trial." Rupert Smith says one month.

If this is correct, the date of the photograph showing the exodus to Thorn Hill would have been September 1904 (or early in 1905, if Bruce returned him to the Mental Hospital as alleged). [The Smith baby in the photograph is Amy, born 1904 and apparently under one year old.]

If one may refer to interviews with the two apostles of the legend, Mr. W. Muscott said "I never believed the baboon story, but it had good news value and will in time become the classical African story." He states that he wrote the *Outspan* story, "The truth about the 'Baboon Boy,' " which appeared over the signature of George Harvey Smith, on 4th September, 1931. Rupert and Ted Smith agree that the language is quite beyond the capacity of G. H. Smith, who supplied the facts only.

Mrs. Ethelreda Lewis is mainly desirous of saving Luke from exploitation. She was misled by the Smiths on two points. Lucas had a name in Burghersdorp and could then talk English. Mrs. Lewis says he must have lived somewhere, which is a truism. She also says that a tale does not originate without some basis. The basis could equally have been a local joke elaborated in the hope of gain.

Ted Smith states that a neighbour (L.K.) told the Smiths that Luke would be worth 35,000 Pounds to them at Hollywood. Mr. Muscott was in touch with a New York Film Firm (21st October, 1930 to 14th January, 1931) who were working a jungle serial, but the negotiations fell through.

G. H. Smith's attempt to exhibit Luke as the baboon boy at East London was a fiasco. He just cleared expenses (Rupert Smith). There was a disturbance (Mrs. E. Lewis) and the police closed the show (A. Hobley).

The difficulty in obtaining first hand evidence of happenings in 1904 is increased by the death of so many of the witnesses. These include Dr. James Tandy Bolger of Burghersdorp; Dr. W. L. A. Leslie, later of Tsolo; Mr. George Smith, Sr., night attendant; Head attendant Walters; Tom Wesson, male attendant; Male nurse Bruce and Mrs. Bruce; J. Lewis Smith, attendant (killed, 1910); George Henry Smith, his employer; Stephen Wallace Colegate; Sergt. Chas. Holsen (died of pneumonia in 1924); Sergt. J. P. Venter (died of dropsy at Port Alfred, 1936).

Mrs. George H. Smith still employs Luke, who has been a cheap and valuable servant. She denies all knowledge of the history and was obstructively reticent and grudging of information.

Dr. J. A. van Heerden found Luke unable to pass the moron tests and will report on his defective mentality.

To sum up my own opinion, Luke or Lucas was a normal half-bred boy until his brain was damaged by the accident which fractured his skull in childhood.

The arrest to his development has not been helped by his later history and environment.

The story is his bullying by persons named by Ted Smith and the suggestions enforced by hot cigarette ends, to my mind explain any story about baboons which Luke is supposed to have told.

I can find no evidence, beyond the story ascribed to Sergt. Charles Holsen, to support the belief that Luke ever lived with baboons, even if the police found him in the neighbourhood of a troop of the animals.

(Signed) E. G. Dru Drury
10th January, 1940

Const. W. J. Coetzer, No. 9563, engaged in tracking stock thefts, interviewed on Tuesday, 25th October, 1939, said:

"I was born in Alice District and speak Kafir. In 1921 I was sent from the Depot in Pretoria and stationed at Spring Mount, Alexandria. Sergeant Charles Holsen was in Command there. He told me the story of the boy with the baboon troop. He did not tell it more than once, but when I came to be stationed at Kafir Drift in 1926 we used to get our horses shod at Smith's. I saw the boy and recollected Holsen's tale. I heard it also from George Smith. I saw the boy catch and eat a cricket in the forge.

"Holsen and Sergeant J. P. Venter certainly believed the tale and were not the sort of people to make up a yarn. He described the spot as the flats on the Bathurst side of Kafir Drift. The native story was that years before, in the Peddie side of the Fish River, natives saw a baboon carrying off a baby. Native superstitions would prevent their claiming the recovered boy. The whole family might have been murdered as witches.

"George Jesse, an ex-native policeman, heard the whole story from an old native, now dead. He will try to recall the name. There is a reliable headman in Mbokatwana location on the Peddie side of the drift whom I will see. He can perhaps help.

"Holsen said that they kept the boy for some weeks at the Trapps Valley Station and, finding him speechless, sent him in to the doctor for examination.

"Holsen died in the Grahamstown Hospital in 1924. He came in for a musketry course and got pneumonia.

"Venter died two or three years ago.

"Holsen belonged to the old C.M.P. He did not patrol outside the Bathurst District. I do not know whether he was ever in the Burghersdorp area."

(Signed) E. G. Dru Drury

RECORDS FROM THE ARCHIVES OF GRAHAMSTOWN MENTAL HOSPITAL

Case record of Lucas. Admitted 30/3/04; registry number 2679. From Burghersdorp. Age 13 years. Kafir. First attack—duration 2 mo. Single; herd boy. Cause, injury to head. Weight 56 lb. Certified by Doctors Caiger and Bolger in February, 1904. "Foolish and nervous manner. Laughs without cause and tries to stand on his head. He is destructive to his clothes and dirty in habits. Said he was a goat, also that he was a sheep."

History of case. He was admitted to the Burghersdorp gaol as an indoor pauper, nothing being known about him or his people. While in gaol the Gaoler reports that he was destructive and behaved like a lunatic, though quite harmless.

Condition on admission. Little "niggar" boy with very mischievous expression. Very emaciated and having a large semilunar indentation over the left side of the skull for about 5 inches from tip to tip. This, he says, is the result of a kick from an ostrich. Right tibia shows a badly united fracture.

Notes. (31/3/04): "Quite happy, laughing and saying this is a very comfortable place. His temperature is normal today and he eats ravenously anything within reach." (7/4/04): "Very bright youngster, but rather incoherent in his replies to questions. He is in good health, eating and sleeping well." (1/5/04): "Remains the same. In good health and spirits. (10/5/04): Lucas has gone on a week's trial to stay with Attendant Bruce. Being the youngest member on the Native side and inclined to be very cheeky and impertinent to his elders, he was continually in danger of being assaulted." (15/5/04): Discharged as recovered, to remain with Attendant Bruce. The diagnosis was "Acute mania" and he was neither epileptic, suicidal, nor dangerous.

(Copied by Dr. C. G. Cassidy)

SOUTH AFRICAN POLICE

Office of the Deputy Commissioner
Cape Eastern Division
Grahamstown, South Africa
1st March, 1940

Dr. E. G. Dru Drury
High Street
Grahamstown

(Enquiry re Baboon Boy—Lucas)

Sir:

With reference to our conversation relative to the above matter, I have the honour to inform you that Constable Coetzer has again been seen by me. He informs me that the late Sergeant Holsen definitely told him that he (Holsen) caught this native as described by him in his report of the 29th April, 1939.

Full facts of the herd boy Luke and the Basuto shepherd called "August" certified at Burghersdorp and later admitted at Grahamstown were explained to Constable Coetzer. He states that although Holsen was a very truthful man, he (Coetzer) now feels convinced that "Lucas, the Baboon Boy" is identical with Lucas certified at Burgersdorp and later admitted at Grahamstown.

In my report of 1st May, 1939, I stated that there is no reason to believe that the story of finding of the "Baboon Boy" by the Police is not authentic.

At that time I based my belief on the facts then revealed, but considering the evidence now brought to light, I am definitely inclined to the view that the boy Lucas certified at Burghersdorp is identical with Lucas the supposed Baboon Boy.

I have the honour to be,
Sir,
Your obedient servant,
(Signed) O. J. T. Horak (Lieut-Colonel, Deputy Commissioner
S.A. Police Commanding Cape Eastern Division)

OJTH/SC

A summary of the evidence in the case of Lucas reveals the following pertinent points.

(1) According to report, Lucas was captured by two troopers of the Cape Police; according to documentary evidence, Lucas was in the Burghersdorp gaol at the time of his reported capture.

(2) The Bathurst side of the Kafir Drift, the place of his reported capture, and Burghersdorp, where he was incarcerated as an indoor pauper, are widely separated. It would have been impossible for Lucas to have shuttled between the two places.

(3) According to report, the officers making the capture took Lucas to the Grahamstown Mental Hospital; according to documentary evidence, Lucas was transferred to the Grahamstown Mental Hospital from the Burghersdorp gaol.

(4) The story of Lucas's feral existence was not current at the time he was at the Hospital. It did not appear upon the Hospital records and it was not known by report among the attendants or other patients.

(5) The story appeared after Lucas was given into the care of George H. Smith, who attempted to exploit the tale by exhibiting Lucas as a "baboon-boy." It was, therefore, to Smith's interest to promote the story—which he did vigorously.

(6) The retardation of Lucas and much of his anomalous behavior may be accounted for from a brain injury over Borca's speech area, which is indicated by a 5-in. semilunar fracture in the skull—probably caused by the kick of an ostrich. The present Lucas of Smith's Thorn Hill Farm is positively identified with the hospital records by a recent x-ray of the old skull fracture.

Dr. Foley's note on "The 'baboon boy' of South Africa" was accurate for the time it was written. Its publication, however, was precipitous, as the recent evidence clearly shows. The Lucas case indicates how thorough investigation must be in matters of this kind, and how cautious and conservative scientists should be in accepting and in sifting the evidence for feral man.

University of Denver ROBERT M. ZINGG

THE ELEVENTH ANNUAL MEETING OF THE EASTERN PSYCHOLOGICAL ASSOCIATION

The eleventh annual meeting of the Eastern Psychological Association was held at Chalfonte-Haddon Hall, Atlantic City, on April 5 and 6, 1940, with an attendance of 356 members and 199 guests, making a total of 555. While the total attendance was more than 100 below the number attending the meeting at Bryn Mawr in 1939, several compensating factors must not be lost sight of. Since this was the first time that the annual meeting was held away from a collegiate institution and a large center of population, the attendance must be regarded as very satisfactory indeed. On the other hand, the number present at the Presidential dinner, 270, was the largest on record for this function. It should also be noted that in spite of the slight reduction in attendance, the number of dues-paying members and guests, hence the income of the Association, was greater than in any previous year. The total income amounted to $630.57 which leaves a net surplus of $231.16 for the year. Enthusiasm and interest were high and many members expressed a preference for meetings at a hotel such as this one was. Taking everything as a whole, the Association must be regarded as having made progress during the past year while trying out the new experiment of meeting under unusual circumstances and in a different environment.

There were 17 sessions featuring 109 papers, each paper having 15 minutes for presentation and 5 minutes for discussion. On Friday morning 37 papers were given in six sessions under the following topics: Methodology and Statistics, Applied Psychology, Cutaneous and Auditory Processes, Physiological Psychology, Social Psychology, and Perception and Memory. In the afternoon there were five sessions with 32 papers in the following categories: Brain Function, Conflict and Motivation, Visual Processes, Tests and Measurements, and Films. Under this last rubric were included papers requiring films and a number of instructional films produced at Columbia University by C. J. Warden, T. A. Jackson and G. M. Gilbert. Sandwiched in between regular sessions and the dinner hour late Friday afternoon were six Round Tables which are becoming increasingly popular at the meetings: Selection and Training of Civilian Airplane Pilots, J. G. Jenkins, University of Maryland, Chairman; The Effects of Frustration, A. H. Maslow, Brooklyn College, Chairman; The 1937 Revision of the Binet Test, R. T. Rock, Fordham University, Chairman; The Psychology of Social Change, Ross Stagner, Dartmouth College, Chairman; The Licensing of Psychologists, R. C. Challman, Columbia University, Chairman; and The Census from the Standpoint of Social Psychology, J. B. Maller, New York University, Chairman.

After a meal which all agreed was really a 'banquet' the President of the Association, F. L. Wells of Harvard University, was introduced by the previous President, K. M. Dallenbach of Cornell University. The subject of President Wells' address was "Mental measurement and college objective." The address threw new light on college objectives both from the point of view of officers who formulate them and the students who are subjected to them. It was pervaded with a quiet humor that took the sting out of some of the facts brought to the attention of the audience. Since the address is to appear in print no attempt will be made here to give a better idea of its contents.

Six sessions in which 40 papers were given were held Saturday morning on the following subjects: Abnormal Psychology, Learning and Conditioning, Child Psychology, Social Attitudes, Personality, and Brain Function.

Owing to the need for time to consider the new by-laws, which were prepared by a Committee for Revision of the By-Laws, the general session was omitted and the whole of Saturday afternoon was allowed for the annual business meeting. The following business was transacted: a budget amounting to $510.00 was adopted for the year 1940-41 with an amendment that if it is necessary to exceed this budget in the execution of the new by-laws the Directors may authorize necessary increases in expenditures; a motion was passed supporting the stand of the New York City Board of Higher Education in its appointment of Bertrand Russell to a Professorship of Philosophy in City College; another motion bearing on this questoin was then passed according to which no publicity shall be given to the motion supporting the Board of Higher Education without a statement of the total membership of the Association, the number attending the business meeting, the number voting for the resolution and the number voting against. There were 75 present at the business meeting, with 37 voting in favor and 23 against the Russell motion.

Before taking up consideration of the new by-laws, Douglas Fryer of New York University, Chairman of the Committee on Revision of the By-Laws, read a paper entitled "The new by-laws in relation to our history." In his address Professor Fryer gave an illuminating account of the origins, problems, development, and present status of the Association backed up by statistics pertinent to the chief points of interest. The members present at the business meeting voted unanimously to accept the recommendation of the Directors that the new by-laws be adopted in principle for one year with final action to take place at the next annual meeting after making any detailed changes necessary to translate the by-laws into action. The chief changes in the new by-laws are as follows: membership is limited to properly qualified individuals who are not more than two years in arrears in dues; guests will pay the same fee that members pay in dues (with certain exemptions for relatives of members); the Nominating Committee is eliminated and members will henceforth present one candidate each for President and two members of the Board of Directors on a nominating ballot, and will vote by preferential ballot for a certain number nominated for each office in a second mail ballot.

New officers were elected as follows: W. S. Hunter, Brown University, President, 1940-41; Harry Helson, Bryn Mawr College, Secretary-Treasurer, 1940-43; Elmer Culler, University of Rochester, and Irving Lorge, Teachers College, Directors, 1940-43. The Program Committee appointed by the Directors for 1940-41 consists of D. G. Marquis, Yale University; L. Max, New York University; and R. A. McFarland, Harvard University, Chairman.

With the passage of motions thanking Douglas Fryer for his work as Chairman of the Committee on Revision of the By-Laws, the Secretary-Treasurer and his staff for their work at the meetings, and the management of Chalfonte-Haddon Hall for the excellent facilities and handling of the various matters connected with the meetings, the business meeting came to a close. The next annual meeting of the Association is to be held at Brooklyn College on April 18 and 19, 1941.

Bryn Mawr College HARRY HELSON

BOOK REVIEWS

Edited by JOHN G. JENKINS, University of Maryland

American Psychology Before William James. By JAY WHARTON FAY. New Brunswick, N.H., Rutgers University Press, 1939. Pp. x, 240.

Everybody says that James and Ladd and Hall imported into America the "new psychology" from Germany and that before them there was no American psychology. Such an assertion is, of course, improbable on its face, for there must have been in the American colleges and universities some discussion of the philosophy and nature of mind. Mr. Fay set himself to resolve this contradiction, to find out just what psychology had been thought and taught and wrought in America from Henry Dunster in 1640 to William James in 1890; and an excellent encyclopedic job he has done! All the facts are now in his book—that is to say, there are enough of them in exact quotation, in summary paragraph, in explicit biographical note, in precise commentary on bibliographical citation, to enable the careful student to cull the data, to find his way to the literature, or to reconstruct the total picture adequately.

Whether the picture turns out to be dreary or engrossing depends upon the reader. Mr. Fay is absorbed by it and champions the neglected Samuel Johnson, Thomas Upham, and Noah Porter, attempting to rescue them from the oblivion in which they have been unjustly immured. That is right, for an author should be loyal to his subject; but I, who am interested in the past, not, be it admitted, for its own sake, but because it leads to the present—I found the book chock full of information and utterly dull. The past does not necessarily lead to the present even though it come first; dynamic relations between successive events have to be positively established if the late is to be explained by the early. So we have the right to ask: What did James and Hall owe to Porter and McCosh? I still am convinced that James and Porter or Hall and McCosh are as incommensurable as were their European prototypes, Helmholtz and William Hamilton.

Mr. Fay's book, after despairing of any modern's understanding the Puritan mind, treats of two and a half centuries in five periods. (1) The first is the period of scholastic education after the English pattern (1640-1714), for which the author quotes from the note-book of Samuel Johnson, then an eighteen-year-old tutor at Yale, to show what American academic psychology was like in the days before the arrival of Locke's *Essay* from England. (2) The second period (1714-1776) dates from the arrival of the *Essay* and shows Samuel Johnson quoting, practically verbatim and at length, from it in 1752. Meanwhile Bishop Berkeley himself had been in America for two years and stimulated beliefs in empiricism and idealism. Jonathan Edwards and Thomas Clap were the other important Americans who spread this influence. (3) Next came the period (1776-1827) of Scottish philosophy, of faculty psychology, followed (4) by the "era of American textbooks" (1827-1861). Scottish psychology, Mr. Fay says, was diluted, after fifty years of supremacy, by an admixture of Cousin, Kant, and Coleridge. Important texts were published, as, for example, by Upham (1827), "the Sully of America." Benjamin Rush started abnormal psychology (ca. 1812). (5) Finally there was the period of British and German philosophical influence (1861-1890), where the important names are James Rush (Benjamin's son), Noah Porter, James McCosh (Scottish in person and thought), and B. P. Bowne. They knew their minds and selected their influences. It was not Darwin and Galton in England nor Helmholtz and Wundt in Germany who fired their imaginations!

The Notes are forty-seven pages of fine print to which is added a useful eight-page chronological table. Although they are inconvenient, being separated from the text and yet not making sense without the text, they have in them a mine of biographical and bibliographical information and exhibit clearly the author's careful and unwearied scholarship.

Mr. Fay does not wholly ignore the dynamic relationships of historical evolution. He exhibits with assurance the effect of Locke and the later effect of Reid, and he traces both influences along their courses. At other times, however, he seems to me to mistake sequentiality for causality, or at least he implies too much. Take these sentences: "Behaviorism did not spring Minerva-like fully grown from the brain of J. B. Watson. It was announced half a century before by his John the Baptist, James Rush" (p. 145). In addition to the difficulty of imagining Watson simultaneously as the Chief Olympian and the Prince of Peace, there is the confusion that arises from the simple fact that all sorts of objectivists, besides Rush and better known, anticipated Watson, who was himself an excellent founder of a school and therefore—on the view that movements can not be founded until after they have been prepared—not so good an originator.

To Mr. Fay's horrification—there could be no doubt about it—I venture to suggest that one important use for his book is to make some ignorance safe. One is never safe in being ignorant unless he knows what he is being ignorant of. Mr. Fay tells us what we do not need to know. His is a book to own on the shelf and use at need.

What, then, has become of the thesis of this book, that all this American psychology, from Samuel Johnson to McCosh, being the antecedent of modern American psychology, is also its partial explanation? Can wisdom be complete if ancestry be ignored? Well, Mr. Fay does not convince me that he has got the genealogy right. "American Psychology Before William James" died, I think, without issue. Living in the old house are some immigrants of mixed blood, who stem in part from Darwin and in part from Helmholtz or from their kin. These immigrants have even moved in their own furniture.

Harvard University EDWIN G. BORING

Introduction to Psychology. Edited by E. G. BORING, H. S. LANGFELD and H. P. WELD. New York, John Wiley & Son, 1939. Pp. xxii, 652.

Although the present volume is essentially a revision of *Psychology: A Factual Textbook,* it has undergone such major changes that it merits detailed reconsideration. No one will quarrel with its premise that the "Facts should be presented to the young student of Psychology free from the bias of metaphysical presuppositions or of psychological systems," yet one can not escape the conclusion that, at the present stage of psychology, such bias is intrinsic to a presentation of the material. An author may, for pedagogical reasons, omit theoretical discussions and avoid controversial points, but whether he states and discusses his systematic and metaphysical biases he uses them in the selection of his facts for presentation, and in his relative emphasis upon various sections of the subject matter.

In the *Introduction to Psychology* the most striking systematic note is the strongly social or sociological point of view which is emphasized by an inversion of the order of presentation from that of the earlier book. The change was made partly in response to a popular demand and partly for pedagogical reasons. Psychology becomes, now, the search for an answer to the question "What is man?" The program as outlined in the first chapter is to study "persons," first in their "social relations" and then as "single individuals." The second chapter deals with "social behavior," the third with "personality," the fourth with "individual differences," and the fifth with "motivation." At this point the evaluative attitude gives way somewhat to the study of the data of experience and of physiology. "Emotion and Affective Responses" are discussed. Interest in them derives from a consideration of "needs," in which physiological labels already have been utilized, but the treatment remains interpretive. With physiology thus introduced, a detailed consideration of the "Response Mechanism" and of "Response" in general follows. Then "Learning" and related forms of behavior, such as "Retention," "Recollection and Imagination," and "Thought" are discussed in that order. The final third of the text is devoted to "Perception" and "Sensation." "Perception is the apprehension of immediately present objects and events," and as a consequence, the chapters present many disparate items of that type of knowledge without offering any unifying concepts of integration or organization of descriptive data. "Sensation is the core of perceptions. It is what the organism would perceive if it had only the nature of the stimulus and the stimulated sense organ to go upon." The nature of the stimulus is analyzed in some physical detail for each sense department. Likewise the anatomy of the sense organs is described and expositions of the associated physiological hypotheses are given.

The "modernistic approach" involved in the rearrangement of topics and in the emphasis upon social interpretations follows certain trends of current psychology, but as Fernberger has pointed out, such trends run in cycles in psychology as elsewhere. The impression one gets of the treatment as a whole is strongly reminiscent of James, in that it begins with gross phenomenal entities—society and man—and leads toward simpler phenomenal entities of the same order—responses and sensations. "Sensation," now as then, is an epistemological unit, the simplest item of knowledge. Whether it has ever been anything else is arguable.

Although teachers of psychology seem to have found it too matter of fact and lacking in "thrill" to offer student "appeal," the concept of sensory data comparable with the data of other sciences has been experimentally fruitful. Data ordinarily form the material out of which scientific systems are built by interpretations under the laws of logic. When data are relegated to the end of the discussion, however, they lose this function. It is not surprising, therefore, that much "of the scientific details of those fields which are today richest in established facts" has been omitted. Modernization, it seems, has reversed the order of reasoning so that we go, now, from interpretation to facts—but then the facts seem of less significance. Hence Chapter 20 consists of one paragraph the thesis of which is "Da Capa!"

We have not forgotten, however, that the major purpose of a text book is pedagogical. It may be that systematic sacrifice is justified on this ground. There is no certainty that social concepts are closer to an individual's experience than are concepts of experiential data, nor are complex social values necessarily easier to comprehend than simpler descriptive ones. It is interesting, therefore, that the temper of the times with respect to psychology should deviate so markedly from the accepted procedure of the other natural sciences. One wonders what would be the reaction to beginning the study of elementary physics with electronics, or of elementary chemistry with the structure of colloids.

A danger that the reviewer sees in introducing the study of psychology with a discussion of "Social functions of the Individual" is that the language one must use is replete with words that have significances different for the student from those intended by the writer. The author of Chapter II is fully aware of the danger but it is difficult to see how it can be avoided without labored definition. The alternative device of cross-reference (on page 23 "see page 275 f." or on page 33, "see page 498") is annoying, inconvenient, and probably useless, if a response is expected from the student. The section on "Reification" might well have been studied and applied more carefully by the authors themselves. They are guilty of reification on many occasions, and there is little in the book's attitude of social, personal evaluation that will force students to distinguish between their reified opinions and facts.

In the hands of those instructors for whom it was written, this *Introduction to Psychology* will find ready acceptance. There is no question that the writers of the several chapters on the social and applied aspects of psychology, which set the tenor of the book, have given adequate presentations of their material. Students will tend to be intrigued by the apparent utility of the evaluations. Whether they will understand themselves or learn how to live better is not so certain. On the other hand, if some of he "social psychologists" can be induced to follow its rational treatment of social behavior much may be accomplished.

Conservatives, reactionaries, and some who have fought for psychology as a natural science will be disappointed with this product of so many of the influential laboratories of psychology. Both the systematic implications and the pedagogical results are less than might have been hoped.

Hobart College FORREST LEE DIMMICK

Collective Behavior. By RICHARD T. LAPIERE. New York and London, McGraw-Hill Book Co., 1938. Pp. xiii, 577.

This book is designed as a "second" course in social psychology. Emphasis is placed on "social interactions themselves" rather than upon "the processes by which individuals develop their personalities"—a distinction which left me as cold and mystified at the conclusion of the book as it found me at the commencement. Why is this a "second" and not a first course? There is nothing more difficult about this text or its matter, nor more sequential to or dependent upon prerequisites, than happens to be the case with any treatise on social psychology, which is very little indeed—so little, in fact, that it would be sheer humbug to pretend that it contained anything that a freshman of *IQ* 100 couldn't assimilate immediately, with no more than ability to read elementary English.

The reviewer fails, however, to 'resonate' to this book for reasons of a more disturbing kind, stemming from the gratuitous distinction above, which draws so much water for LaPiere. The time seems long past when it was considered profitable to regard personality in a vacuum and social interactions by "themselves," whatever that may be. Personalities interact and thus develop, so that the resulting personalities and modes of interaction, never separable, reciprocally modify one another. What else is there to personality, once the individual has started with the slight hint from his genes? With the best will in the world this distinction between "social interactions themselves" on the one hand and determinants of personality on the other must be regarded as a manifestation of the sociologist's chronic reification of "social processes," as something above and beyond the individual, as an independent entity like the cat's grin in *Alice in Wonderland*. It may be forgivable as a convenient schematization, provided the student be cautioned once to a page that it is no more, but in LaPiere no evidence is given of the recognition of such discrimination as sheer artifice that does violence, perhaps a necessary violence, to the facts. This naïveté is fully the equal of that of the physical scientist who sees in water no more than oxygen and hydrogen. If this book stands for anything systematic, it stands for the rehabilitation of the group-mind notion, despite LaPiere's pious disavowal, but now in the trappings of the more sophisticated and verbally less occult guise of "social interactions." Aware of his vulnerability in this respect, LaPiere does some shadow boxing in which he gives himself the decision. He accuses those who regard the individual as the sole and ultimate repository of social reality of throwing the baby out with the bathwater, of destroying "the body since they do not like the soul imputed to it." He accuses them of denying the very existence of collective behavior or social interaction. Such denial is news to the reviewer. What is denied is that it can remain as an entity, like the grin of Alice's cat after the cat was out of sight. Behavior is always individual and can be nothing else. In isolation it is one thing. In a social context it is another and different thing, because the determinants are different, but it is still individual. That there should be the slightest opportunity or occasion for such a discussion as mine above suggests that the group-mind controversy, far from being a "thing of the past in American social psychology," as LaPiere avers, has preserved a perennial vitality, among sociologists at any rate.

LaPiere commends his book for its logical cogency and unity, but it is an externally imposed unity that makes for neat dialectics but doubtful psychology. This unity consists of the rigorous application of a formula of treatment to each type of social interaction. For example, the type "Conventional Behavior" is analyzed into the nuclear topics of *Origin and Function, Ideologies, Membership, Overt and Covert Aspects of Interaction,* and *Leadership*. Precisely the same formula of analysis is applied to "Regimental Behavior," "Formal Behavior," "Congenial Behavior," etc., with relentless insistence.

Beyond establishing a rigorous formula, however, this logical cogency sometimes breaks down sadly. In discussing conversional audiences (p. 258), LaPiere, influenced apparently by the heavy weight which economists place upon "marginal utility," feels that he has here a key concept of the utmost importance, for it is, according to him, the "marginal listeners" of an evangelist who determine the fate of his movement. The analogy to "marginal utility" is so close as to be painful to the critical reader. At any rate, LaPiere holds that the primary concern of the leader is not with his followers, who are already loyal, or with his antagonists, who are in opposition, but with the "marginal listeners," who are indifferent and can turn the issue in his favor if they are converted. Unless LaPiere has a very original notion as to the number of such "marginal listeners" in any really important movement, the idea sounds as if the Christian conversion of the Roman Empire had never occurred, a Hitler had never appeared, and a Roosevelt had never been elected. Was the Christian conversion, the rise of Hitler, or the election of Roosevelt—or for that matter any considerable social transformation—the result of the slight addition of weight from the side of the indifferent, or of the conversion of a far greater number of the active opposition? For want of actual figures, we prefer to venture the guess that disgruntled pagans, Social Democrats, and Republicans had a determining effect on the results. Inconsistently enough, LaPiere too seems to agree with this view (p. 258), only he calls such converts from the opposition also "marginal." The question is, then, when is a listener "marginal"? When he's initially indifferent? Yes, says LaPiere—and then proceeds to include the originally hostile. Obviously, conversion is meaningless if it can affect only the initially indifferent. For effectiveness in social transformation it must succeed also in drawing in the hostile, who can be won if the leader has the right code of appeal, just as his adherents can be lost if he has the wrong one for them at the present moment. Thus the matter involves a complex total pattern in which initial adherence, hostility, or indifference are merely components among others.

University of Tampa ELLIS FREEMAN

Moses and Monotheism. By SIGMUND FREUD. Translated by Katherine Jones. New York, Alfred A. Knopf, 1939. Pp. 218.

The Hebrew Moses: An Answer to Sigmund Freud. By TRUDE WEISS ROSMARIN. New York, Jewish Book Club, 1939. Pp. 64.

Every new book of Freud's not only repeats but also modifies, and sometimes clarifies, his fundamental theories. His last book, properly a supplement to his *Totem and Tabu* of 1912, is no exception. Now that Freud is dead, a special interest attaches to his last word.

It is nonsensical, says Freud, to maintain that psychoanalysis is practiced if the early periods of life are excluded from one's investigation; yet this claim has been made in many quarters (p. 114). Traumata belong to early childhood, the period up to about five years. Impressions during the time when the child begins to speak are especially interesting. The period between two and four years is the most important (p. 116). The strongest obsessive influence derives from those experiences which the child undergoes at a time when his psychical apparatus is incompletely fitted for accepting them (p. 199).

Childhood traumata concern impressions of a sexual and aggressive nature and also early injuries to the self (injuries to narcissism). It is very striking that the sexual factor should predominate. Analytic work alone can yield a knowledge of the forgotten experience, or—to put it more concretely, though more incorrectly—is able to bring those forgotten experiences back to memory (p. 116 f.).

Human sexual life shows an early blossoming which comes to an end at about the age of five years. Then follows a period of latency. This suggests that man is derived from a species of animal that was sexually mature at five years, and arouses the suspicion that the postponement, and the beginning twice over, of sexual life has much to do with the transition to humanity. Man seems to be the only animal with a latency period and delayed sexuality. Investigations of primates would furnish a test for this theory. Perhaps this state of affairs is a necessary condition for the existence of neurosis, which seems to be a human privilege (p. 117 f.).

The effects of the trauma are twofold, positive and negative. The former are endeavors to repeat the traumatic experience. If it was an early affective relationship it is revived in an analogous connection with another person. These endeavors are summed up in the terms 'fixation to the trauma' and 'repetition-compulsion.' The effects can be incorporated into the normal ego and lend to it immutable character traits. The negative reactions are defensive, culminating in inhibitions or phobias. These negative reactions also contribute to the formation of character. Actually they represent fixations on the trauma no less than do the positive reactions. The symptoms of neurosis constitute a compromise. The opposite reactions create conflicts (p. 118 f.). Neurosis may be regarded as an attempt at cure (p. 122).

A partial return of the repressed takes place when these conditions are present: an illness which acts on the ego, sleep, puberty, a new trauma similar to the old (p. 150), new problems of life (p. 121).

The topography of the psyche (id, ego, super-ego) has in general nothing to do with cerebral anatomy; there is only one point where it impinges on it (p. 153).

The theme of *Moses and Monotheism* is that religious phenomena are to be understood only on the model of the neurotic symptoms of the individual, as a return of long-forgotten important happenings in the primeval history of the human family (p. 88). The analogy with psychopathology is very complete, approximating to identity, in the nature of an axiom (p. 113 f.). In spite of the attitude of biological science, which rejects the idea of acquired qualities being transmitted to descendants, Freud cannot picture biological development proceeding without taking this factor into account (p. 158). "Men have always known . . . that once upon a time they had a primeval father and killed him" (p. 159). From this ancestral trauma of father-murder religion stems.

On this basis Freud reconstructs human history in six phases: a traumatic period of father-murder, totemism and mother-right, humanization of God and father-right, Judaism, Christianity, and Islam. The last three monotheistic phases are represented as stages in the 'return of the repressed.' In the case of the Jews, the return of the repressed was precipitated by a new trauma similar to the old, the killing of Moses.

With this Freud enters the difficult field of Biblical criticism, a field in which even experts more often go wrong than right—and Freud is no Biblical expert. The one time he uses a Hebrew word he confuses it with a German word (p. 180). When arguing that Moses was an Egyptian, Freud neglects to mention that Moses is called an Egyptian in the Bible (Exodus, 2, 19), although the context, it is true, robs the passage of probative force. In assuming the murder of Moses, Freud accepts the opinion of a German scholar, an opinion based upon the emendation of an obscure verse in Hosea. No other scholar has accepted the emendation. *The Hebrew Moses, an Answer to Sigmund Freud,* is an expert's answer to Freud's Biblical venture.

It certainly should not be beyond the ingenuity of psychoanalysis to explain the indubitable 'paternal' elements in religion without assuming ancestral traumas and the historical killing of fathers and father-substitutes.

Freud's concept of repetition-compulsion merits the consideration of general psychology. Repetitive urges are a prominent feature of childhood. In connection with the controversy over the rôle of repetition in learning, it is pertinent that repetition, or self-imitation, is the child's natural mode of practice. A repetitive urge is also found in the tendency to resume an uncompleted task. The efficacy for recall of an 'intent to learn' is to be related to the fact that such a future intention gives the learning task an 'unfinished' character and thus, as has been proved by experiment, enhances its reproductive value.

Cornell University S. FELDMAN

Studies of Abnormal Behavior in the Rat. By NORMAN R. F. MAIER. New York, Harper & Bros., 1939. Pp. 81, 16 plates.

This monograph is an account of Maier's investigation of experimentally-produced neurotic attacks in the rat which won the thousand dollar prize awarded by the American Association for the Advancement of Science at the Richmond meetings. What is a neurotic attack? It consists of an active phase, which is characterized by hyperactivity, convulsions, hopping movements and tics, and a passive or coma-like phase during which the animal manifests various degrees of waxy flexibility. The similarity of this pattern to an epileptic attack is noted by Maier.

Maier employed the following procedure for inducing neurotic behavior. A rat was trained on the Lashley jumping apparatus to discriminate between a 'correct' design, *i.e.* one which designated the opening to food, and an 'incorrect' design, *i.e.* one which if jumped to did not give way and consequently dumped the animal into a net below the opening. After the discrimination was established, changes in the original pair of stimulus-cards were made so that the rat no longer had available cues upon which to base a choice. The rat, thereupon, resisted jumping. By applying either an air-blast or an electric shock to the animal, the jumping reaction was forced.

At this point, either before or after the animal had jumped, neurotic attacks made their appearance, but only to the air-blast and never to the shock. Maier explains this difference on the ground that the shock is something which can be avoided, whereas the air-blast cannot be. A test of this hypothesis could easily be made by having the air-blast turned off as soon as the animal jumped, just as the electric shock as a stimulus ceases upon the rat's leaving the platform.

Recent work by the Morgans at Rochester and in the Western Reserve Laboratory suggests that the labor involved in training rats to make discriminations (and anyone who has used the jumping technique will appreciate the use of that word 'labor') in order to obtain neurotic attacks is unnecessary. Attacks of a very severe kind can be obtained by placing the rat in an enclosure and releasing compressed air through a nozzle for about two minutes. Maier, himself, describes one attack to the air-blast when the rat was not in the experimental situation. Maier doubts the efficacy of hissing air alone, since in an experiment with six rats, in only 3 out of 63 exposures to air alone did attacks appear whereas there were 27 attacks out of 67 tests using the air-blast in the discrimination situation. The details by which the adequacy of this experiment might be judged are not available in the present monograph. Even though a conflict situation may evoke more attacks, the observations of Maier and others indicate that conflict is not a necessary condition for evoking the neurotic pattern. A hissing noise alone is an adequate stimulus.

Maier distinguishes between nervous and neurotic behavior. A rat may show marked fear reactions and yet not 'break down.' The reviewer suggests the hypothesis that nervous behavior may actually protect the animal against attacks. Studies in progress at Western Reserve indicate that it is the phlegmatic rather than the emotional rat which is more likely to manifest an attack.

The reader will find no final answers in Maier's monography. He will, however, find himself asking many significant questions and hurrying to the laboratory to find the answers (we hope!). The reviewer contended several years ago that the comparative psychologist might be of real service to the abnormal psychologist. This monograph is an important demonstration of the truth of that contention.

A word as to the organization of the book. After a short introduction and general description of the method, there follow rather detailed case histories of ten neurotic rats. These histories take up more than half of the book. The last 30 pages are devoted to a discussion of the results. There are 16 splendid full page photographs of different aspects of the rat's behavior.

Western Reserve University CALVIN S. HALL

Phantasie und Kunst. Edited by FELIX KRUEGER and ALBERT WELLEK. Neue Psychologische Studien, Volume 14, No. 1, Munich, C. H. Beck, 1939. Pp. 131.

This number of Felix Krueger's *Neue Psychologische Studien* contains two articles: the first, *Gefühl und Kunst* (Feeling and Art) by Albert Wellek, is theoretical; the second, *Beobachtungen an Zweiklangen in getrenntohriger und beidohriger Darbeitung* (Observations on intervals presented to the ears separately and to both ears simultaneously) by Hans Sandig is experimental.

Sandig's investigation was designed to test Krueger's theory that consonance and dissonance are complex qualities, immediately perceptible without analysis, and that difference tones are important for the formation of these complex qualities. Stumpf had sought to disprove Krueger's theory by the assertion that when the two tones are presented to different ears, no difference tones can occur, and yet consonance and dissonance are perceived as usual. Krueger, on the other hand, found that when the tones are presented to different ears, the character of consonance and dissonance shifts toward neutrality. Three sources of error in Stumpf's experiments were (1) air conduction, (2) bone conduction, and (3) the fact that the observers knew what intervals they were to hear.

In Sandig's experiments, the first two sources of error were entirely ruled out and the second reduced to a minimum. Sandig's experiments substantiated Krueger's theory. When presented to both ears simultaneously, the intervals had their normal musical effects. They were immediately perceived as consonances or dissonances, and the intervals were recognized without effort. When presented to the ears separately, the two tones seemed like unrelated noises without musical meaning.

Sandig showed that neither beats nor combination tones originate centrally. A physiological condition for their occurrence is the peripheral union of the primary tones. The latter, he found, is also a necessary condition for a definite, normal experience of consonance or dissonance.

Albert Wellek's article, *Gefühl und Kunst,* is an interesting attempt to resolve the conflict between two schools of esthetics, the formal and the expressive. He contends that the error of formal esthetics consists in setting up an opposition between form and feeling. Form, he points out, is no enemy of feeling, but its necessary vehicle. He contends that the essential mark of esthetic forms is *Gestalttiefe* (form depth), by which he means the significance of the forms as vehicles of feeling.

Whereas some esthetic theorists have thought that esthetic feelings were not real feelings but merely "sham feelings," Wellek insists that they are genuine, and that they do not differ from the feelings of everyday life in quality, warmth, or intensity. They do differ in vital Ich-Nähe (ego nearness) and in their dynamics in the direction of overt behavior. The peculiarity of esthetic life is that esthetic feelings are shut off from practical activity. Just as a work of art is "framed" within the real world, so is the esthetic feeling framed. Children and naïve adults lack this frame of reference. That is the reason that art sometimes moves them to overt activity (such as shouting a warning to "Snow White" not to bite the apple).

Having established the criterion that esthetic feelings are shut off from overt activity, Wellek is logically forced to conclude that the feelings which drive the creative artist are *not* esthetic feelings.

In artistic creation, the feelings vary with the different stages of the work and with the personality of the artist. Often the artist feels as though he were being driven by a supra-individual stranger, who may be loved or hated or both at the same time. In certain stages of artistic creation, the feelings of the artist are eminently practical.

The highly personal spasms of amateurs have little or nothing in common with the work of real artistic creation, for the emotional significance of a work of art is strictly dependent upon its form, according to Wellek.

West Springfield, Mass. M. F. MARTIN

Spiritualità della Natura. By ALBERTO V. GEREMICCA. Bari, Laterza & Figli, 1939. Pp. 114.

Geremicca defends the thesis that the mechanistic interpretation in science may deny but can not destroy the possibility of spiritualism in nature. Mechanistic interpretation has become "official" in science. This orthodox viewpoint employs empty phrases and subtle sophistry in defending its position that man is a machine. Analysis is emphasized, synthesis is disappointingly neglected. When the arguments of the mechanists are reduced to fundamental postulates, mysticism results. Although it may be insisted that man is a machine, it must be remembered that every machine presupposes a machinist.

A case for spiritualism is presented by Geremicca. The existence of spiritualism might be denied at the present time, he states, but it can not be proved that spiritualism is excluded as a possibility in evolution. The theory of mutations teaches that equal forms may evolve into different things. There are, therefore, endless possibilities of variation. For example, "reading ability followed the development of perception in man . . . an outcome impossible of prediction." May not spiritualism result from an analogous variation? It is obvious, therefore, says Geremicca, that phylogenetic studies in science are indulging in phantasy when attempts are made to set limits to development.

In all of the four chapters (*Instinct, Heredity, Intelligence, Evolutionary Development*) Geremicca proposes and defends his argument that spiritualism can not be disproved. He writes with enthusiasm, clarity, philosophic orientation, and logical rigorousness. His words are nicely selected resulting in a style literary in quality. The book is scholarly rather than scientific in emphasis. The few references are drawn largely from biology. The reader may disagree with the thesis but he is nevertheless compelled to question his own habits of thought.

Psychological Test Bureau, Rochester, N.Y. ANTHONY J. MITRANO

Outline of Psychiatric Case-Study. By PAUL W. PREU. With a foreword by Eugen Kahn. New York, Paul B. Hoeber, Medical Book Department of Harper & Bros., 1938. Pp. 140.

This little manual is quite similar to one previously put out by Chaney several years ago. It offers little that is new in outlining psychiatric procedures. The book is composed essentially of two parts. The first part is an outline of the psychiatric history taking which is an outline in semi-tabular form of the essentials to be investigated by the psychiatrist who tries to elicit a past history of his patient. The usual subjects are gone into: family history, past personal history, and personality. The remainder of the book, except for short chapters on physical examination and other similar nonpsychiatric aspects, is composed of an outline of mental examination. The headings here are "appearance," "general behavior and attitude," "motor status," "stream of speech and mental activity," "content of thought," "emotional state and sensorium," "mental grasp and capacity." The book is to be commended because it is so complete, but it is to be condemned because of the lack of sample questions given to enable the psychiatrist to make a study of patients, who does not carry with him standard, exemplary replies to questions. In other words, the student or beginning psychiatrist picks up this book with the idea of carrying on a psychiatric examination is provided with an extensive prospectus on how the history and examination are to be made but is not provided with any means of evaluating the responses and reactions that he gets. A number of words are used in improper text such as the term "visual field" with respect to hallucinations, the term being more properly used with sensory examination of the eye. There is no glossary to explain the technical terms to beginners for whom the book is supposedly designed.

Detroit, Mich. LOWELL S. SELLING

The Psychology of Exceptional Children. By KARL C. GARRISON. New York, Ronald Press Co., 1940. Pp. iv, 351.

About one-fourth (5 chapters) of Dr. Garrison's book is devoted to a preliminary consideration of the origins of child behavior, individual development and differences, trait differences and education, and measurements in education (as well as in psychology). About one-sixth of the space (4 chapters) is given to a discussion of gifted children, slightly less than one-fourth (6 chapters) to mentally retarded children, and slightly over one-fourth (6 chapters) to children with visual, auditory, speech, and orthopedic defects, to children of lowered vitality, and to problems of left-handedness.

The student who is not familiar with the field will find here a brief summary on gifted and handicapped children, well written in the main in non-technical language, straight-forward, well organized, fairly well documented, and all-in-all sound in its main findings and conclusions. On the other hand, the experienced specialist on deviating children will find little in the book with which he is not already familiar. It contains practically no original contributions and it supplies little evidence that the author has had any firsthand clinical, educational, supervisory, or administrative experience with the types of deviates that are discussed.

The summary provided is indeed fairly satisfactory for a book of 340 pages, although isolated statements and conclusions occur that probably will not be accepted by some specialists in the areas concerned. References occur to a wealth of investigations consulted by the author. Many of the writers quoted, however, have made only a single contribution and have not been closely identified with work among handicapped children. On the other hand, no reference is made to many significant studies (some more important than those cited), and a number of authorities who have for long published extensively and who for long have given all their time to the study of deviates, some as pioneers, are not cited at all, nor are their publications listed. In some cases literal quotations are made from such books in which the copyrights have not expired without the use of quotation marks or references (*e.g.* on pages 79 and 270 f.), although the author states that "in each case an attempt has been made to give credit by a rather complete citation."

Whether so much space should in a brief book be given to the material in Part I, which is readily available elsewhere, and much of which is not needed for the subsequent discussion, and whether a discussion of bright children should be included in a book dealing mainly with handicapped children are questions on which authorities may differ. The problems of the gifted and the handicapped are different problems. Usually those who work with the handicapped do not work with the gifted; similarly vice versa. It is doubtless true that the school administrator should know something authoratative about both the gifted and the handicapped, but it does not necessarily follow that the two should be presented in the same book, particularly when limitations of space preclude an adequate treatment of either group. With space saved in accordance with the above suggestions the treatment of the handicapped could have been profitably extended—*e.g.* to the inclusion of epileptic and encephalitic cases, the treatment of cardiopaths, Braille work for the blind, sight-saving instruction for the partially sighted, and the expansion of many topics whose treatment is all too brief.

It is not clear why the title of the book is limited to the "psychology" of exceptional children since it contains much that is not psychological. To the reviewer its place seems to be limited to the general student in psychology or education who merely wants a brief overview of the field without any thought of specializing in its clinical, social, or educational phases. As a text its value will be enhanced if the course is offered by a specialist who can supplement the book with extensive first hand experience. Most of the questions at the chapter ends are excellent. The author has shown considerable versatility and skill in the construction of varied and provocative exercises.

Delaware Division of Mental Hygiene J. E. W. WALLIN

Biographies of Child Development: The Mental Growth Careers of Eighty-Four Infants and Children. By ARNOLD GESELL, BURTON M. CASTNER, HELEN THOMPSON, and CATHERINE S. AMATRUDA. New York, Paul B. Hoeber (Medical Book Department, Harper & Bros.), 1939. Pp. xvii, 328.

This latest book from the Yale Clinic of Child Development consists almost entirely of case histories. Part I written by Dr. Gesell, deals with a group of 30 cases which were first reported 10 years ago (A. Gesell, *Infancy and Human Growth,* 1928). These same children have been examined further to determine to what extent the predictions formulated when they were infants are consistent with the diagnoses 10 years later. The bulk of the report consists of the detailed findings for each of the children. In the majority of these selected cases it was found that the predictions were fairly accurate.

The second part of the book, written by the various members of the staff, is concerned with special problems. Here also the predictions made during infancy are compared with later diagnoses. These special problems include the superior child (Thompson), language and reading problems (Castner), irregular mental development (Castner), twins (Gesell), foster care and adoption (Amatruda), and children with physical complications (Amatruda).

The book includes descriptions of children in which the early predictions were later verified, showing what can be done in the way of predicting, and it also includes descriptions of children whose later development was inconsistent with the early predictions. In these latter cases the authors attempt to analyze the reasons for the incorrect prediction or change in the rate of development. In general in the cases cited the superiority or inferiority of a given child was usually predicted accurately if the home conditions were good, but prophesies were unreliable in those cases in which the children were brought up under adverse conditions. Accurate predictions were often made far in advance. For example, superiority or difficulty in language or reading was often prophesied before the child was old enough to talk or read.

There is no indication anywhere in the book of the total number of cases which were predicted correctly or incorrectly, and the cases described were selected, as Gesell mentions in his introduction, not because they were typical but because they taught the members of the staff the most. Therefore these cases do not give a typical picture of development, and the study is of no value except as a comparison of the individual cases described.

The book is of interest to those who have followed the earlier work at Yale and should be considered merely as a supplement to the earlier volumes, not as an independent work or general treatise on child development.

Cornell University — MARY S. RYAN

Pintner General Ability Tests: Verbal Series. By RUDOLF PINTNER, BESS V. CUNNINGHAM, and WALTER N. DUROST. New York, World Book Co., 1939.

This series consists of primary, elementary, intermediate, and advanced group tests, which are all available in two equivalent forms. The Pintner-Cunningham Primary Test is for kindergarten, grade 1, and the first half of grade 2, and is published in forms of seven subtests each, composed entirely of pictures which are marked by the subjects according to the examiner's verbal directions. Form A of the Primary Test is like the original Pintner-Cunningham Primary Mental Test except that the pictures have been redrawn and new items have been substituted for dated ones. The Pintner-Durost Elementary Tests for which norms are now being developed is designed for the last half of grade 2, grades 3 and 4. The Intermediate Test is for grades 5 to 8 inclusive and each form consists of 8 subtests: vocabulary, logical selection, number sequence, best answer, classification, opposites, analogies, and arithmetic reasoning. All answers are of the five alternative, multiple-choice type. The Advanced Test is similar to and an extension of the Intermediate. Working time for the various tests ranges from 30 to 55 min.

A well prepared manual of 16 pages for the Intermediate and Advanced Tests gives a description of the series, a brief but unusually adequate statement of the requirements of a good test, characteristics of statistical data and standardization of the series, and suggestions for interpretation and use of the tests. A nomograph for facilitating conversion of standard scores to *IQ*s, a procedure which is justified by the authors on the basis of refinements in statistical analysis and completeness of their norms, is appended to the manual. Directions for administering and scoring which have been carefully edited are given in a separate manual.

Sampling procedures as well as number of cases used in establishing the norms (the total is above 100,000 *Ss*) would seem to be satisfactory. Every child in the public schools in the appropriate grades of the coöperating communities took the test, and the authors believe they have eliminated sampling errors which might arise from intracommunity selection. Communities were chosen which were widely different in size, socio-economic status, and geographic location.

The eight subtests of the Intermediate and Advanced batteries measure several psychological characteristics which are statistically somewhat independent. When the verbal factor is kept constant the median partial intercorrelation of the subtests is 0.30 for the Intermediate and 0.26 for the Advanced.

Statistical reliability of total scores on the Intermediate Test, Form A correlated with Form B, is 0.94 based on 101 cases (odd-even reliability is 0.94 with N = 101). The interform reliability of the Advanced Test, based on 92 cases, is 0.84 (odd-even reliability 0.90 with N = 587). Reliabilities for the total tests are high enough for group comparison but test performance must of course be interpreted with great caution when the purpose is individual diagnosis or orientation. Probable errors of measurement, expressed in terms of standard scores, are 2.6 points for the Intermediate and 3.9 points for the Advanced.

Reported statistical validity of the tests is very high. The Intermediate Test correlates 0.84 with total scores on the Metropolitan Achievement Tests for 168 cases in grade 5, and 0.79 for 209 cases in grade 7. Coefficients between the Pintner General Ability Tests and seven other psychological tests range from 0.71 for the Revised Stanford Binet to 0.87 for the Otis Group Intelligence Scale.

The reviewer was favorably impressed, not only by the care used in the development of the tests, their elaborate standardization, and their detailed evaluation *before* publication, but also by the clear and conservative presentation of interpretative material in the manual. Repetition of such statements as the following quotation from the manual will do much to aid test users in general to appreciate limitations of adequate instruments of measurement—"success or failure must be interpreted in terms of a variety of additional facts concerning the child."

University of Maryland ROGER M. BELLOWS

The Effects of Varied Amounts of Phonetic Training on Primary Reading. By DONALD C. AGNEW. Durham, Duke University Press, 1939. Pp. viii, 50.

In an introductory chapter Dr. Agnew points out the *a priori* nature of statements concerning the value of phonetic training, discusses the disagreement between teachers and writers of textbooks, and stresses the need for additional experimental investigations of the problem. The following six chapters are devoted to an exposition of two investigations made in the primary schools of Raleigh and Durham.

The Raleigh study was made to determine the relative value of phonetic and nonphonetic training as a basis for teaching reading abilities. Groups of third grade pupils were equated for *MA, IQ,* and rate of progress in school, but differed in amounts of phonetic training as determined by teachers' rating scales. Group tests of silent reading abilities and vocabulary, and individual tests of oral reading, word pronunciation, eye-voice span, and phonetic abilities were administered. The differences in test performance between the groups were small and not consistent.

It was thought that the small differences in amounts of phonetic training between the groups, or the interference of one type of training with another due to the inconsistency of phonetic training in the school grades, might have had some influence on the results. Phonetic training in the Durham schools had been greater in amount and more consistent than in Raleigh, so the second study was undertaken there as a check on the first. When groups of Durham and Raleigh pupils were compared, performance of the former was significantly higher on all tests except those of speed and accuracy in silent reading, indicating that intensive phonetic training does improve certain reading abilities. In a final chapter the results are summarized and certain implications are discussed. A bibliography of 47 titles is appended.

One is impressed by the caution with which Dr. Agnew generalizes and by his control of many variables. In a field which is replete with generalizations of unknown dependability and with *a priori* conclusions he has not attempted a final answer to the questions involved, but has stressed the necessity of obtaining adequate criteria for the teaching of primary reading before advising extensive use of phonetic training.

University of Maryland JOHN W. MACMILLAN

We Call It Human Nature. By PAUL GRABBE. In coöperation with Gardner Murphy. New York, Harper & Bros., 1939. Pp. 120.

This attractive little book represents an attempt at an accurate but popular account of the highlights of scientific psychology by an author who is not himself a psychologist. He has here utilized methods of presentation which had been developed previously for the teaching of languages. His factual material was obtained under the guidance of Gardner Murphy, who has apparently given freely of his time and assistance.

Grabbe's approach to his subject consists of a very terse text, liberally illustrated with diagrams, cartoons, and photographs. Most impressive are the photographs, which are excellent, both pictorially and in subject matter. The diagrams are designed to provide schemas of various psychological concepts and functions. These drawings make use of certain special symbols which represent "organism," "stimulus," "drive," "response," and the like. For full understanding, rather careful reading with adequate reference to the glossary is necessary.

If shortcomings must be pointed out, we may begin with the condensation of the text. The process of boiling down has progressed to the point of virtual stylelessness, which makes for rather choppy reading. Another result of this over-condensation is a relative lack of clarity. One wonders what the naïve reader, to whom this volume is addressed, will do when confronted with the technical terms employed by Grabbe. Perhaps twice as many words would have resulted in a book more readable. Further, some will note the lack of any sensory material whatsoever. Could not colors, sounds, and even flavors be both interesting and important to the reader? Should *all* learning be treated as though it were conditioning? Why, after a purely naturalistic development of the subject of psychology, does the author so uncritically present psychoanalytical constructs as proven truths?

Even if the above objections are valid, one can, with few misgivings, recommend this work to someone who wants to read a book, not too long, which will tell him something of what psychology is about.

University of Alabama F. NOWELL JONES

Human Nature Writ Large. By F. CREEDY. Chapel Hill, University of North Carolina Press, 1940. Pp. 484.

This is an attempt to look at Occidental civilization with a detached objectivity, comparable to the anthropologist's examination of a native culture far away or long ago. A central thesis is that there are in human nature certain primal urges—self-preservation, reproduction, coöperation, creative intellectual activity—necessary to survival, whose realization in a non-conflicting way must, therefore, be the purpose of a human society. The expressions of these urges get conventionalized and fixed in the mythologies of theistic religion, of nationalism, of a fictitious free economic competition, of sexual taboos, of romantic love; and these become effective in establishing "crowd habits" through the exaltation of certain personal heros to dramatize the myths, the crystallization of the myths in art and literature, and the loosing of an abundance of catchwords or "trigger phrases" to touch off the unthinking popular prejudices. The general reader will find the book refreshing in its very independence of those conventionalities and its critical and constructive suggestions.

The psychologist will not be impressed with the references to his working concepts and tools. The 'basic human urges' are apparently traditional ones, and show no hint of original observations nor of contact with more recent laboratory studies. The treatment in antithesis of 'habit' and 'attitude' is not in line with the recognized variability of habit, nor the truly habit-character of attitude. Definitions offered or projection,' 'transference,' and the like, leave much to be desired; nor is it true to speak of them as having been defined by 'introspections.' The frequent references to 'psychic conflict' are not enlightening. The method of 'behavioristic psychoanalysis by means of documents' is such only in a figurative way: it is essentially a detailed sentence-by-sentence syllogistic analysis of a text to make explicit its implicit assumptions. All in all, the psychological foundations of the work are uncertain and shaky. It seems that the author has not writ human nature in the large, but has attempted to come the other way—from his observations of social phenomena to check their foundations in the human nature of the individual.

Apparently much more command is shown of the economic field; and the book remains stimulating in its many suggestions there and in other matters of public concern.

University of North Carolina JOHN F. DASHIELL

Tests and Measurements in the Improvement of Learning. By ERNEST W. TIEGS. Cambridge, Mass., Houghton Mifflin Co., 1939. Pp. xxi, 490.

Designed as a text for classes in tests and measurement, this book is an extension of the author's earlier *Tests and Measurements for Teachers* (1931). The emphasis on "approaches based more closely on the activities and problems of the educational program" attempts "to meet the measurement problems and needs of teachers on different levels as well as to provide data for the solution of supervisory and administrative problems." The first four chapters give a straightforward and interesting discussion of measurement, use, and meaning of the IQ; and construction of the various types of "informal" diagnostic devices. 'Informal' seems in this usage to be synonymous with 'standardized' and one might take serious issue with the author for urging such judgment-making devices on the teacher if he had not stressed the frequent use of such casual objective tests primarily as means of diagnosing student difficulties interfering with mastery of clearly-stated objectives. The danger of having teachers come to rely on such *diagnostic* devices as *appraisal* devices must be pointed out.

The next six chapters deal with diagnosis and appraisal in primary learning, elementary grades and secondary schools and colleges. This treatment, according to levels of education, leads to repetitious and cursory summaries of areas of diagnosis, even though it provides a convenient reference for teachers interested in a specific level. For example, diagnosis of reading is discussed in three different places, rather than in one coherent and unified chapter. The sections on personality and interest diagnosis are the least critically discussed throughout these chapters, with inadequate mention of the specialists who can and do contribute aid to the teachers in these areas. Furthermore, the tests used for illustration are not discussed critically in terms of their own reliability or validity.

Chapters on ability-grouping, curriculum-building, teaching effectiveness, marking and promotion, and planning and interpreting the measurement program pick up the interesting and informal style of the opening chapters. Essential statistics for the teacher are included in two chapters of extremely rapid development by listing and illustrating formulae and their use.

As minor criticisms, the index is not well organized; the selected bibliographies carry occasional errors in authors' names; throughout the nineteen chapters ideas and concepts are too frequently repeated. On the whole, however, teachers in training and in service should find much of the book stimulating and helpful.

University of Minnesota JOHN G. DARLEY

The Health of College Students. By HAROLD S. DIEHL and CHARLES E. SHEPARD. Washington, D.C., American Council on Education, 1939. Pp. 169.

Since 1907 the student health services in our colleges and universities have been frequently surveyed and the results published. Nine such surveys have revealed such a wide variation in these services that the development of a practical set of standards has not been forthcoming. Two National Conferences on College Hygiene (one in 1931, the other in 1937) attempted to set up minimal standards without any special consideration of the size and type of the institution. This report by Diehl and Shepard of their study of 549 colleges and universities attempts not only to summarize the main facts regarding student health service and college health teaching, but it also makes definite suggestions as to the organization and development of these activities: (1) in all colleges; (2) in public institutions—except teachers' colleges—large private coeducational institutions, women's colleges, and large colleges for men; (3) in teachers' colleges and colleges with enrollment under 1,000; and (4) in colleges with day students almost exclusively.

Though based upon data derived from a questionary and subject, therefore, to the definite limitations of that type of research, this report gives us in its 169 pages a surprisingly clear picture of the efforts our colleges and universities are making to protect and improve student health. We may question the recommendation that a Wassermann test be required as a routine part of every college health examination. We may wonder why these authors recommend one college health physician to every 800 students, while the National Conference on College Hygiene recommended one to every 500 students. We may wonder if the accuracy of the data justified the rather involved statistical treatment given them. The authors have, however, succeeded in presenting very plainly what present student health service practice is at the present, and what it ought to be in the future.

Cornell University D. F. SMILEY

A Study of Jealousy as Differentiated from Envy. By T. M. ANKLES. Boston, Bruce Humphries, 1939. Pp. 109.

The present volume was presented as a part of the examination for an Academic Postgraduate Diploma in Psychology at the University of London. It would appear that the A.P.D.P., which the author writes after his name, is roughly equivalent to the M.A. or M.S. degree in America. The publication under consideration is approximately of the calibre of the average Master's thesis, yet the book has previously been published in England, and the jacket cover advertises this printing as the *first* American edition.

Mr. Ankles' research consisted in interviews of 30 subjects, selected at random from "not-too-intimate acquaintances" and from among persons met across the tea-table in public restaurants. Mr. Ankles presents separately the result of each interview.

Mr. Ankles has humanitarian as well as scientific interests, as is shown by the final paragraph of Case Study No. I:

"Before dismissing Mr. A., it is well to say that part of the bargain for his interview was that I make plain to him the reason for his homosexual attachment. Mr. A., it was found, loved his mother intensely as a youth, but was unrequited, so fixated on his father, and all women to him (unconsciously) are 'his mother' multiplied. This cause being made clear, Mr. A. promised to try to see things in a different light."

There is no table of contents, but this is scarcely surprising, as the book is not divided into chapters. There is no bibliography, but in the text there are occasional references to Adler, Freud, Briffault, Darwin, and Watson.

University of Virginia WAYNE DENNIS

This Business of Living. By L. W. GRENSTED. New York, Macmillan Co., 1939. Pp. 185.

This book discusses science, art, and religion from a common sense standpoint in order to indicate what each may contribute to the solution of everyday problems of living. Dr. Grensted is Nolloth Professor of the Christian Religion at the University of Oxford. Aside from the fact that it is refreshing to find a British divine keeping more or less up to date in psychology, the book has but little interest for the professional psychologist. His discussion of science is commonplace and limited in its point of view, that of art is based upon a single theory, the intuition of values, which, like all the other esthetic theories, is of much disputed validity. His discussion of religion is by far the best part of the book, and seems to a layman to be of considerable practical value. He characterizes religion as that which "constitutes for each one of us the guiding principle by which the business of living is directed." This may be pleasure or the satisfaction of appetites, money making, power, the welfare of men, truth, beauty, goodness, holiness, etc.; and for each of these there is a god, which, in the business of living, is the actual object of worship of various groups of men.

Hobart College FOSTER P. BOSWELL

Textbook of Abnormal Psychology. By R. M. DORCUS and G. W. SHAFFER. 2nd ed. Baltimore, Williams & Wilkins Co., 1939. Pp. xvii, 475.

The first edition of this volume won many commendations, including the present reviewer's, and Knight Dunlap is probably right in crediting the present revision with maintaining its first place among texts on abnormal psychology. It is comprehensive, and bears witness to painstaking search of literature, as well as personal contacts with the field. There are two notable drawbacks, both looking like the result of hurried preparation. The presentation is not sufficiently articulated to spare an instructor a considerable burden of further organizing it for the learner. Again, some might refuse to call it notable but it is real, the literary quality is scarcely at par; there are passages that seem lifted bodily from informal classroom discourse; *e.g.* use of the pronoun *I* (p. 103) in a work of joint authorship. Examples of terminological looseness are the definitions of neologisms (p. 93) and of aphasia (p. 107). A mental age reaching up to 12 yr. (p. 351) is not, and should never have been, an acceptable characterization of "moron;" and what can be the reason for printing, in a book dated 1939, a three page summary of the 1916 Stanford-Binet (pp. 354-357), while according to the 1937 revision three lines?

In the future editions through which this book will doubtless and deservedly pass, time and trouble should be taken to guard against remediable weaknesses such as these. Perhaps the work would be more useful if organized as a source-book, for which the authors appear amply equipped. Without in any way prejudicing the present book's position as meritorious and the best in its field, it has still some ground to cover before it invites, from detached judgment, all the laudatory foreword by Dunlap, or comes within hailing distance of Chapman's opening appraisal.

Harvard University F. L. WELLS

Le mystère animal. By DANIEL ROPS AND OTHERS. Paris, Librarie Plon, 1939. Pp. ix, 301.

This little volume is a curious mixture of prose poems, essays, and scientific surveys by students of animal life. The book is divided into two parts. The first part, entitled "Animals and Men Today," has little scientific value. It contains: "Animals I Have Known" by Colette; "The Love of the Animals" by Edmond Jaloux; "The Wild Animal and Man" by André Demaison; "Portraits of Domestic Animals" by Charles Silvestre; "What Man Owes to the Animal," by Jean Canu; and "The Inhuman World and the Love of the Animals" by Daniel Rops.

The second part, entitled "From Animal to Man," begins with an article on "The Brain of the Animal and That of Man" by Walther Riese—a valuable survey of the present theories of neuro-physiology. This is followed by Eduard Claparède's article "From Animal to Human Intelligence"—an excellent survey of the recent results in animal psychology. Less good is the essay by Paule Règnier on "Animal and Human

Pain." The most interesting chapter of the whole volume is that by Marguerite Combes and Charles Boucaud on "Animal and Human Society." At the end is a worthy sermon, "From Animal to Man: The Mystery of the Soul" by Abbè Jean Plaquevent which is written from the point of view of the Roman Catholic Church.

New York City ERNST HARMS

Conscious Orientation. By J. H. VAN DER HOOP. New York, Harcourt Brace & Co., 1939. Pp. vii, 352.

Conscious Orientation is a detailed exposition of the various types of human mental structure from a phenomenological point of view. Unfortunately the author's psychiatric experience has not served to anchor the theories to the dynamic social relations which they attempt to explain. In his systematic presentation, Dr. Van der Hoop draws most heavily on the personality typology of Jung, though he differs from him in such respects as insistence on a distinction between feeling and emotion, and between sensation and perception. It thus becomes possible for the author to extend the types of possible variations of extraversion and introversion.

This attempt at "a study of personality types in relation to neurosis and psychosis" bogs down because of the too tenuous lines which are drawn between psychology, metaphysics, and philosophy. The writing (or it may be the translation is inclined to be excessively wordy, very rambling and overconcerned with minutiae. There is throughout the tendency to regard abstractions of mental phenomena as if they were functioning entities.

The author takes a step in the right direction when he refuses to view the contributions of Jung and Freud as necessarily incompatible because they represent different schools. The Jung-Freud synthesis which he presents is not, however, especially fortunate.

Conscious Orientation will not offer much to the systematic student in his search for a workable typology. This is so primarily because of the work's essential nondynamic view concerning the mechanics of personality development, and its dependence upon vague concepts of "inherited predispositions," "instinctual life," "intuition," and "feeling types." Despite its occasional stimulating discussion of the current systems of classification, the book seems more like a curious historical relic of naïve mentalism, than a psychological treatise offered in 1939.

Brooklyn College ROSALIND GOULD

Gausteine zur Psychonalyse. By SANDOR FERENCZI. Postume works edited by Vilma Kovacs. Bern, Hans Huber, 1939. Vol. 3, pp. 543; Vol. 4, pp. 411.

Sandor Ferenczi, highly rated as Freud's most eminent disciple, died in 1933. In 1926 he had issued two volumes of studies under the title *Bausteine zur Psychoanalyse,* to which friends have now added two further volumes which contain the scientific heritage of this doubtless very able psychiatrist.

Vol. 3 contains original studies from the years 1908-1933, among which the papers "Psychoanalyse und Paedagogie" und "Entwicklungsziele der Psychoanalyse" are known as the best of his contributions to his movement. Vol. 4 reprints criticisms and referats, fragments and important notes, as well as a bibliography listing over 300 publications.

New York City ERNST HARMS

Mastering your Nerves. By PETER FLETCHER. New York, E. P. Dutton, 1939. Pp. 241.

This book is mainly a discussion of functional as opposed to organic processes in borderline mental difficulties, and is especially intended to give patients insight into their own problems. The first part of the book is analytical and the second part is a practical guide to help the reader "build up a new strategy of living." This book does not pretend to offer new approaches or theories, nor does it review advances in psychomedical science. It is a clear and practical discussion of principles familiar to psychologists, and intended for readers for whom such principles are neither clear nor familiar. The author's basic ideas are quite commonplace but sound, avoiding cumbersome and dubious assumptions, especially those of psychoanalysis. The author enlivens his pages with case histories and occasionally with shrewd insights.

Wells College C. O. WEBER

About People. By WILLIAM STEIG. New York, Random House, 1939. Pp. 105.

"Steig has something new and important to say about the unconscious as it is related to the world we know." Even if psychologists are somewhat hesitant about accepting at fact value the publisher's statement on the dust-jacket, they cannot fail to be amused and stimulated by the 105 drawings in this thin volume. There is no verbal commentary beyond the title and, indeed, none is needed. 'The Affable Man' is portrayed as a top with a rounded bottom, bobbing back to vertical no matter how struck. 'Persistent Thought' depicts a man swinging pendulumwise on a rope that binds him hand and foot. 'Anxiety' is represented by a mannikin attached to the head of an unfortunate who peers about with a worried air. It would not be an exaggeration to describe the book as presenting a series of epigrams in the form of line drawings.

University of Maryland JOHN G. JENKINS

The Nature of Creative Activity. By VIKTOR LÖWENFELD. New York, Harcourt, Brace & Co., 1939. Pp. xvii, 272.

The author conducted an elaborate series of experiments with a group of children having decided visual defects, either physical or mental or both, and compared them with a corresponding group of children with normal vision. His procedure was to ask the child to represent a specific situation graphically, and he publishes here a very extensive series of drawings from different ages and different types of artistic activity.

His main contention is that the artistic experience does not necessarily have to be an experience of the sense of sight but can be haptic in origin. He accordingly describes two types: the visual and the haptic, the latter being strikingly illustrated in the drawings of those who have defective vision but which may also be shown by those who have normal vision.

He divides art forms into two large groups; the impressionistic and the expressionistic. "The impressionistic world is the world of appearances, the world of our senses. The world of expressionistic art is the world of expression, of feelings of subjective processes."

The book is a valuable contribution to the general theory of esthetics.

State University of Iowa CARL E. SEASHORE

The Psychology of Conversation. By T. H. PEAR. London, New York, Thomas Nelson, 1939. Pp. ix, 171.

This little volume appears as a number 51 of a popular series (Discussion Books). Despite this fact it not only is written upon a high scholastic level, but its argument is excellently supported by experimental results gleaned from various technical psychological journals. It is also furnished with a bibliography of 58 items.

A list of the chapter headings indicates the scope of the work: I. Questions about Conversation; II. The Art of Conversation; III. Conversational Tact; IV. Conversations with Oneself; V. Remembering Conversation; VI. Subjects of Conversation; VII. Children's Conversations; VIII. Techniques of Interviewing; IX. Conversation in Psychotherapy.

It is a sufficient evaluative comment upon this book to remark that in general plan, style, and content it admirably serves the purpose for which it was intended.

Indiana University J. R. KANTOR

Rural Youth: Their Situation and Prospects. By BRUCE L. MELVIN and ELNA N. SMITH. Washington, D.C., U. S. Gov. Printing Office, Res. Monog. 15, Div. Soc. Res. W. P. A., 1938. Pp. xx, 167.

This monograph presents a most comprehensive survey of the present situation and future prospects of rural youth. Sociologists and psychologists alike will find the account of the inadequate economic opportunities, of the equally inadequate educational and recreational facilities a necessary base for the understanding of the problems of present-day youth. Any discussion of guidance and counseling techniques will naturally best stem from a thorough knowledge of the actual areas of living in which difficulties are most likely to be experienced. Some attempts to meet youth's problems are discussed. These attempts reveal the necessity for guidance and education if we are to have any maintenance, much less progress, of the citizenry standards of today.

Brooklyn College ROSALIND GOULD

Rural Families on Relief. By CARLE C. ZIMMERMAN and NATHAN L. WHETTEN. Washington, D.C., U. S. Gov. Printing Office, Res. Monog. 17, Div. Soc. Res. W. P. A., 1938. Pp. xxiv, 161.

Psychologists will find less direct interest in this monograph. The statistical survey is adequate and clearly presented, but only hypothetically can one deduce the psychological effects of migratory existence, of low status, of a narrowly restricted concept of the 'future,' upon the family heads and the development of their children, from these data. A clinical survey of the effects upon family relationships and the 'growing child' of protracted existence on relief would be most valuable. Sociologists would probably be interested in the important implications of these data for future social relations.

Brooklyn College ROSALIND GOULD

Avoir une âme: Essai sur les existences virtuelles. By ETIENNE SOURIAU. Paris, Annales University of Lyon, 1938. Pp. 141.

This book is a combination of philosophy and *belles lettres.* It contains little of interest to scientific psychology.

The Rice Institute F. A. PATTIE

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FERAL MAN AND EXTREME CASES OF ISOLATION

By ROBERT M. ZINGG, University of Denver

Feral man is the term for extreme cases of human isolation: either, as the present cases of the Wolf-children of Midnapore, of abandoned infants adopted and suckled by animals; or of older children who have wandered away, into the wilds to survive by their own efforts, unaided by human contact.

The name and concept of feral man was introduced into organized science in 1758 by the great classifier, Linnaeus, in the tenth and subsequent editions of his *Systema naturae*.[1] His account was based on the poorly attested cases reported in the historical sources prior to his time. These cases are the following.

(1) The Lithuanian Bear-boy, 1661.
(2) The Hessian Wolf-boy, 1344 (Linnaeus mistakenly dates this as 1544).
(3) Irish Sheep-boy reported by Tulp, 1672.
(3a) Bamberger Cattle-boy, end of the 16th century (a poor case).
(4) Wild Peter of Hannover, 1724.
(5) The Pyrenees boys, 1719 (a most dubious case).
(6) The girl of Cranenburg, 1717 (*Puella trans-isalana*, Linn.).
(7) The Songi girl from Champagne, 1731 (*Puella campanica*, Linn.).
(8) Jean of Liège.

As will be indicated in the tabular summary (pp. 500-503), Linnaeus, from the inadequate data, could have concluded little more than his terse characterization of feral man as *mutus, tetrapus,* and *hirsutus*. The last, *hirsutus*, is certainly in error since few of the cases are reported as hairy.

Buffon introduced the data and concept of feral man to the French about 1750 in his famous works. Apparently from him the concept fell into

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* Accepted for publication May 5, 1940.

[1] Carl von Linné, *Systema naturae*, 10th ed., I, 1758, 21.

the unscientific hands of Rousseau, who, in 1754, in his famous essay on inequality[2] appears to have found the case of the Hessian Wolf-boy as congenial to his simplistic philosophy of environmental determinism. This radical thesis down to the present day has done more than anything else to prejudice the case of feral man.

It was thirty years after the brief romantic treatment of feral man by Rousseau before the next important literary and philosophic discussion of feral man. The German romantic philosophy treats feral man in Herder's great work, *Ideen zur Philosophie der Geschichte der Menschheit,*[3] still cited in histories of anthropology. Herder's citation of feral man has no such sweeping theoretical implications as that of Rousseau. Herder's discussion is merely in connection with his treatment of the upright position of man, from which Herder mistakenly uses feral data in a preëvolutionary argument against man's descent from lower forms of animals.

Herder apparently became acquainted with the data of feral man through Schreber's contributions to German knowledge in an encyclopedic natural history similar to Buffon's earlier and more famous works. In his discussion, Schreber gives the detailed data on eight of Linnaeus' nine cases of feral man, putting *(3a)*, the Bamberger case, in a foot-note, where it deserves to be. To the remainder of Linnaeus' cases, Schreber adds the following.[4]

(9) The second Wolf-boy from Germany, that of Wetterau, 1344.

(10) The second of the Lithuanian Bear-boys, that of 1694.

(11) Later the data on a third Lithuanian Bear-boy were added to the literature from another source of 1698;[5] comparable to the bear boy of India reported alive in 1893 as cited by Sir James Frazer (see p. 491). This case is not included in the tabular summary because no data are given other than failure to master the upright position and language.

In France scientists were opposing the romantic and revolutionary thesis of Rousseau as late as 1812. In that year, Gall and Spurzheim, the fathers of phrenology, generalized on the subject of feral man unfavorably, without much regard for the facts of the particular cases except for brief notes on two other possible cases not mentioned elsewhere. They are, however, too inconsequential to be included in the tabular summary.[6]

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[2] J. J. Rousseau, *Discours sur l'origine et les fondements de l'inegalite parmi les homme,* 1754.

[3] J. G. Herder, Ideen zur Philosophie der Geschichte der Menschheit, *National Litteratur,* 77, 1784, 1070-1109.

[4] J. C. D. von Schreber, *Die Säugthiere in abbildungen nach der Natur mit Beschreibungen* (8 vols.), I, 1774-1846, 31.

[5] The original account of the third Lithuanian Bear-boy is given by the English physician to the Polish King, Bernard Connor, who saw the second case and quotes a letter from a witness of the third, the Netherlands Ambassador to London, J. P. van Kleverskerk. See Bernard Connor, *The History of Poland,* I, 1698, 348-350.

More substantial scientific opposition to Rousseau's romantic philosophy of feral man was implemented by the not altogether fair realism of Blumenbach, a great German scientist, whom the physical anthropologists acknowledge as the father of their discipline. In 1811 Blumenbach unfavorably reviewed the whole problem of feral man, giving the data in some detail, even if inadequately and unfairly, on all cases of feral man cited by Linneaus.[7]

Wild Peter was caught naked in a field in Hannover in 1724 and showed the characteristics of a feral case. Like many other such, he reached the hands of the ruler, in this case King George I of England and Hannover. Partially on this account Wild Peter attracted much attention and considerable material on him has come down to us. Almost a century later Blumenbach not only went through the English data but also through the Hannoverian archives in Hameln and elsewhere. This makes his paper an excellent source on Wild Peter even though we do not agree that Wild Peter has no more interest than an ordinary idiot. Though he lived to be 80 years of age and was not deaf he never learned to speak, easily wandered off and got lost, and could never be trusted with even the simplest task unsupervised.

When Wild Peter was first found he hated confinement, cunningly succeeded in escaping, and was filthy in his habits. At first he did not like bread but would peel green sticks to chew the juice out of the bark, besides eating cabbage, grass, and bean-plants. According to Blumenbach's critical data, Wild Peter supported himself for a year in the woods where he had been driven by his father, one Krüger, to whom he returned after a year, quite tattered. Again he was driven away with blows, like a strange dog, by his new step-mother.

One wonders if a complete idiot could have survived in the wilds, a year at a time unseen by human beings, until at least a year later, by sailors passing down the river, just before he was captured. Nor do Wild Peter's pictures, which Blumenbach says he was assured in England presented a good likeness, show Wild Peter as a hopeless idiot, but rather "a right good-looking old man, whom you would consider quite bright, if we didn't know better."

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[6] F. J. Gall, and G. Spurzheim, *Anatomie et Physiology du system nerveau en general et du cerveau en particular,* 1812, 41-42.

[7] J. F. Blumenbach, Von Homo sapiens ferus Linn. und namentlich von Hammelschen wilden Peter, *Beyträge zur Naturgeschichte,* 2nd pt, 1811, 10-44. This paper has been translated into English by Bendysche, *The Anthropological Treatises of Johann Friedrich Blumenbach,* 1865, 1-35.

Whatever Wild Peter's mentality was, imbecile and tongue-tied as he is represented to us, we cannot today think but that the tragic abuse that he had received from an unnatural parent and from his step-mother must have been some factor in his retardation. Whether or not this would now be admitted by Blumenbach, he did science a service in critically examining the new data he gathered, as well as in blasting the romanticism of Rousseau's "natural man." Still Blumenbach may be criticised for unfair treatment of some of Linnaeus' other cases, and for omitting the more important cases which had attracted wide attention in the interval. Such a case was that of the Wild Boy of Aveyron of 1799, who was observed by competent and distinguished scientists in France.

Blumenbach's critical review did not succeed in banishing feral man to the limbo of mythology. In 1830 Rudolphi, one of Blumenbach's first students of anthropology, as it was then understood to include physiology, was impelled to discuss the problem.[8] Blumenbach's argument against feral man was not, however, to be adequately answered until 1848. In this year appeared Tafel's substantial contribution to the problem of feral man. In his *Fundamentalphilosophie*,[9] he collected all the data on most of the cases in Europe of feral man. The work itself is a significant treatment of pre-Darwinian evolutionary thought; and, though it has never been cited in English histories of science, it well might be. Instead of dismissing and slurring the data other than for Wild Peter, as Blumenbach had done with the other cases, Tafel gives us all the data on Linnaeus' cases and adds several others.

After presenting the data in the manner closely followed by all the later writers who have presented them, Tafel then argues, in direct opposition to Blumenbach, that none of these cases involves congenital idiocy, citing writers on idiocy current in his day. He argues that the condition of feral cases is the result of their isolation from human society.

Besides the European cases prior to 1848 reported by Tafel, the horizon of feral man was greatly widened and its data enriched by reports in 1852 of six cases of wolf-children from India, similar to the recent ones of Midnapore. The distinguished author of the former account was Major General Sir William H. Sleeman, K.C.B.,[10] who spent 1849-50 in the Indian

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[8] K. A. Rudolphi, *Grundrisz der Physiologie* (2 vols.), I, 1830, 25-29.

[9] J. F. I. Tafel, *Die Fundamentalphilosophie, in genetischer Entwickelung mit besonderer Rucksicht auf die Geschichte jedes einzelnen Problems,* 1st pt., 1848, 44 ff.

[10] *An Account of Wolves Nurturing Children in Their Dens,* by an Indian Official. Jenkin Thomas, Printer, 9 Cornwall Street, Plymouth, 1852. Apparently unaware that the identical account had appeared in Sleeman's book (*A Journey Through the Kingdom of Oude,* London, Richard Bentley, 1848, 2 vols.), the material on the wolf-children was reprinted in the organ of the Linnaean Society, *The Zoologist,* 3rd ser., 12, 1888, (no. 135), 87-98.

Kingdom of Oudh (N. W. India), the region in which Midnapore is located. Here he was engaged in the suppression of the Thugs. General Sleeman recounts two cases which he appears to have seen personally, and four others of which he heard through persons on whose integrity this competent observer was willing to risk his high reputation for veracity. There is another case, that of an old man whom he himself saw, who may or may not have been reared among wolves. This case, unlike the others, is not included in the tabular summary.

Besides Sleeman's six cases of feral man, which brings the total to 17, several later cases from India have been sufficiently adequately reported and agree so well with the recent Midnapore case as to be worth consideration and inclusion in the tabular summary. These cases are:

(18) Case reported from Sultanpur;[11]
(19) Wolf-boy of Shahjejanpur;[12]
(20) Dina Sanichar, the Wolf-child of 1867 at the Sikandra Orphanage;[13]
(21) Second Sikandra Wolf-child of 1872.

Numerous other reports of cases from India are unsatisfactory.

The publication of General Sleeman's account of the wolf-children from India attracted to the problem of feral man, the great English cultural anthropologist, E. B. Tylor, who is rightly called the father of that subject in England. This new material, Tylor says, "makes it necessary to re-argue the question whether children have ever been carried away and brought up by wild animals,"[14] which had been dismissed by Blumenbach, the father of physical anthropology. Though Tylor agrees that, on his inadequate data, Blumenbach could not have done otherwise, Tylor's contribution shows the sharp divergence between physical and cultural anthropology which has carried down to the present in the problem of feral man. Tylor's acceptance was followed by Galton,[15] and Sir James Frazer[16] who cites a bear-boy in India reported alive in 1893, reminiscent of Lithuanian bear-boys of 1698.

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[11] H. G. Ross, *The Field* (London), 9, 1895, (no. 2237), 786.

[12] Anon., Wolf-children *Lippincott's Magazine,* 61, 1898, 121.

[13] Valentine Ball, *Jungle Life in India,* London, de la Rue and Co., Bunhill Row, 1880. For other materials see, India's wolf-children found in caves, *Literary Digest,* 95, 1927 (Oct. 8), 54-56. A copy of Dina Sanichar's obituary and his picture as well as a copy of the record of his admission were sent me (1939) from the Orphanage. The wolves from which he had been rescued were brought dead with the child to an English magistrate for the wolf-bounty. The name of this magistrate is elsewhere given as Mr. Lowe, a son-in-law of Sir W. Muir; but no official report appears in the literature, much less from the skeptical correspondent, W. F. Prideaux, Wolf boys, *Notes and Queries,* (6th series), 12, 1885, 178.

[14] E. B. Tylor, Wild men and beast-children, *Anthropological Review* (London), 1, 1863, 21-32.

[15] Francis Galton, The domestication of animals, *Transactions of the Ethnological Society of London,* N.S., 3, 1865, 136.

[16] James Frazer, *Fasti of Ovid,* II, 1929, 269-281.

Tylor's paper cites a report of missionaries from Polynesia of isolated native survivors of a massacre which is not strictly a feral case and so is not included in our tabular summary. Similarly not included is the case, that Tylor also accepts as probable, recorded from ancient Rome at the time of the conquest of the Goths. It is strange that myth should account for the founding of Rome by Romulus and Remus, and that history should record at its fall a similar case of a child suckled by a goat among the ruins of one of its villages.

The chief contribution of Tylor's paper to the data on feral man is the record of two cases from Overdyke, an asylum for foundlings established in Germany after the horrors of the Napoleonic Wars, when many children were lost or abandoned in the forests and fields. The second of the Overdyke boys had apparently lived almost wild in the forest. He climbed trees with remarkable ability to get eggs and birds from the nests. The eggs and birds he devoured raw, a habit of which he was never cured.

The other Overdyke case reported by Tylor was Clemens, a child who had been set out to herd pigs and given so little food that he had to suck milch sows and eat herbage with the pigs. When he first came they had to keep him out of the salad beds as though he were a pig-himself. The Overdyke cases are *(22)* and *(23)* in the tabular summary.

Number *(24)* is another case of a child who had lived with swine. She was seen in Salzburg and reported by Horn.[17] She was 22 years old, and by no means ugly despite having been brought up in a hog-sty among the hogs. She had sat there for so many years with her legs crossed, that one of them was crooked. She grunted like a hog and her gestures were brutishly unseemly in a human guise.

We must turn from the sole general discussion of feral man in English to the German for the next consequential contribution, that of Rauber of 1888.[18] Rauber, apparently unfamiliar with Tafel's earlier and even better complication of the materials, again presents the data on the classic cases of feral man. To these he adds several, one of which, that of the Wild-boy of Kronstadt, is well worth inclusion in our tabular analysis as *(25)*. Rauber's general grasp of the psychological implications of the problem of feral man is distinctly modern. Rauber's conclusions may be summarized in this quotation:

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[17] Wilhelm Horn, *Reise durch Deutschland, Ungarn, Holland, Italien, Frankreich, Grossbritannien, und Ireland* (2 vols.), I, 1831, 138.

[18] August Rauber, *Homo Sapiens Ferus oder die Zustande der Verwilderten und ihre Bedeutung für Wissenschaft Politik, und Schule,* 2nd. ed., 1888.

"We must not forget that the human child learns more during the first two years of his life under the influence of his living surroundings than in all the entire period afterwards. . . . If these influences fail, let us say until the fourth, sixth, or tenth year, how could it be possible that this could be without influence in the normal development of the brain?"

These cases of feral man group themselves into two classes: (a) those who have wandered away into the wilds to survive by their own efforts unaided by human contact; and (b) children nurtured by wild or domesticated animals. Strictly not feral cases are the similar ones of children shut away from human association by cruel, criminal, or insane parents. These furnish the commonest case and are reported in the press from time to time. They show the same effects from isolation; but they more commonly recover normal minds and personalities not only due to the shorter period of isolation involved before they attract the attention of neighbors or child protective societies, but also due to the fact that they do not have animal or other wild conditioning to unlearn. To this group belongs the famous case of Kaspar Hauser. His case, like others of this sort, is rightly classified by Tredgold as *Amentia* of Isolation,[19] as also are the deprivations suffered by the deaf and dumb.

(a) True feral cases are rarer than *isolation amentia* in these times of modern communication and contact. Of feral cases the rarest are those of children lost in the forests to survive to adolescence. Yet in the disasters of the great wars of the 18th and 19th centuries several cases were reported on the scientific authority of men like the great English anthropologist, Tylor (Second Overdyke case), or an educator like Itard (The Wild Boy of Aveyron). We may suspect that in post-war Russia, among the hordes of abandoned children, some may have wandered away into the wastes; and, but for the rigors of the climate, may have survived. In the present wars in China and Spain, children may have been reduced to this feral condition. As the publication of each new case in the past has brought reports of others, perhaps this publication may bring others to light.

The Wild Boy of Aveyron,[20] who appeared in 1799, offers our best evidence for a case of this type. During that year, in France, hunters pulled a boy out of a tree, who had apparently got lost or wandered off into the woods as a child and survived until found at 12 years of age as a wild creature living on nuts, and berries. He gave no evidence of nurture by animals and could walk, run, and climb like a wild thing. He was examined by Pinel, the leading psychologist in France of that day. Pinel called him an incurable idiot, despite the fact that he had had animal intelligence enough to have lived alone in the wilds from nature's products, unaided by man or animals. Not daunted by the pronouncement of the great psychologist, the education of this wild boy was undertaken by Itard, a doctor of medicine and a teacher in a special school for the deaf and dumb. Itard gives a full account of the education of this wild boy. By this we see that with the best of educational method for deaf, dumb, and sub-normal children (which was surprizingly well-advanced in France by the educational theories of Rousseau, set in motion by the French Revolution then at its height), this wild boy lost his bestial appearance and manners and became an appealing but much sub-normal person. He never learned more than a few words before his death at 40 years of age. Kamala, the elder of the Wolf-children of Midnapore, learned more words, about 50, and could use some of them intelligibly in short sentences before she died at seventeen after 9 years of education. The Wild Boy of Aveyron is analyzed here in the table as case *(26)*.

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[19] A. F. Tredgold, *Mental Deficiency (Amentia),* 1920, 304 f.

[20] J. M. G. Itard, *The Wild Boy of Aveyron,* Translated by George and Murial Humphrey, 1932.

Other cases of feral man of this type made better recoveries; much greater than those recorded for feral children reared by animals, as Kamala was. Cases that recover are of the greatest significance for this study. In science it is the exception that proves the rule. Thus of particular importance are the cases of regaining the ability to speak fluently together with the other attributes of a developed human personality.

Such a case is *(7)* in the tabular analysis, the Wild Girl of Songi (Champagne), whom Linnaeus named the *Puella campanica*. The sources on the *Puella campanica* are inadequate and especially incommensurate with the importance of this case. They are incomplete or defective, especially with regard to how long she had lived in isolation and regarding the mysterious lady who had first undertaken her education, taught her to embroider, etc., before she appeared at Songi. Also the primary data are marred by fantastic theories of her origin as an Esquimo or on the Island of "Guoadalupe." Still we are inclined to admit Rauber's conclusions "that it is impossible to doubt either the intelligence of the girl, or her former wildness."

The defects in the data are the more lamentable because of the crucial light that the case of this girl of Songi could throw upon the question of isolation and feral man, due to her not inconsiderable recovery of human faculties. The record would seem to merit some consideration since it cites contemporary persons of importance, such as the Intendent of Champagne and the Bishop of Chalôns, the latter the one who provided her with asylum in a Catholic hospital.

The girl of Songi is the only case of this sort, so far recorded, who recovered speech, to talk distinctly if somewhat brusquely. She clearly seemed to be intelligent since the record shows that she learned easily what was shown her. That the growth of her intelligence was sufficient for her to assimilate enough of the Catholic religion to become a nun seems certain enough. But, through the defect in the records as to the degree of her exposure to isolation, no conclusions seem justified despite the remarkable recovery of the frog-and-fish girl recorded in the beginning to become a devout nun, who had assimilated enough of the ability to speak and of the Catholic religion to say:

> "Why should God have searched for me and saved me from the power of wild animals, and made me a Christian? Should this have happened in order to leave me and make me die from hunger? This is not possible. I know only Him; He is my Father and the Virgin Mary is my mother; they will take care of me."

In early anthropological sources there are records for another interesting case of substantial recovery from a half-wild life caused by human neglect of a fellow creature. The early writer on anthropology, Michael Wagner,[21] gives data on this interesting case, not cited by any of the previous writers, apparently because it is not a case of completely feral man. Wagner extracts from the records of Zips, Hungary, the account of a poor creature who, through human neglect, had fallen into a half-wild condition. That he had lived in some degree of human association was shown by his gratitude for favors. Under the decent care of the unknown scribe, whom Wagner quotes, his mental condition improved rapidly, though his physical condition presaged an early death. He learned to speak Slovak sufficiently to be largely intelligible to his benefactor; and, though he persisted in an invented language of his own, he came to understand German. His case offers comparisons to that of Kaspar Hauser in the improvement noted, as well as in the convulsive movements that he fell into under emotional strain. Like all other cases, this man from Zips showed a surprizing indifference to sexual impulses, though he was a mature man. This case is *(27)* in the tabular summary.

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[21] Michael Wagner, *Beyträge zur Philosophischen Anthropologie und den damit verwandten Wissenschaften* (2 vols.), I, 1794, 259-264. (Copy in Harvard Library.)

The most significant case of recovery, and one with the best attested data, is Kasper Hauser *(28)*. This is a case of *isolation amentia;* but it is especially comparable to feral ones because his only remembered associations were with toy hobby-horses. These conditioned him to horses as feral cases are conditioned to the species of their animal associates. From the lack of real animal association, he had, however, nothing to unlearn.

Kaspar Hauser appeared in Nürnberg in 1828 at about 17 years of age. He was barely able to walk a few steps, but could write the name Kaspar Hauser by which he has always been known. He was unable to utter more than an unintelligible parrot-like phrase, "A Reutä Wähn, wig mei; Vötta Wähn is," perhaps very garbled German for "I wish to be a rider as my father is." He also repeated the word *Ross* (Bavarian dialect for "horse"). When placed in a jail cell, he was given a child's hobby-horse which he played with for hours on end, oblivious to everything else. He could eat only bread and drink only water, on which he later said he had lived as long as he could remember. When he appeared he was so weak that he could scarcely lift the hobby-horse across the threshold.

He became the ward of the town of Nürnberg and was placed under a very competent teacher, G. F. Daumer, who has left us an excellent and scientific account. He also attracted the attention and guardianship of one of the greatest of the jurists of Bavaria, Paul J. Anselm, Ritter von Feuerbach, whose authority is again unimpeachable for honesty and veracity.

Kaspar Hauser, since childhood apparently, had been shut up in a cellar dungeon and fed bread and water as long as he could remember later. That he was fed and cared for physically shows some evidence of human contact and association, which probably had something to do with his remarkable recovery of human qualities. In his dungeon cell he had had two hobby-horses and a toy dog, which accounts for his crying "Ross" and playing with a child's hobby-horses for hours at a time when he was released and so unexplicably appeared before the town gates of Nürnberg.

From lack of all other association and no childhood association with animals, Kaspar Hauser appeared into this world as a *tabula rasa* rather than as animal-conditioned. In the five short years of life accorded to Kaspar Hauser, he lived a whole life, developing mentally from a child of three years in an adolescent body of seventeen who played with hobby-horses, to a young man who spoke and wrote fluently and served Feuerbach as a legal clerk for a short time.

Due to deformations in his legs from having sat all his youthful years in his narrow cell with his legs stretched out before him, he never learned to walk well or to run. But due to his years of playing with hobby-horses, he loved real horses and had no fear for them. His weak legs, calloused from life-long sitting, were perfectly adapted for riding and he immediately developed into a horseman who could out-ride the troops. He had apparently been given hobby-horses to create in him this interest with the intention of his jailer that he should join the Bavarian cavalry. When he appeared in Nürnberg he carried a letter to the cavalry commander, asking him to be put in this service.

Instead, however, of getting lost in the army, he became the famous ward of the town, known all over Europe who attracted visitors from everywhere. When it was broadcast that he had developed so much that he was able to write his autobiography, the same forces that had ordained this "crime against a human soul" of his incarceration, climaxed it by assassinating this harmless and attractive youth who had won the pity and admiration of Europe.

Who was Kasper Hauser? Why was he kidnapped, imprisoned, and finally assassinated? All puzzles, now insoluable, that have been argued pro and con in over a thousand books, pamphlets, and articles.[22] Now that the German petty kingdoms are no more, it is safer to investigate than when Feuerbach was stopped in his search by the "colossus that guards the gates of certain castles;" but it is too late to question witnesses. Modern opinion in Germany agrees with the hints of Daumer and Feuerbach that Kaspar Hauser was the legitimate heir to the throne of the petty Grand Duchy of Baden,[23] who had been sequestered soon after birth to make way for the morganatic Hochberg line, but who was kept alive as a hostage lest the plot go awry.

The historical aspects of this great unsolved crime of a century ago are as interesting as a romance, but the scientific interest is of far greater value and rests on better evidence. This is one of the rare cases of isolated individuals to regain almost a complete human personality. The best literature on Kaspar Hauser, his own autobiography, and the evidence of those who knew him best has lately been collected in Germany.[24]

(b) The strangest form of feral condition is that of human children suckled by animals. Such a case is the recent one of the Wolf-children of Midnapore, which is the best attested on record because several persons are recorded as actually seeing the children living as animals among animals. The almost unique element in the recent case is not the adoption of infants of wild animals; but the fact that only in the present case do we have a careful record of nine years in human association after the rescue by the man who, in company with several others, saw the children in association with the animals which they killed before the children were brought again into human society.[25] The Midnapore girls, Amala and Kamala, are tabulated

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[22] Peitser-Ley, *Kaspar Hauser,* Druck von C. Brugel & Sohn AG. Ansbach.

[23] The best account in English of the Hauser-legend is by Elizabeth Edson (Gibson) Evans, *The Story of Kaspar Hauser from Authentic Records,* 1892. As late as 1930 the same hypothesis was re-stated in German by Luise Bartnig, *Altes und Neues zur Kaspar Hauser,* 1930.

[24] Herman Pies, *Kaspar Hauser,* 1926. Here included is Feuerbach's important account which was translated into English more than a century ago, *Caspar Hauser* translated by H. G. Linberg, London, 1833, Simpkin and Marshall.

[25] J. A. D. Singh, The wolf-children of Midnapore, (manuscript unpublished). See P. C. Squires, "Wolf children of India, this JOURNAL, 38, 1927, 313-315; W. N. Kellogg, More about the "wolf children" of India, *ibid.,* 43, 1931, 508-509; A further note on the "wolf children" of India, *ibid.,* 46, 1934, 149-150.

as numbers *(29)* and *(30)*. In all other such cases, the children pass through several hands after the rescue from animals, before reaching the person who recorded the case.

The entire status of the problem of feral man has hitherto been uncertain because of the lack of recorded witnesses of the animals as foster-mothers to human beings. Cases reported as secured from wild or domesticated animals have just been reviewed from the historical records of Europe from the 14th century (the Hessian Wolf-boy) down to the nineteenth (Clemens of Overdyke). From India we have seen that reports of this sort, as the present cases, have been much more frequent. This is probably because wolves are not killed on superstitious grounds and steal many children every year. Apparently a mother-wolf in suck occasionally adopts one of the victims. In the region of India surrounding Midnapore, children reported as rescued from wolf foster-mothers have been so frequent since the English entered the Northwest Provinces (Bengal) in 1850, that it actually appears that one or more wolf-children have been under institutional care somewhere on that continent, ever since that time. Though Kamala died in 1929 there are reputable accounts of others that are worth checking, who are possibly alive in India today.

J. H. Hutton, William Wyse Professor of Social Anthropology, St. Catherine's College, Cambridge University, in the *London Times* of July 24, 1939, communicates the following reference to such a case, with which he became familiar while serving in the high scientific position as Director of the Census of India. This case reported in the *Journal of the Bombay Natural History Society* by E. C. Stuart Baker in 1920 is the more interesting because it is the only one involving a leopard foster-mother. There was an isolated and brief mention of this case,[26] which I took as fantastic, especially since so fierce an animal as the leopard was involved. Yet this case has been again brought to attention through so distinguished a correspondent to the *London Times* as Professor Hutton, whose experience in India makes him an excellent judge of its credibility. Such things counter-indicate rejecting this or any other case without every possible checking. The data on this case follow.[27]

> Before leaving the subject of feline senses it may be of interest to relate a story of a leopard child which has not yet ever been published though it was pretty well known at the time.
>
> In the North Cachar Hills, where the boy was found . . . When questioning a man why he wanted exemption from such labor he told me that he had a little "wild" son to look after and as his wife had recently died, he could not leave the village to work or the boy would run back to the jungle.
>
> I accordingly went outside the court to see the "wild child" and satisfied myself as to the truth of the story. There sure enough outside was a small boy about seven years of age, or less, squatted on the ground like a small animal; directly I came near him he put his head in the air and sniffed about, finishing by bolting on all fours to his father between whose legs he looked like a small wild beast retreating into a burrow. Looking closer at the child I saw that he was nearly or entirely blind from some form of cataract and his body was covered with the white scars

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[26] Wolf children of India, *Living Age,* 332, 1927, 1020-1022. This article, like that in the *Literary Digest,* 95, Oct. 8, 1927, 54-56, is principally about a then current case, that of Maiwana, some 75 miles from Allahabad in British India. In 1927 he was reported in a mental hospital in Bareilly, but my correspondence with India on this case, has so far revealed only that the Institution is now closed, so I have added nothing to the meager notes given in the above articles, which are certainly worth checking farther.

[27] E. C. Stuart Baker, The power of scent in wild animals, *J. Bombay Natural History Society,* 27, 1920, 117-118.

of innumerable healed tiny cuts and scratches. Struck with his appearance I asked the father to tell me about the boy and he then narrated the following wonderful story which I fully believe to be true, but which my readers must accept or not as they think fit.

It appears that about five years before I saw the father and son, the Cachari villagers of a village called Dihungi, had found two leopard cubs close to their village which they killed. The mother leopard had tracked the murderers of her children back to the village and had haunted the outskirts for two days. The third day a woman cutting rice in some cultivation close to the village laid her baby boy down on a cloth whilst she went on with her work. Presently hearing a cry, she turned around and saw a leopard bounding away and carrying the child with it. The whole village at once turned out and hunted for the leopard and baby but without success and finally they were forced by darkness to leave the boy, as they supposed to be eaten by the leopard.

Some three years after this event a leopardess was killed close to the village by a sportsman who brought in the news of his success together with the information that the leopard had cubs which he failed to secure. On hearing this the whole village turned out and eventually captured two cubs and one child, the boy of this story. He was at once identified by his parents, claimed by them, and their claim admitted by the whole village.

Subsequently when visiting Dihungi I interviewed the head man and also the man who actually caught the child and they both corroborated the father's tale in every detail. It appeared that at the time he was caught the child ran on all fours almost as fast as an adult man could run, whilst in dodging in and out of bushes and other obstacles he was much cleverer and quicker.[28] At that time he was only suffering from cataract to a slight extent and could see fairly well, but after he was caught his eyes rapidly became worse. His knees, even when I saw him and he had learnt to move about upright to a great extent had hard callosities on them and his toes were retained upright almost at right angles to his instep.[29] The palms of his hands and pads of toes and thumbs were also covered with very tough horny skin. When first caught he bit and fought with everyone who came within reach of him and, although even then affected in his eyes, any wretched village fowl which came within his reach was seized, torn to pieces and eaten with extraordinary rapidity.[30]

When brought before me he had been more or less tamed, walked upright when startled into extra rapid motion, was friendly with his own villagers whom he seemed to know by scent,[31] would eat rice, vegetables, etc., and consented to sleep in his father's hut at night. Clothes, being a Cachari child of tender years, he had not been introduced to.

His blindness was not in any way due to his treatment by the leopard—if the story is true—as I found that another child, a couple of years older, and the other also had the same cataract. At the same time the defective sense of sight may well have intensified his smell as the loss of the one must have caused him to rely more on the other. When caught the child was in perfect condition, thin but well covered, and with a quite exceptional development of muscle.

The inadequate data in this case are tabulated as number *(31)*.

There are other stories of feral cases in India that are still alive and should certainly be worth checking. Professor Hutton has also referred the writer to mention of such a case in the *Illustrated Weekly of India* (Feb. 5, 1933, p. 37) which gives the following meager details, which are now being checked by correspondence:

A British Officer at Jhansi has rescued a child from a pack of wolves which brought it up. This strange creature has been exhibited at the Gwalior Baby Week, presumably as a bait to see the more instructional of these excellent shows. At the moment Dr. Antia, the Chief Medical Officer of Gwalior, is trying to cure the child of the wild habits it has learnt from its wild companions in the jungle, and the method of progression on all fours has already been changed to the upright carriage. The Indian *beriga,* or wolf, is found in Rajputana particularly and in most other parts of India except Lower Bengal. Considering that it frequently carries off children besides sheep and goats, it is remarkable this child was allowed to live.

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[28] Similar details given of the Wolf-children of Midnapore.

[29] The same feature is reported by Rev. Singh of the Wolf-children of Midnapore.

[30] Similar things are told of several feral cases.

[31] Feral man is often reported as with highly developed sense of smell, though here it is probable that it was even greater due to compensation for defective sight.

Another report of an Indian wolf-child who may still be alive is the Maiwana case of 1927.[32] Though I have not been able to see the original reports from India, details on this case are given in *The Literary Digest,*[33] and the *Living Age.*[34] These reports would appear possible to check and well worth the trouble.

The value of careful checking in the field of the data of alleged cases of feral man is shown by the case of Lucas the alleged Baboon-boy of South Africa. The newspaper accounts of this case looked worth sending to Dr. R. R. Gates, of the University of Bristol, whose interest in this problem and whose strategic situation in London, enabled him to put almost all the facilities of the British Empire, including the well-known Letters column of the *London Times,* to the service of this study. Dr. Gates forwarded the clippings to Dr. Raymond Dart, Dean of the Medical School of the University of the Witwatersrand. Dr. Dart's importance in South Africa is sufficiently great to have caused the highest officials in South African Mounted Police to check their records back to 1904, where there was convincing but hear-say evidence from a constable that Lucas had been rescued from baboons by two constables, now dead. It seemed sufficient basis for the recent preliminary notes on this case that were published in both the popular and scientific press.[35] Continuing careful work in South Africa through the researches of Dr. Dru Drury and Dr. van Heerden, however, broke down completely any claims for Lucas as a case of feral man.[36]

The present writer has unfortunately been unable to get in touch with scientists in India to check and re-check similarly the cases reported from there. Through the good offices of men familiar with India through much travel or long residence, such as Professor R. R. Gates and Professor J. H. Hutton, every effort, however, has been made to check the current cases. The report of Rev. J. A. L. Singh is attested to not only by Bishop H. Pakenham Walsh, Tadagam P. O. Coimbatore, but also was investigated by the English magistrate of Midnapore, who went over Rev. Singh's material on the spot. Publication of the valuable document, Rev. Singh's diary account of some 150 pages of the rescue and life in human society of the two wolf-children of Midnapore, has been held up for almost three years by me while working with many other interested scientists making every effort to check the account. This labor has not succeeded in breaking down any of the data reported by Rev. Singh.

Not so much for this case but for the others apparently still alive in India, it might be worth while for one of the research institutions of the United States even to send scientists to gather and check materials on the spot.

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[32] The original sources on these cases are given me by Professor Hutton as the Indian periodicals, *The Englishman,* April 7, 1927; and *The Pioneer,* April 5, 1927.

[33] *Op. cit.,* 95, 1927, (Oct. 8), 54.

[34] *Op. cit.,* 332, 1927, (no. 4307), 1021 f.

[35] J. P. Foley, The "baboon boy" of South Africa, this JOURNAL, 53, 1940, 128-133.

[36] R. M. Zingg, More about the "baboon boy" of South Africa, this JOURNAL, 53, 1940, 455-462.

TABULAR SUMMARY FERAL MAN AND CASES OF EXTREME ISOLATION

Case	Sex	Years old when rescued	Years old when isolated	Obvious idiot	Associate animal	Fierce behavior when caught	Animals unafraid of	Shares food with	Grew up wild in the forest	*Hirsutus*, (Hairy)	*Tetrapus*, four-footed motion	Taught to walk	*Mutus* (dumb)	Made animal-like sounds
(1) First Lithuanian Bear-boy of 1661 ?(1657)	Male	12			Bear		X				X	X	X	X
(2) Hessian Wolf-boy 1344	Male	7 to 12	3		Wolf						X	X	?	
(3) Irish Sheep-boy 1672	Male	16			Sheep								X	Bleat
(3*a*) Bamberger Cattle-boy	Male				Cattle	Escaped					X			
(4) Wild Peter of Hannover 1724	Male	12	11?						X				X	
(5) The Pyrenees Boys 1719	Males										X			
(6) Girl of Cranenburg 1717	Female	18							X				X	
(7) Girl of Songi, 1731	Female								X				X	
(8) Jean of Liege	Male	21	5						X					
(9) Second Wolf-boy, Wetterau	Male				Wolf									
(10) Second Lithuanian Bear-boy, 1694	Male	10			Bear						X	X	X	X
(11) Third Lithuanian Bear-boy	Male	12			Bear						X		X	X
(12) Sleeman's first Wolf-child	Male				Wolf	Wild		Dog			X	No data	X	X
(13) Sleeman's second Wolf-child	Male				Wolf	X					X	No data	X	No data
(14) Sleeman's third Wolf-child	Male				Wolf	No data				X			X	X
(15) Sleeman's fourth Wolf-child	Male				Wolf						X	X	X	
(16) Sleeman's fifth Wolf-child	Male				Wolf						X	No data	X	
(17) Sleeman's sixth Wolf-child	Male				Wolf	X	X				X		X	
(18) Wolf-child of Sultanpur	Male	4			Wolf						X			X
(19) Wolf-child of Shahjehanjur	Male	6?			Wolf					X	X		X	X
(20) Dina Sanichar	Male	6		X	Wolf	X					X		X	
(21) Second Wolf-child Sikandra, India	Male	10			Wolf	Too weak					X		X	X
(22) Second Overdyke wild-boy	Male								X					
(23) Clemens of Overdyke	Male				Pig						X		Unintelligible	
(24) Swine-girl of Salzburg	Female				Pig						Crippled		X	Pig-like
(25) Wild-boy of Kronstadt	Male	23							?	X	?		?	
(26) Wild-boy of Aveyron	Male								X				X	
(27) Half-wild boy of Zips, Hungary	Male								Half wild				X	
(28) Kaspar Hauser	Male	17										X	X	
(29) Amala, younger Midnapore girl	Female	2			Wolf	X		Dog			X	X	X	X
(30) Kamala, elder Midnapore girl	Female	8			Wolf	X		Dog			X	X	X	X
(31) Leopard-boy of India	Male	5	2		Leopard	X					X			

TABULAR SUMMARY FERAL MAN AND CASES OF EXTREME ISOLATION—*Continued*

Case	Hearing sharp	Sense of smell sharp	Could see at night	Prowled at night	Food	Smelled food before eating	Severe illness after capture	Eats or drinks like an animal	Sex expression inhibited	Sense of shame undeveloped	Years in human society	Tears off clothing	Fond of company of animals
(1) First Lithuanian Bear-boy of 1661 ?(1657)					Cabbage Grass, Meat							X	
(2) Hessian Wolf-boy 1344					No data						Short time		
(3) Irish Sheep-boy 1672					Grass, hay	X							
(3a) Bamberger Cattle-boy					No data								
(4) Wild Peter of Hannover 1724					Bark, grass				X		68	X	
(5) The Pyrenees Boys 1719													
(6) Girl of Cranenburg 1717					Herbs, leaves					wore apron			
(7) Girl of Songi, 1731					Frogs, fish				X				
(8) Jean of Liege		X			Greens, roots								
(9) Second Wolf-boy, Wetterau											68		
(10) Second Lithuanian Bear-boy, 1694					No data								
(11) Third Lithuanian Bear-boy					Bread	X							
(12) Sleeman's first Wolf-child					Raw meat			?				X	Dogs
(13) Sleeman's second Wolf-child					Raw meat			laps like a dog				X	
(14) Sleeman's third Wolf-child					Raw meat	X							
(15) Sleeman's fourth Wolf-child					No data								
(16) Sleeman's fifth Wolf-child	X				Bread, meat	X							
(17) Sleeman's sixth Wolf-child					Raw meat							X	Wolf
(18) Wolf-child of Sultanpur					Raw meat								
(19) Wolf-child of Shahjehanjur					Raw meat						14	X	
(20) Dina Sanichar					Raw meat	X		Sharpened teeth on bones			28	X	
(21) Second Wolf-child Sikandra, India				X	Raw meat	X							
(22) Second Overdyke wild-boy					Birds' eggs								
(23) Clemens of Overdyke					Greens								Pigs
(24) Swine-girl of Salzburg													
(25) Wild-boy of Kronstadt					Grass, roots, meat				?			X	
(26) Wild-boy of Aveyron	X				Nuts, roots				X				
(27) Half-wild boy of Zips, Hungary					Roots, raw meat				X				
(28) Kaspar Hauser	X	X	X		Bread, water		X		X	X			Horses
(29) Amala, younger Midnapore girl	X	X	X	X	Raw meat		X	Laps milk			1	X	Animals
(30) Kamala, elder Midnapore girl	X	X	X	X	Raw meat		X	Gnaws bones on floor		X	9	X	Animals
(31) Leopard-boy of India		X			Raw meat								

TABULAR SUMMARY FERAL MAN AND CASES OF EXTREME ISOLATION—*Continued*

Case	Shuns humans	Completely selfish	Learned to be unselfish	Attached to person who feeds	Learned to like human society	Insensitive to heat and cold	Expresses anger and impatience	Expresses joy	Sympathy between wolf-children	Liked music	Degree of recovery	Number of words learned	Learned to do simple work
(1) First Lithuanian Bear-boy of 1661 ?(1657)											Little		
(2) Hessian Wolf-boy 1344											Little		
(3) Irish Sheep-boy 1672						X					No data		
(3a) Bamberger Cattle-boy											Great		
(4) Wild Peter of Hannover 1724											Little		When watched
(5) The Pyrenees Boys 1719													
(6) Girl of Cranenburg 1717											Great		None
(7) Girl of Songi, 1731											Great	To talk	
(8) Jean of Liege											No data		
(9) Second Wolf-boy, Wetterau											Considerable	?	
(10) Second Lithuanian Bear-boy, 1694											Slight	To talk	
(11) Third Lithuanian Bear-boy											No data		
(12) Sleeman's first Wolf-child	X					X							
(13) Sleeman's second Wolf-child				X									
(14) Sleeman's third Wolf-child													
(15) Sleeman's fourth Wolf-child	X												
(16) Sleeman's fifth Wolf-child													
(17) Sleeman's sixth Wolf-child													To light a pipe
(18) Wolf-child of Sultanpur											Became policeman		
(19) Wolf-child of Shahjehanjur						X					None		
(20) Dina Sanichar						X			X		None		A little
(21) Second Wolf-child Sikandra, India									X		Little		
(22) Second Overdyke wild-boy													
(23) Clemens of Overdyke													
(24) Swine-girl of Salzburg											None		
25) Wild-boy of Kronstadt							X			X	Little	None	Filled water-jug
26) Wild-boy of Aveyron		X				X	X				Little	3 words	
(27) Half-wild boy of Zips, Hungary							X	X			Much	To talk	Chores
(28) Kaspar Hauser								X		X	Very great	Fluent	Clerical
(29) Amala, younger Midnapore girl	X					X	X		X		Some		
(30) Kamala, elder Midnapore girl	X		X	X	X	X	X		X		Some	50 words	Errands
(31) Leopard-boy of India													

TABULAR SUMMARY FERAL MAN AND CASES OF EXTREME ISOL

Case	Learned a foreign language	Kept eating things clean	Liked to smoke tobacco	Learned to like warm fire	Rooted like a pig	Chased cattle from fields on signal	Preferred the color red	Imitate the cries of birds
(1) First Lithuanian Bear-boy of 1661 ?(1657)								
(2) Hessian Wolf-boy 1344								
(3) Irish Sheep-boy 1672								
(3a) Bamberger Cattle-boy								
(4) Wild Peter of Hannover 1724				X				
(5) The Pyrenees Boys 1719								
(6) Girl of Cranenburg 1717								
(7) Girl of Songi, 1731								
(8) Jean of Liege								
(9) Second Wolf-boy, Wetterau								
(10) Second Lithuanian Bear-boy, 1694								
(11) Third Lithuanian Bear-boy								
(12) Sleeman's first Wolf-child								
(13) Sleeman's second Wolf-child								
(14) Sleeman's third Wolf-child								
(15) Sleeman's fourth Wolf-child								
(16) Sleeman's fifth Wolf-child						X		
(17) Sleeman's sixth Wolf-child								
(18) Wolf-child of Sultanpur								
(19) Wolf-child of Shahjehanjur								
(20) Dina Sanichar		X	X					
(21) Second Wolf-child Sikandra, India								
(22) Second Overdyke wild-boy								X
(23) Clemens of Overdyke					X			
(24) Swine-girl of Salzburg								
(25) Wild-boy of Kronstadt								
(26) Wild-boy of Aveyron				X				
(27) Half-wild boy of Zips, Hungary	A little Latin							
(28) Kaspar Hauser							X	
(29) Amala, younger Midnapore girl								
(30) Kamala, elder Midnapore girl							X	
(31) Leopard-boy of India								

SUMMARY AND CONCLUSIONS

In studies of human behavior and development, scientific experiment is often forced to take the direction of animal experimentation or the use of accidental cases approximating a controlled experiment. No scientist would dream of an actual experiment with a human subject under any such rigorous control as removing all human association and contact. Cases of feral man, and certainly the recent one of the Wolf-children of Midnapore, offer objective data, subject to this control, of fundamental importance to theories of human studies. Thus we may say that we have satisfactory evidence from far-away India of a crucial experiment made by a mother-wolf.

From his inadequate data, Linnaeus hit upon two of the constant features of feral behavior—*mutus* and *tetrapus*. All the cases of feral man agree with this one of Midnapore in being mute, often despite an acute sense of hearing. Most cases made animal-like sounds when recovered (see table.) Also, among animal-reared children there is but one case of recovery of human speech beyond the degree of recovery recorded of the Wolf-child of Midnapore, Kamala, of ability to say about fifty words. The failure of development of speech, the most characteristic human faculty, is of the greatest significance in the retardation of their mental faculties.

All cases of feral man reared by animals agree with the Midnapore cases and with Linnaeus' characterization, *tetrapus*, in the characteristic of walking on hands and feet or hands and knees, despite man's biped structure of limbs. In all cases reported, this biped structure is sufficiently modified by conditioning to a surprisingly rapid quadruped locomotion that it is only by protracted education that the erect position of man is attained for walking. In none of the cases of feral man, reared by animals, can they run upright; but, like Kamala, must take the four-legged position for rapid movement.

Despite the obvious gross human structure of feral man in brain and nervous system, we unfortunately have no post-mortem material on these Wolf-children of Midnapore or any other strictly feral cases. The only material that we have is on Kaspar Hauser. This is of paramount importance because he made a substantially complete recovery of human faculties and speech after severe and long-continued isolation. Yet his brain was undersized with an underdevelopment of the cerebrum and of the convolutions, approaching an impression of atrophy. Though the degrees of recovery vary from almost complete in the case of Kaspar Hauser to practically nothing in such cases as the idiot wolf-child, Dina Sanichar, the initial human behavior of all cases appears idiotic, despite animal-like keenness of the senses of smell, hearing, and sight (the latter, especially at night), so often reported of feral cases.

The universal initial imbecility of feral man when regained to human society raises the question of throwing out the whole of the data as irrelevant to problems of human heredity and psychology, on the grounds that congenital idiocy is involved, if only because idiotic children would be more apt to be abandoned than normal ones. It would seem improbable, however, that out of some 30 creditable cases of feral man, pure chance should have operated to the result that all should be natural idiots. Further the case of Dina Sanichar, an obvious natural idiot, gives a datum in feral cases to work from.

Two other lines of considertion counter-indicate universal idiocy. First, several of the cases are of children who have wandered into the wilds, alone, and have survived to adolescence by their own wits and strength, unaided by human association, which would require considerable native intelligence. Secondly, in many of the cases of animal-reared children, as in the Midnapore case, there is the degree of recovery shown by all cases save Dina Sanichar, when in human society for some years. In the Rev. Singh's diary on the Wolf-children of Midnapore, one sees evidence of great stress and strain in the struggle of the elder child, Kamala, after the death of her younger human companion of the wolf-den. This struggle was to orient her affections and interests from animals to human beings. At first, she seemed about to sink into the gloom of unrelieved and despondent idiocy; but soon she began to take a glimmering of interest in her new surroundings and associations without the leadership of the younger and more plastic child, Amala, whose early death after a year in human contact was most unfortunate for the development of the elder girl.

The account reveals how the Reverend and Mrs. Singh were forced to treat both children when first rescued as newly born human babies. In the social, and in a very real sense, this is what they both really were upon their introduction into human society at the ages of one-and-one-half and of eight years of age. They had to be kept in bed, to be fed milk from a bottle and to be clothed only in a diaper-like breach-cloth. The breach-cloth had to be sewed on because they tore off clothing as savagely as animals would do.

This last reveals that their human handicap was greater than that of the difference between their chronological and mental ages. They had to unlearn all their animal behavior patterns and habits as well as shift their interests and sympathies from animals to human beings. Rev. Singh thought that the first step was successfully accomplished when the wolf-children got over their craving for raw meat, on which, with animal milk, they had lived.

Their development of human behavior was slow due to the inhibitions of this animal patterning. The elder did finally learn control in evacuation and stopped tearing off her clothes. At long last, she came to demand her "fok" (frock) when company came or the children went out for a walk, though no color appealed to her except red.

The complete account of the Rev. Singh brings out clearly the difficulties in the reëducation of Kamala to the erect position in walking which took years and a complicated series of exercises. Yet her mastery of man's erect posture was so incomplete that for running Kamala, like other cases of animal-reared children, had to go on all fours. When she ran thus, it was hard for a person to overtake her.

In the complete account we see a development in language from animal communication to rudimentary human behavior in this essentially human faculty. Kamala mastered about 50 words which she was able to use in sentences of at least a subject and predicate. Thus the Rev. Singh's diary account of Kamala records the transformation of a human creature from an effective animal into a pathetic little sub-normal girl, who gave no one the impression of being idiotic; but one who became the favorite child in the orphanage where she was so fortunate as to live for nine years with many children of her own age under the kind care of the Rev. Singh and his wife.

If we speak of idiocy in many of these cases of feral man, we should follow Tredgold's concept of *isolation amentia* of Kaspar Hauser; or, in feral cases, in general, we should think with Rauber in terms of this idiocy as a *dementia ex separatione,* from isolation. As early as 1885 Rauber points out that the first years of human life are the most significant ones. Individuals subject during these years to isolation long and severe enough, recover from its effects to a degree only by an unusual strength and power of human plasticity.

Since natural or congenital idiocy does not appear involved in the instance of the wolf-children of Midnapore, as in some of the other instances, we are led to admit the relevance of these data of isolation to problems of human psychology and sociology.

Food and eating. Man, like other animals, eats; and both classes of feral man show a striking adaptability of the human organism to raw food both animal and vegetable, digested without preparation or refinement. All of the cases of children adopted by wild animals, wolves, and bears, were sustained by raw meat, and nothing else, save milk, would be accepted at first recovery. After rescue these children exhibited the greatest craving for raw meat and would steal offal, carrion, etc. Tomko of Zips, Hungry, is reported as eating offal, as one of the Midnapore cases is shown doing in an actual photograph. Some of the cases that General Sleeman reported ate carrion at the edge of the villages with the dogs.

The cases of children who have been lost in the wilds to survive show even more strikingly the adaptability of the human organism for food requirements. Survival on bark, roots, grass, herbs, and leaves are recorded. Thus man appears to be able to sustain life as either an herbivorous animal or a carnivorate. Several cases, if not all, are reported as suffering a severe illness soon after rescue. One cause of this, no doubt, is the change of diet. In the light of recent research in the effects of diet on the intelligence, there is but little doubt but that the inadequate diet of feral man has something to do with his mental retardation. There is, however, hardly enough evidence available to attribute it all to this cause.

In many of the cases reported, we have the animal behavior pattern of smelling food before eating. Such reports are those of the third Lithuanian Bear-boy *(11)*, as well as Sleeman's cases *(14)* and *(16)*. Other clear patterns of animal conditioning in eating are reported of Clemens of Overdyke *(23)* who, like the pigs he had lived with, went four-footed and rooted in the salad-beds until these were fenced against him. Another animal pattern of eating is seen in actual photograph of one of the Midnapore wolf-children drinking milk like a dog on all fours with the head down. This same thing is also reported for one of Sleeman's Indian wolf-children *(13)*. Dina Sanichar *(20)* the Sikandra wolf-child, sharpened his teeth on bones, like a dog or wolf. It is noted of this child that the keeping clean of the cup and plate used in eating was one of the few human attributes he recovered. About the only other human behavior-pattern he mastered, besides the erect position in walking and the use of clothes, was the smoking of tobacco, a minor human vice of which he appears to have been notably fond.

Dina Sanichar's keeping his plate and cup clean, may be significant of the importance and power of eating in the shaping of human conditioning and the channeling of man's emotions. As with animals, feral man, *e.g.* Kamala *(30)*, reveals that the first emotional attachments for human beings is to the person who feeds him.

Also the hunger motive is so impelling in human behavior that in a few of the cases of feral man we see only in his food habits a differentiation beyond that of the animals in the direction of the invention of a favorite method of subsistence which appears as the rudimentary beginnings of human culture. The second boy of Overdyke *(22)* had learned to climb trees for birds' eggs and young which he could never be taught not to do. He was very skilled in climbing trees, as was the Wild-boy of Aveyron *(26)* who specialized in climbing for nuts and wild fruits. This reminds us of the Girl of Songi *(7)* who is recorded as expert in swimming for fish and frogs which furnished her favorite food.

Clothing. Other than this evidence of the strength of the hunger motive for the cultural beginnings for the food quest, we apparently have no evidence of other motivations in feral man strong enough for the invention of tools, houses, or the use of fire. Since feral man is reported as insensitive to ordinary climatic changes of heat and cold, any evidence of the use of clothes among feral children, that have grown up wild, has been interpreted as evidence of human contact rather than invention.

Contact rather than invention is indicated in the case of the Girl of Songi *(7)*, who wandered back into civilization with a bottle-gourd on her head and with her body covered with rags and the skins of animals. Also the Cranenburg girl *(6)* is reported as having been captured wearing an apron of straw, as almost the sole relic of previous human contact. Human contact for both of these cases is also indicated by the considerable recovery reported for them. The Cranenburg girl, although not learning to talk, could soon pass for an ordinary person; while the Girl of Songi not only learned to speak, but regained so much of human personality as to become a nun, as we have already seen.

Less recovery is noted in the other two cases, those of boys, found wearing remnants of clothing as relics of human contact. Wild Peter *(4)*, though naked when caught, had around his neck the last strings of a shirt, while the tanned marks on his thighs were convincingly adduced by Blumenbach as evidence that he had worn trousers and thus that he could not have been away from human contact for more than a year. The Wild-boy of Aveyron *(26)* showed evidence of human contact at no distant time before his final re-capture in the remnants of clothing apparently given him at some previous time.

In contrast to these cases of children who have wandered off wild in the forests, children reared by animals, when first caught, tear off clothing as savagely as wolves themselves might do. This is detailed in full for the Midnapore cases *(29)* and *(30)*, from which we see their gradual conditioning to wear clothing, which ended in the pathetically human desire of Kamala to demand her "fok" whenever the children went walking, or visitors came to the Orphanage. Wolf-children savagely tearing off clothing at first and a long-continued disinclination to wear them is recorded for Sleeman's cases *(12)*, *(13)*, and *(17)*, as well as the case from Shahjehanjur, *(19)*, and Dina Sanichar of Sikandra, *(20)*. It is recorded that the first Lithuanian Bear-boy *(1)* was forced by blows to wear clothes, a harsh treatment also accorded to some of the wolf-children.

Besides such difficulties in getting wolf-children accustomed to clothes, we read that Wild Peter *(4)* threw away his clothes, especially his cap, with glee; and that only after three years in captivity did the Kronstadt Boy *(25)* become accustomed to clothes, especially shoes.

Shame. That the wearing of clothes is small indication of the human sense of shame, so lacking in animals, is clearly indicated by primitive custom familiar to anthropologists. Such data as we have from feral man regarding a sense of shame suggests an infantile naïveté, which develops into the human attitude through social conditioning. The best account of this, as in the other cases, is that of the Wolf-children of Midnapore and that of Kaspar Hauser, Feuerbach says of the latter and a new case:[37]

> When once my wife and myself undressed him, in order to cleanse his body, he gave full proof of his innocence and ignorance; his conduct, on that occasion, was precisely that of a child; quite natural and unembarrassed. Not long afterwards, however, a feeling of modesty was awakened in him; and he became as bashful as the most chaste and delicate maiden. An exposure of his person he now regards with horror. After the wild Brazilian girl, Isabella, whom Messrs. Spix and Martins had brought to München, had lived for some time among civilized people and worn clothes, it was not without much trouble, nor yet without threats or blows, that she could be brought to undress herself that her shape might be drawn by an artist.

The straw apron worn by the Cranenburg girl *(6)* on her capture is evidence of a human sense of shame which probably, like the apron itself, derived from previous human contacts.

Sex. Whatever may be the relationship of the human sense of shame to sexuality, the latter, unlike the former, is a trait man shares with the animals and one which is closely linked with his biological endowment. Feral cases yield unexpected evidence of inhibition of the sexual impulse apparently due to unfamiliarity in the new situation and impossibility in the old.

While there are no special data on this head for the Girl of Songi *(7)*, the fact that she became a nun might indicate that the sex impulse was not strongly developed in her. Tomko of Zips *(27)* is recorded as appearing

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[37] Paul J. Anselm, Ritter von Feuerbach. *Caspar Hauser*. Translated by H. G. Linberg, 1833, 42 f.

"to feel no temptation to lust, despite the fact that he shows adequate sexual development. Once when herding sheep on a pasture, a girl tried to seduce him. He told about it afterwards with much disgust and repugnance." Similarly the accounts of Kaspar Hauser's life in human society, almost exactly during the period of adolescence from the ages of 17 to 23 years, during which almost his every act was observed and recorded, are devoid of anything to indicate other than that sex was dormant.

There are hints in the diary on the Wolf-children of Midnapore that Kamala *(30)*, before her death at 17 years, showed diffuse and unpatterned sex expressions, which were also indicated for the Wild-boy of Aveyron *(26)*. The account of the Wild-boy of Kronstadt *(25)* traces more clearly the emergence of unpatterned sex expression, unchanneled through a complete lack of proper association:

> Probably it was his complete unfamiliarity with his new condition . . . (that) he showed not the slightest emotion at the sight of a woman. (Wagner's foot-note) In this respect is it surprizing that he showed no adjustment? For how could this wild boy of Kronstadt, like Wild Peter of Hameln, known anything about women? They saw in a woman nothing else than a human being dressed in clothes different from those of a man. This question of their interest in woman could only be solved if they had seen and been in communication with human females freed of their cultural accoutrements. The following proves this observation. When I saw him again after three years, this apathy and disrespect had disappeared. As soon as he saw a woman, he broke out into violent cries of joy, and tried to express his awakened desires through gestures.

Wild Peter, just mentioned, lived to the ripe age of 80 years without any such behavior being recorded for him; but this does not disprove Wagner's thesis.

The evidence of the Wold-boy of Kronstadt shows the emergence of the sex impulse at puberty, causing restless and unpatterned behavior, as in the Wild-boy of Aveyron, who lived so long that he sank back unexpressed into dormancy from which in Kasper Hauser and Wild Peter it appears never to have emerged. These data indicate that the social channeling of sex is much more necessary for its full expression than might be thought. Certainly this preponderating evidence of the inhibition of the sex impulse through isolation from social contact is surprizing, especially considering the other gross animal behavior of feral man in other urges, like hunger.

Sensation. Sensation is a fundamental psychic phenomenon, apparently closely linked with the biological endowment of man, since highly developed sensory organs go back in the scale of life as far as such molluscs as the octopus and the squids. The sensory perceptions of feral man show surprizing conditioning to their animal or other non-human experience.

Both classes of feral cases show striking ability to see, especially at night. The Wolf-children of Midnapore *(29)* and *(30)* were able to see surprizingly well at night. They prowled "like wolves" as did also the second Sikandra boy *(21)*, and Sleeman's second wolf-child *(13)*.

The conditioning of Kaspar Hauser, having been as long as he could remember in a dark dungeon, resulted in visual prowess that is as much more extraordinary as our account is more complete and detailed. On the darkest nights he could walk with certainty, and was amused by others groping for the stairs or gates.

The sense of smell of feral man, when reported, is described as sharp. One of the Wolf-children of Midnapore located by smell the fresh entrails thrown out with the garbage. Sharpness of smell in feral cases is indicated by the frequent report of smelling all food before eating it. This is recorded for the third Lithuanian Bear-boy *(11)*, the Irish Sheep-boy *(3)*, the two Sikandra Wolf-boy *(20)* and *(21)*, and Sleeman's similar cases numbered *(15)* and *(16)*. Of Jean of Liège *(8)* about all that is recorded of his behavior after his rescue from the wilds, other than his subsistance on wild vegetables, is that his sense of smell was extraordinarily sharp; and, even at a distance he could recognize his female guard by her odor. In time, according to account, he lost this sharpness of smell as he became accustomed to our common food.

The sense of hearing of none of the cases of feral man is reported as defective. Our better records, such as the diary of the Midnapore Wolf-children, show that feral cases have had an unusually sharp sense of hearing. Also the excellent account of the Wild-boy of Aveyron records that he responded to the sound of the cracking of a nut, to the eating of which he had been accustomed when wild. Sleeman's fifth wolf-child *(16)* "heard exceedingly well." It is thus not only quite clear that the mutism of feral man is not due to defective hearing, but also that feral cases have improved their hearing by animal conditioning—the only sensory improvement noted.

There are few data in the records relative to the sense of touch in feral man, but there is frequent note of a surprizing insensitivity to hot and cold. The Wolf-children of Midnapore agree with the records of the wolf-reared children, *(12)* and *(19)*, in their appearing insensitive to the annual shift in temperature in Northwest India, and refusing covers on the coolest of nights. The same is reported by Tulp (whose portrait we have from the brush of Rembrandt, as the demonstrator in the well-known painting, "The Anatomy Lesson") for the Irish Sheep-boy. The Wild-boy of Aveyron is recorded in Rauber as so insensitive to hot and cold as to have taken potatoes out of a pot of boiling water, soon after his capture. Anthropolgists are familiar enough with records of similar exposures of the hands in primitive ordeals.

The best account of the sensory perceptions we have is of Kaspar Hauser, and is one that deserves detailed attention from psychologists interested in the development of sensory perception. The inadequate accounts of the true cases of feral man give no data inconsistent with the following discussion by Feuerbach of his observations on Kaspar Hauser, penetrating for 1833:

> The most remarkable fact of experience in respect to him, which I learnt, but which was not fully explained to me until several years afterwards, was the result of the following experiment, which was suggested to me by a very obvious association of ideas, leading me to compare what was observable in Kaspar, who had not come forth from his dark dungeon to the light of day before the age of early manhood, with the well-known account, given by Cheselden, of a young man who had become blind but a few days after his birth, and who, in consequence of a successful operation, had been restored to sight at nearly the same age (as Kaspar Hauser).
>
> I directed Kaspar to look out of the window, pointing to the wide and extensive prospect of a beautiful landscape, that presented itself to us in all the glory of summer; and I asked him whether what he saw was not very beautiful. He obeyed; but he instantly drew back, with visible horror, exclaiming, "Ugly, ugly" and then pointing to the white wall of his chamber, he said, 'There are not ugly.' To my question, "Why was it ugly?", no other reply was made, but "Ugly, ugly." And thus nothing remained, for the present, for me to do, but to take care to preserve this circumstance in my memory, and to expect its explanation at the time when Kaspar should be better able to express what he meant to say. That his turning away from the prospect pointed at could not be sufficiently accounted for, by the painful impression made upon his optic nerve by the light, appeared to me to be evident. For his countenance at this time did not so express pain as horror and dismay. Besides he stood at some distance from the window, by the side of it, so that although he could see the prospect pointed at, yet, in looking at it, he could not be exposed to the impression made by the rays of light entering directly through the window.
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> When Kaspar, afterwards, in 1831, spent some weeks with me, at my own house, where I had continual opportunities of observing him accurately, and of completing and correcting the results of former observations, I took an opportunity of conversing with him respecting this occurrence. I asked him whether he remembered my visit to him at the tower; and whether he could particularly recollect the circumstance, that I had asked him how he liked the prospect from his window, and that he had repeatedly exclaimed, "Ugly, ugly" . . . I then asked him, why he had done so? and what had then appeared to him? To which he replied, "Yes, indeed, what I then saw was very ugly. For when I looked at the window it always appeared to me as if a window-shutter had been placed close before my eyes, upon which a wall-painter had spattered the contents of his different brushes, filled with white, blue, green, yellow, and red paint, all mixed together. *Single things, as I now see things, I could not at that time recognize and distinguish from each other.* This was shocking to look at; and besides, it made me feel anxious and uneasy; because it appeared to me as if my window had been closed up with this parti-colored shutter, in order to prevent me from looking out into the open air. That what I then saw were fields, hills, and houses; that many things which at that time appeared to me much larger, were, in fact, much smaller, while many other things that appeared smaller, were, in reality, larger than other things, is a fact of which I was afterwards convinced by the experience gained during my walks; at length I no longer saw any thing more of the shutter." To other questions, he replied, that in the

beginning he could not distinguish between what was really round or triangular, and what was only painted round or triangular. The men and horses represented on sheets of pictures appeared to him precisely as the men and horses that were carved in wood; the first as round as the latter, or these as flat as those. But he said, that, in the packing and unpacking of his things, he soon felt a difference; and that afterwards, it had seldom happened to him to mistake the one for the other.

Emotions. One thinks of certain of the emotions as closely linked with the biological inheritance of man. The inhibition of sexual expression has already been noted. The other data on the expression of emotion in feral man show the expression of violent emotions of anger and impatience, not only for the Wolf-children of Midnapore, but also for the half-wild Tomko of Zips, Hungary, the Wild-boys of Aveyron and of Kronstadt, and the idiotic Wolf-child, Dina Sanichar.

So human an expression of emotion as laughing, or even smiling, is not recorded for feral cases. The Rev. Singh well considers it near the crisis of the quest of Kamala for humanity, when one tear fell from her eyes in grief at the death of Amala, as herein recorded. As for any observable effect of music on the emotions of feral cases, this was great in the case of Kaspar Hauser, as is in keeping with his greater sensitivity. Wild Peter used to jump gaily in rhythm of music, whereas the Wild-boy of Kronstadt only listened to tunes on the piano with apparent pleasure. His slight interest in music did not, however, leave any impression of it with him, we are told. No other data are given.

Social aspects. The material of feral man deals a hard blow, as the Rev. Singh specifically points out, to the "social instincts" of gregariousness, or to what Giddings called the "consciousness of kind." The Wolf-children of Midnapore were noted to shun human society at first almost as completely as a wolf might have done. This is also recorded by Sleeman for his first and fourth wolf-children *(12)* and *(15)*. More than shunning human society, several cases of feral man, besides the Wolf-children of Midnapore, are reported as preferring the society of animals, the more closely related to the species of their prehuman associates, the better. Any animal, however, was preferable to humans with the Midnapore children. They followed the chickens; and Kamala was helped in walking by the strengthening of her legs in imitation of the climbing of a cat, when she wouldn't follow human instruction at all.

Other animals associations preferred to human society are reported of feral cases. Kaspar Hauser loved horses and played with hobby-horses hours at a time in utter unconsciousness of human presence. Clemens of Overdyke preferred pigs to human associates; and Sleeman's cases, *(12)* and *(17)*, loved dogs in lieu of their foster wolf parents. More extraordinary is the additional report in the last case, as well as in that of one of the Lithuanian Bear-boys, that the wild animals involved lost their fear of man in the presence of the feral children. Rev. Singh had the insight to keenly regret having to kill the wolf-mother of the children of Midnapore, observation of which would have been very interesting on this point.

Even greater sympathy than between feral children and wild or other animals is the strong bond of sympathy between pairs of wolf-children when they have occurred together as at the Midnapore and the Sikandra Orphanages. All other feral cases, like Kamala after the death of Amala, become attached at first only to the person who feeds them. A further impression of feral cases, as they counter-indicate an instinctive counsciousness of kind, is that they are completely selfish, though definite statements for this are lacking except for the Wild Boy of Aveyron. In the present record of Kamala we have an excellent, and the only detailed, record of the shift in the emotional ties of a feral case from animal to human associates. Thus Kamala came to like human society and missed Mrs. Singh greatly during a long absence.

Thus defective are the most elementary emotional responses of feral man in these most rudimentary human traits of socialization. Feral man has the sensory and other biological equipment that man shares with the animals. The biological foundation is there; but the human superstructure is vastly lacking, when the children are found. Perhaps half of the cases show a noticeable recovery which may be indicated by the number of words learned, varying from 30–50 in some cases to a much more considerable recovery shown by a more or less fluent mastery of one or even two languages, seen in the cases of Tomko of Zips and Kaspar Hauser. Of these recoveries, however, only Kaspar Hauser gained a real mastery of the essential patterns of human personality. Even about him, there is from all observers, a sense of tragedy that he had been so irreparably harmed by the "crime against a human soul" that had been committed against him. Certainly Kaspar Hauser and the cases of feral man have the greatest significance to students of human psychology.

For experimental control psychology may well study man as a set of variables while assuming or seeking the environment as a constant, leaving to sociology the variables of the social environment while it assumes the individual as a constant. These data of the 'controlled' experiments of feral man approximate such a set-up.

Man is born with a neutral organization unique in the biological realm, yet apparently the individual has to live with other human beings in order to enter into his human heritage. Radicals from the days of Rousseau have prejudiced the scientific case for feral man by the fervor of their espousal of these cases as proving that the environment completely molds the human mind, and mentality. They overlook that mentality is a bioneurological mechanism, and mind is the environmentally conditioned content organized by that mechanism. Though in the main part of this study we have seen well-attested cases of human beings reduced to wolf-conditioning, the radical thesis still needs a case of a wolf raised to human behavior and language.

Meanwhile we shall continue to envisage human heredity and the psychological factors as paramount, with the environing factors operating with them in a complex interplay. It would appear clear that, within the complex organism of human neurology, man's faculties and capacities arise to function, but they need the environmental stimulus as seeds need the good earth. Deprived of the earth the seeds die, and so human faculties atrophy when deprived of the environmental stimulus of language, thought, and human association. By animal association this human equipment can be conditioned to animal behavior, locomotion, food-habits, and emotional response. Deprived too long of human association, or animal-conditioned too strongly, the sensitive potentialities of human development are permanently inhibited and the traces of animals conditioning are never completely lost.

Sociology, which investigates man's society for its significant variables in the conditioning environment, has long been interested in the data of feral man as crucial. This material, while in no way invalidating the hereditary and bio-psychological factors, does attest most emphatically to the validity of the realm of social conditioning as a field for the investigation into some of the most significant problems of human behavior.

Sociology studies and delimits the social realm as a super-organic structure which provides the conditioning environment of the individual from the day of his birth. That human society is a structure or organization with the most profound of effects upon the individual could not be more definitely shown than by the data on the Wolf-children of Midnapore, and to a less degree by the other cases of feral man, here summarized.

Cultural anthropology has added a valuable concept to social science in its conception of culture. Just as society is the environing structure or mechanism, so culture is the corpus of most of these patterns of human behavior passed on through the social mechanism. It is precisely these patterns of behavior which feral man lacks.

This technical concept of culture is different from the ordinary use of the term in such phrases as "a person of culture" or "a cultured man."

In such usage, culture means simply having education, moderated conduct, and a marked set of manners which connotes a sophistication and a depth of background of a certain sort. Anthropologists have found it useful to widen this conception to include the results of all human association and background.

By culture, the anthropologists mean that enormous part of human thought, belief, and behavior that results from human contact and association with others. These feral cases show how much the individual owes to the stimulation of contacts with and ideas from others. Thus is indicated how important a thing culture is in this sense, as a repository of all that has gone before which has found human acceptation.

As culture is the super-organic repository of all that which has gone before to find acceptation in human conditioning; and, as sociology is the study of the social structure, environment, and interactions of men whereby this heritage is taken over, so social psychology is the synthesizing science between these social and cultural fields on the one hand, and the psychic sciences of man as an individual on the other.

Social psychology occupies this intervening field and effects a necessary methodological simplification by assuming man as the 'individual,' to be a biological mechanism. For social psychology this bio-psychic mechanism is assumed as a constant to be investigated by the psychologist.

From this assumption the social psychologist investigates man as the 'personality' in a technical sense referable to the *personae, i.e.* the masks or rôles that the 'individual' is conditioned to wear before his fellow-players in society. That posture, locomotion, sexual expression, and food habits are subject to such patterning, and language and culture the products of it is shown by the data on feral man, which clearly reveal the reality and significance of these *personae* patterns or rôles in which the 'individual' participates with his fellows from the time of his birth. In feral cases, such as those of the Wolf-children of Midnapore, association with wolves gave these two Indian girls the effective conditioning of wolves sufficiently strong as to have inhibited human behavior. Nine years of careful education in human society, in which most of these animal behavior patterns were put aside, were necessary before the elder surviving child had attained sufficient familiarity with human *personae* patterns to develop in her the essentials of human personality (the rudiments of language, the use of clothes, and the upright position). Unhandicapped by either animal conditioning or that of survival isolated in the wilds Kaspar Hauser, appearing in the world at 17 years of age as a *tabula rasa,* had neurological equipment sensitive enough to go far beyond the others to the attainment of a well-nigh complete familiarity with the *personae* patterns current in Bavaria of his own day.[38]

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[38] Contemporary cases like that of Kaspar Hauser, of isolation by cruel guardians, so similar to the isolation in feral cases, are under investigation by American scientists. Kingsley Davis' study of a Pennsylvania girl (Extreme isolation of a child, *Amer. J. Soc.*, 45, 1940, 554-565) does not reveal much recovery of human personality, but another case in Ohio, being studied by F. N. Maxfield of Ohio State University, is showing great improvement. It is worthy of note that Davis' conclusion, which I did not see during the preparation of this paper, also refers to *personae patterns*. He writes, "Anna's history, like others, seems to demonstrate the Cooley-Mead-Dewey-Faris theory of personality; namely, that human nature is determined by the child's communicative social contacts as much as by his organic equipment" (*op. cit.*, 565).

A STUDY OF THE WAY IN WHICH A VERDICT IS REACHED BY A JURY

By H. P. Weld and E. R. Danzig, Cornell University

Little is known about the manner in which the evidence in a court-trial is received, weighed, and accepted by a jury. Weld and Roff have made a study in which a large number of individuals listened to the report of the evidence in a criminal trial and, as individuals, reached a verdict concerning the relative guilt or innocence of the prisoner. Since the witnesses themselves were not heard in either direct or cross-examination, since there were neither opening nor summing up statements by the attorneys, and since the individual subjects had no opportunity of discussing the case with each other, no claim was made that the experiment showed what would happen in an actual trial.[1]

In the present study we were able to overcome these omissions, to some extent at least, by availing ourselves of a trial in a moot court, one of a series conducted by Professor Lyman Wilson of the Cornell Law School. The case was prepared by Professor Wilson who also acted as Judge.[2] The counsel for the plaintiff and the defendant were students in the Law School, and three juries were selected from students who were taking courses in psychology. All of the proceedings were recorded by a professional court stenographer. The trial was held in the Moot Court Room of the Cornell Law School on two successive Tuesday afternoons, April 9th and 16th, 1940. The evidential details of the case were presented by witnesses who had been selected and coached by the attorneys.

The entire trial was conducted in the manner of a real trial, with several minor exceptions. The most important of these were that the jurors were not examined and no oaths were administered. The excellent acting by the witnesses, the introduction in evidence of photographs and blueprints, and the earnestness of all participants in the trial gave an air of reality to the proceedings such as to simulate an actual suit at law.

The three juries consisted (A) of twelve men, another (B) of twelve women, and a third (C) of seventeen men and women.[3]

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* Accepted for publication June 13, 1940.

[1] H. P. Weld and M. F. Roff, A study in the formation of opinion based upon legal evidence, this JOURNAL, 51, 1938, 609-628.

[2] We are grateful to Professor Wilson for his hearty coöperation. It was his sympathetic interest and cordial support that made the experiment possible.

[3] This jury originally had 24 members divided into 2 juries of 12 each, but 7 members were unable to attend throughout the trial.

METHOD AND PROCEDURE

The trial in the courtroom was divided into 18 stages, which hereinafter will be indicated by roman numerals. Every member of the jury was given a data sheet on which the stages were printed in order and the juror was directed to record his judgment for the plaintiff or defendant in terms of a 9-point scale, after each stage had been presented to the court. This scale was as follows: *1,* conviction that the defendant is not liable for damages; *2,* strong belief that the defendant is not liable; *3,* fair belief that the defendant is not liable; *4,* slight belief that the defendant is not liable; *5,* doubt that the defendant is liable; *6,* slight belief that the defendant is liable; *7,* fair belief that the defendant is liable; *8,* strong belief that the defendant is liable; *9,* conviction that the defendant is liable.

After the judge had finished his instructions to the jury, the three juries retired to separate jury rooms under charge of a foreman who previously had been selected as one having prestige with his particular group. On reaching the jury room the foreman immediately instructed every juror to record on his data sheet his vote or judgment on the following statement: "I find by a preponderaance of the evidence that the defendant is liable." After the judgments were recorded the foreman polled the vote. The foreman had previously been instructed that if the poll was not unanimous he should make sure that the points at issue as expressed by the judge were clearly understood by every member of the jury, and then proceed to a discussion. When he thought it advisable he could take a second poll in the same manner as the first, and further polls as necessary. The jury was to be dismissed if unanimity was not reached at the end of thirty minutes.

The case. A paraphrase of the case comprising the opening and closing statements of the attorneys for both parties, the testimony, and the judge's charge to the jury, as divided into installments, is as follows:

1. *Opening statement of the plaintiff.* Twenty years ago the plaintiff and her husband were married. During their honeymoon they had spent a few days at the Bear Mountain Inn, a well known summer resort located near Hamburg, State of York Island. They then returned to the town of What Cheer, Iowa, where they have resided ever since. Last June on the twentieth anniversary of their marriage they decided to retrace the route of their honeymoon journey. In so doing they naturally arrived at the old Bear Mountain Inn. Upon parking their car, they walked into the lobby of the building, which was empty, and stepped up to the desk. A young man appeared behind the desk and offered to accommodate them. The plaintiff and her husband did not register, but the young man said that they could have rooms in about two hours. The plaintiff had with her some jewelry, a diamond bar-pin and a platinum bracelet, and four hundred dollars in cash. Not wishing to carry these valuables around with her, she asked the young man if there was a place where she could safely put them. He told her that he would put them in the safe, and thereupon, she handed the jewelry and money to him, and received a receipt for them. When they returned to the desk about 6 o'clock to claim the room they had reserved, the clerk, a different man, informed them that he knew nothing about either the room or the valuables. The manager appeared and disclaimed all responsibility when the plaintiff demanded that the loss be made good.

Attorney for the plaintiff stated further: that the Bear Mountain Inn had five years earlier been sold to the defendant; that the defendant claimed that they ran the place as the Bear Mountain Club, a private organization, and not as a hotel open to the public; that the defense rests upon this ground and upon the ground that the plaintiff had no right to come upon the Club's grounds and that hence the Club owed the plaintiff no duty of care.

Plaintiff will prove that the defendant continued to operate the Bear Mountain Club as a hotel and so is liable for the plaintiff's loss, on the theory of innkeeper's absolute liability; that even if defendant is not responsible to plaintiff on this theory, the defendant in fact held the place out to the plaintiff as a hotel and in so doing owed a duty of care which, by allowing a stranger to appear behind the desk of the club, it failed to meet.

II. *Opening statement of the defendant.* The facts as laid out by the plaintiff were substantially correct. The issues in this case are: (1) Is this a hotel or a club? The defendant will prove that it is not and it cannot be a hotel. (2) Did defendant hold itself out as a hotel? The defendant will show that this Club did all that was in its power or that could reasonably be expected of such a corporation to give the appearance of a club.

The defendant will also prove that the plaintiff, as a reasonable person, could tell this was a club and not a hotel; that the place had changed to such an extent that the plaintiff should have been put on her guard; or that the plaintiff was so contributorily negligent as to bar her recovery.

III. *Ella Meeker. First witness for the plaintiff (direct examination).* About 3 o'clock, June 4, 1939, as she and her husband approached the village of Hamburg in a car driven by Mr. Meeker, he asked a man the direction to Bear Mountain Inn. They received directions and proceeded slowly to the Inn. Upon arrival they noted that the general appearance of the place was much as it was when they were there 20 years before. After parking the car they entered the lobby of the Inn, which was empty. A young man came through a door, which was apparently unlocked, behind the desk and they told him they wanted a certain room on the third floor in the east wing, which they had formerly occupied twenty years previously. The young man said that the room was at present occupied but that the guest was expected to leave about 6 o'clock that evening and they could come back then and have the room. Witness asked if in the meantime she could deposit her valuables and the young man assented. She laid her brooch and bracelet and $400 in cash on the desk and the man gave her a receipt (which she identified and which was put in evidence). She gave Mr. Meeker the receipt and they both walked outside and wandered over the place, returning two hours later, at 6 o'clock. A different man was now behind the desk, and he claimed to know nothing of the room reservation or of the other clerk. He called the manager, who was surprised at Mr. Meeker's questions about the room and asked if the Meekers did not know that this was a club, not a hotel. Mrs. Meeker asked for her jewels, but the valuables could not be found, and the manager refused to make good the loss. Mrs. Meeker identified a blueprint plan as a correct representation of the lobby, and it was put in evidence.

IV. *Ella Meeker. First witness for the plaintiff (cross-examination).* She and her husband had stopped at hotels on the previous two nights, but on neither occasion had she checked her valuables, thinking it unnecessary as she kept them with her always. She did not remember the road to the Inn from Hamburg, but she remembered the appearance of the Inn, which she had not seen in 20 years. As they turned in toward the Inn, she saw a sign saying Bear Mountain Inn, on the left side of the road. She saw no other sign. She saw no throngs of people around the hotel. It did not strike her as unusual that there was nobody behind the desk when she came in. She did not remember whether anyone was behind the desks in the other two hotels at which they stopped. She did not remember the number of the room she occupied 20 years ago, but she recalled that it was in the east wing on the third floor because her husband had talked with her about it several times. She had read the receipt very carefully. The fact that the words, "Bear Mountain Club," were printed under the signature of the first clerk raised no suspicion in her mind. She noticed no signs over the desk. She did not register. Although there was some sort of golf tournament going on, she did not see many people. The clerk who took her valuables was young and presentable, whereas the clerk who was behind the desk when she returned was a different type of man, whom she could identify but did not now see in the courtroom. The first clerk did not say that this was a hotel nor did he say it was a club. She did not understand the term in her complaint, "domestic membership corporation," although she had made a sworn statement that everything in her complaint was true. She understood it at the time she signed it because her lawyer explained it to her then. Her eyes are good, but she saw no little sign under the big one at the entrance to the hotel.

V. *Philip Meeker. Second witness for the plaintiff (direct examination).* On June 4, 1939, he and his wife, on their way to "Bear Mountain Inn or Club," where they had been once before, 20 years ago, "asked a person on a street in Hamburg to direct them to Bear Mountain Inn or whatever it is." Following the directions, they reached the driveway, which had a couple of brick posts on either side, the one on the left bearing a sign reading, as he said, "Bear Mountain Club or whatever it was." They were going about five miles an hour at this point, and Meeker saw no other sign. The building looked much as it had 20 years ago. He parked in the parking lot to the right, and went inside with Mrs. Meeker. There was no one around the hotel, no doorman met him, and no one was in the lobby. That did not strike him as unusual, because it was warm, and nobody sits around hotel lobbies on warm afternoons. No one was at the desk, which was in the corner with a paper and cigar counter, but in a couple of seconds a man pushed open a door from the hall back of the desk. They asked him for a room on the third floor, east wing. Witness remembered the location because when he was there last the sun came in the window in the morning and woke him up. They did not sign the register, but he did not think it strange, as at other hotels they first assign a room to you and then have you sign when they give you the key. The clerk said the room was in use at the time, but would be vacated by 6 o'clock or thereabouts. They thought that would be all right. Witness had been talking with his wife about checking the valuables she had. She had not checked them at other hotels because she wanted to wear the brooch and bracelet, but she did not expect to need them here, as they did not think the Inn was very formal. Mr. Meeker suggested that she check them, so she asked the clerk if she could. He drew out a piece of paper and started to make out a receipt. He took the articles and counted the money, and made out the receipt as if that was his regular procedure. They then went out one of the side entrances, crossed the terrace and walked down along the bridle path. When they came back to the desk they found another person there. Meeker asked if the room was ready, as the other clerk had promised. This clerk seemed surprised, and said he was the only clerk around there. Then Meeker told him about checking the valuables, and he knew nothing about it. The manager came over and joined in the discussion, and a few people collected. As the Meekers were about to leave a man came up and introduced himself as Frank Fleharty.

VI. *Philip Meeker. Second witness for the plaintiff (cross-examination).* When he asked for directions, he asked about Bear Mountain Inn, not, as the cross-examiner said, "Bear Mountain Inn, or Club or whatever it is." The fact that the sign on the post said Bear Mountain Club did not arouse any suspicion, especially since he recognized the building. It did not occur to him that ownership might have changed in the 20 years since he was there. He left his baggage in the car because there was no doorman there. This fact did not appear strange because country inns do not have doormen. He did not carry his luggage in because usually the clerk acts as a jack-of-all-trades and comes out afterward to get it. The sign reading "Club," the lack of doorman, and the absence of a clerk did not cause him to think it was other than an inn. He did not think anything about it, and the clerk appeared almost immediately. It would have been possible for the first clerk to have inserted a key and unlocked the door leading to the desk without Meeker's knowledge. Witness does not know who checked his baggage when he came there 20 years ago. His wife had no valuables to check at that time. He does not always leave it to his wife to do the checking, but these were her valuables and Meeker was right there. Although his eyesight is good, he did not see any signs in the hotel. He had testified that "we asked for the things we had checked" because, although he had not checked anything, they were together, and he had the receipt. Mr. Meeker and his wife had no food or refreshments at the Inn. For sentimental reasons he wanted the same room he had had before.

VII. *Frank Fleharty. Third witness for the plaintiff (direct examination).* He is a bond salesman living in New York City. On June 4 he was at Bear Mountain on his vacation. He came into the Inn about 5 o'clock. He has stayed at Bear Mountain every summer for the last 10 years, sometimes three, sometimes four weeks. He is not a member of the club and he has not a guest card. He knows of several persons that are neither members nor guests who stay there. In the only case he knows of when a person lacking a membership or guest card was turned away, the people were of the colored race. He was familiar with the place when it went under the name of Bear Mountain Inn and he attributes the bankruptcy to the undesirable clientele which kept the better class of people from patronizing the place. In the village of Hamburg, with which he is quite familiar, the natives always refer to it as the Inn. He has never heard it called a club in town. There is a sign over the desk which has to do with restrictions to people with membership cards or members only. It is inconspicuous, hanging among trophies on the wall. He thought the photograph here entered in evidence was a fair representation of the entrance to the club grounds. (In the photograph there is a sign saying "Bear Mountain Club," on a brick pillar, and to one side, almost covered by bushes, is a small sign which says, "for members only."). He had never noticed the small sign.

VIII. *Frank Fleharty. Third witness for the plaintiff (cross-examination).* There is good fishing around the inn, which is why he has been coming back for 10 years. He had heard about the bankruptcy, but he expected no difficulty and went back in his old standing. He knew people who frequented the club but he had no membership list. He knew many town people. The guests are mostly from a distance, but some are from town. He knows people who come to the inn who do not belong to the club, and some who do belong. He does not know if they have membership cards; but he has a good friend who has been admitted although not a member. He does not think his friends in town helped him get in after the club was formed. He does not know whether anyone could stay at the inn without being a member of the club, but he knows he did and his friend did. He does not know whether his friend knew club members or not. The people who in direct examination he had said had been turned away, had been refused because of their race. He had said that the hotel went bankrupt because the clientele was becoming undesirable, yet he continued to go there because he wanted to fish. He was a member of this clientele. The townspeople think the inn is a hotel, they refer to it as an inn, and they think they could go there and get in at any time. He never noticed the little sign that is in the picture. He is not familiar with photography, but he would say that the photographer was standing about in the middle of the junction of the drive and the road when he took the picture. The big sign is on the front of the post facing the road. The driveway is made of gravel and goes off at about a ninety degree angle. He had never met the plaintiff before the day in question.

IX. *Richard Howard. First witness for the defendant (direct examination).* He has been employed by the club for almost five years; it has been a club throughout this period. He is clerk and general handy man at the club. He sells candy and cigars, arranges for golf and tennis lessons for members, and sees that the guests are taken care of. The club is for members and their guests and each of the guests is given a guest card. If a stranger should come with a member, he would ask the latter if the stranger was his guest. If the stranger was not the guest of the member, he would ask him to leave. If the stranger established himself as a guest through his guest card, he would remember him for a reasonable time after that. His other duties take him away from the desk frequently, so that he is around the grounds, is called out every 15 or 20 minutes. On June 4th at about 3:45 P.M., he noticed that the players in the golf tournament were getting on the 18th green so he went to see that the trophies were there. He was on and off the golf course all afternoon. The place was so crowded with spectators that he can not see how anyone could miss seeing that something unusual was going on. There was no reason why he should not leave the desk, as nothing of value is there. After he had been at the desk for an hour, around 6 o'clock Mr. and Mrs. Meeker came in and demanded a room that they said the other clerk had reserved, but he told them he knew nothing about it and that there was no other clerk. He asked to see their membership or guest card and they said they did not know one was needed. Witness explained that this was no longer an inn, and Mrs. Meeker said "all right" and asked for her valuables. She became incensed when he told her he knew nothing about them. He called the manager, who also explained that guest cards were required. A crowd gathered and finally the Meekers left. Mrs. Meeker said nothing about thinking this was a club. The club does not sell rooms. There are rooms for the convenience of members and they are charged a slight fee, but it is restricted to members and their guests only. Sometimes someone would come and spend the summer at the club. This has happened within the past five years. There has been no trouble at the club similar to the present situation. Witness never takes valuables from the guests to keep for them since they have lockers which are rented for that purpose. They are in the locker room, and witness has nothing to do with them. There is no place for him to keep valuables for the guests.

X. *Richard Howard. First witness for the defendant (cross-examination).* He has been working at the inn about five years, works from seven till eight, and finds the job healthy and not too confining. He works there only in the summer and he does not think he works too hard. He usually goes to the movies when he has extra time.

XI. *Thomas Wheeler. Second witness for the defendant (direct examination).* He is steward or manager of the Bear Mountain Athletic Club. When he was called to the desk and Howard explained the situation to him he told Mrs. Meeker that they were not liable because this was a club, and he mentioned the sign which said "Bear Mountain Club." She thought it said "Bear Mountain Inn." She was not certain whether she had given her purse to Howard or to another man. When Wheeler told her that he would search for the purse and mail it to her if it was located, she went away apparently dissatisfied. A diligent but unsuccessful search was made, and witness wrote Mrs. Meeker to that effect. Howard told him that he had not been at the desk. Howard's duties required him to be on the grounds at times. The club has a certain number of members who have cards, and both Howard and witness know all the members on sight so there is no trouble when members come. The members can bring guests who are given guest cards. It is possible but improbable that an outsider could get lodging there without a guest card. It might be, as happened with Mr. Hummel once, that a member would come in with two or three guests and witness would fail to supply them with guest cards. The picture which plaintiff offered in evidence is not a correct picture of the entrance as it looks now. Neither of the two signs shown is now present. There is instead a rustic sign made out of wood in an arch over the road between the two posts. The sign says "Bear Mountain Club" and underneath the sign says "For Members Only." The driveway is approximately 12 or 14 ft. wide between posts and the sign stretches across the whole distance and stands probably 2½ or 3 ft. high. The letters are 2½ to 3 ft. high. The picture shows the entrance as it was five years ago, when the road was graveled. A similar picture was taken in order to get the trustees to appropriate money for a new sign. The road was paved four years ago. Witness remembers because that was when he got a new car and got tar on it.

XII. *Thomas Wheeler. Second witness for the defendant (cross-examination).* The sign was put up because formerly the place had been an inn, and with the change to a club witness thought it necessary to put the sign up. There has been a sign stretching from post to post for the last four years. He tries to keep the grounds restricted to members and guests. The Bear Mountain Club sign is in an arch with letters 2 ft. high. The "For Members Only" sign is considerably smaller, directly beneath the larger sign. It did not occur to him to bring to court a photograph of the new sign to refute the photographic evidence of the old sign submitted by the plaintiff. He does not think this case will affect his job, though he has an interest in the club itself. The club has been involved in other law suits in the last five years. He was on the grounds near the golf course all afternoon but he did not walk around and look at the sign. He did not look at it the day before. He rooms at the club and goes out often. The sign is not lighted up, so that at any time it would be possible for someone to enter without seeing the sign. He could not say when he can remember reading is consciously.

XIII. *Samuel Hummel. Third witness for the defendant (direct examination).* His home is in Hamburg, and five years ago when Bear Mountain Inn failed he was one of three members of a committee, from a sort of athletic club, which purchased the inn at the sale. The club was incorporated and they made every effort to make people quit thinking it was an inn and to make them quit coming there. From the beginning of the club no meals were served to the public. If a group from Hamburg came up for an evening, they could get meals only if a member was with them or if they had guest-cards. The club is entitled "The Bear Mountain Athletic Club, Inc." on the certificate of incorporation of a membership corporation (the certificate was admitted in evidence). It is the duty of the clerk, Howard, to see that everyone who enjoys the club and equipment has a membership or guest card. Howard does his duty well. Members can bring guests if they want to, but they are supposed to sign for them. There are definite rules against the taking of members' property for safekeeping. There are lockers in the club which members rent, but it is definitely understood by all that the club assumes no responsibility for the contents. The club issues receipts for the payment of dues and of small service bills. (Hummel identified one of the blank forms for receipts.)

The committee thought it advisable to change the signs so that people would realize that it was no longer an inn. A rustic sign was erected across the archway reading "Bear Mountain Club" and a little below it was placed a smaller sign which read "Private Club, For Members Only." They put a sign over the desk which said, "The facilities of this club are open only to persons holding membership or guest cards." This sign could easily be seen from the position where Mr. Meeker testified he was standing, as it is about a foot square. A person driving up the driveway in the afternoon would have the sun behind his shoulder. There must have been a big crowd on June 4. The last hole of the golf course is not far from the clubhouse and he thinks over to the right.

Frank Fleharty is a good friend of Hummel and the witness has several times taken him to the club. Mr. Fleharty is able to go to the club by himself because he is an old friend of Hummel; he likes fishing and Hummel, knowing he was planning to come back after the inn became a club, told Howard and Wheeler to put him up and not ask any embarrassing questions or make him feel that he was not wanted. If he was not Hummel's guest or the guest of someone else in the club, they would stop him and ask for credentials and a card. If he did not have them they could not let him in.

XIV. *Samuel Hummel. Third witness for the defendant (cross-examination).* Witness heard Wheeler testify that the picture which is in evidence was taken by the club, but he does not believe it was taken by the club; it was taken by a member for the purpose of persuading the trustees to have the sign changed to its present form. That was between four and five years ago. The only other picture witness has of the entrance is an airplane view. Frank Fleharty was a good friend of his but he had to pay the regular service fees that a member's guest would pay, which is more than a member would pay. To the knowledge of witness, Fleharty should not have anything against the club. He cannot explain Fleharty's testimony that others, who were neither members nor guests, were allowed to go into the club, and that he knew of only one case where they were turned away. He thinks the statement erroneous, but Fleharty must have had some basis for it.[4]

XV. *Philip Meeker. Rebuttal.* Witness took the photograph of the club entrance which was introduced in evidence earlier in the trial. He took it about 6:15 P.M., June 4, as he was leaving the club. The manager had told him there was a sign which he should have seen, so he took particular notice on the way out and decided it would be a good idea to have a picture of it. He had the picture enlarged a short time after he returned from the trip—probably within the week. He saw the original print, which is the same as Exhibit C. The picture is of the left side of the entrance. There is another post on the right which is not shown in the picture. Witness did not take picture of the right hand post because he was only interested in the two signs. He did not notice any signs on the brick post at the right, but he did not look carefully because the manager had said the two signs were both on the left post. If there had been a rustic sign two feet high spread from gate post to gate post, he would have seen it, but there was no such sign.

Mr. Meeker is a jeweler, having been in the business about 20 years. About 12 years ago he gave a diamond bracelet and platinum brooch to his wife for Christmas. He got it wholesale and paid $600 for the brooch and $500 for the bracelet. Before he left home on this trip he gave his wife $450 and he watched her count out $400 to the clerk at the inn.

When he was first on the stand, he did not testify that he was a jeweler because he was not asked. The plaintiff in this case is the wife of the witness, and he wants her to win.

XVI. *Summing up by counsel for defendant.* A review of the plaintiff's story reveals a peculiar amount of coincidence. Mrs. Meeker, on her trip, stopped for two nights at hotels. On neither occasion did she check her valuables. She says that she slept with $1500 under her pillow. She and her husband had forgotten where the "club" was, so they asked a native of Hamburg who told them the way to "the Inn." Although they did not remember how to get to the place, they remembered the location of the room they had 20 years ago. The fact that the people of Hamburg still spoke of the Club as "the Inn" is of no significance, since they had been accustomed to speak of it in this manner for many years before the change in ownership took place. The Meekers, following directions, turned into the driveway, with the sun at their backs, and yet they did not see the two signs over the roadway. Remember, these were stretching from post to post and were new. When they parked the car, no bellboy came to help with luggage. Although a big golf tournament was in progress and the 18th hole was near the driveway, they saw no crowd. No doorman met them as they went into the Club. Passing through the lobby, they failed to see the sign over the desk. There was no clerk, or anyone else in the lobby. Yet the Meekers said they still had no suspicion that this was not a hotel. Though she had slept with her valuables under her pillow at the two previous stops, Mrs. Meeker immediately turned her money and jewelry over to this stranger who suddenly appeared behind the desk. What has become of this mysterious Mr. X, this clerk of whom no one except the Meekers has seen or heard? Note the receipt which he is supposed to have given the plaintiff in return for her valuables. On its back is the printed title, "Cornell Law Library." Mrs. Meeker said she read the receipt carefully, yet she failed to pay attention to the words written under the signature: "Bear Mountain Club." The Meekers went walking for two hours—past the ski jump, past the golf course, past the tennis courts; still they did not suspect that the place was changed.

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[4] We omit from our paraphrase of the stenographic report evidence which showed that the witness was a German, that thrice during the World War he had been arrested for sabotage, that the incorporators of the Bear Mountain Club were members of the German American Bund, etc. The judge in his instructions warned the jury that this testimony was to be wholly disregarded, that it had no bearing whatever on the issues of the case. This evidence was introduced for the purpose of measuring the effect of a political prejudice on the making of a verdict. Jury C, a control jury, was twice sent out of the court room, and heard neither the testimony nor the instructions of the judge. Every member of the other juries had previously shown, by means of a carefully prepared inventory, some degree of prejudice against Nazi organizations in America. The results were negative. Only one juror even mentioned this evidence in the jury room and he was at once reminded of the judge's instruction. Jury A found for the defendant, and every vote in Jury B against the defendant has been accounted for. There is no evidence that the prejudice has any effect whatever.

In order to establish liability, there must be a host-guest relationship. There was no such relationship here. The plaintiff did not even know if she would be able to get a room.

The Club was not negligent. It had a definite, functioning system to keep nonmembers and guests out. The testimony of Fleharty was rebutted; he got in because he was the guest of the trustee, Hummel. The clerk's absence from the desk was not negligence; he was attending to the duties of his job. The Club had put up signs, it had erected a ski jump, and it had built a golf course and tennis courts. It had done all that could reasonably be expected. Surely it was not necessary that the building be torn down in order to inform the public that it was no longer a hotel.

In regard to the value of the property which the plaintiff claims to have lost, the only indication is the Cornell Law Library slip signed by an unknown imposter who conveniently disappeared. Mr. Meeker did not think of his story of the value of the jewels until he had completed his testimony and the trial was practically over. To intelligent men and women it must be apparent that the plaintiff is attempting to perpetrate a hoax. The jury must find for the defendant.

XVII. *Summing up by counsel for plaintiff.* If you look at the history of the law pertinent to this case, you find that an innkeeper is considered a member of a common calling, and that he therefore has exceptional duties to the public. Innkeepers often resort to subterfuges of one sort or another in an attempt to escape these duties. But the law looks to the fundamental facts and not to the window dressing. What is the test of an innkeeper? Commonsense. You must disregard names and ask, "Is he doing an innkeeper's business?"

This place had all the appearances of an inn. There was nothing to indicate that it was not an inn. One who had been there before would find no outward changes sufficient to make him suspect that it was no longer an inn. On the contrary, the appearance was such that any member of the public might reasonably have thought this was what the plaintiff thought it was, a hotel or inn.

Is there any reason to doubt the plaintiff's credibility? It is evident that her story was straightforward and honest, and that her acts were the acts of any reasonable person. Is Fleharty dishonest? There we have the testimony of a completely disinterested party to the facts that this place is operated as an inn. We have the picture of the sign which shows that it was small, half-covered, and completely inconspicuous. By leaving the desk accessible to outsiders, and by not having some one on duty, the inn allowed agency to be established between itself and the man who took the plaintiff's valuables. The reason Mrs. Meeker kept her jewels with her on previous occasions was that those hotels were formal, whereas she knew the inn to have an air of informality and that her jewels would be unnecessary there.

Defendant has attempted to make much of the fact that the Meekers did not see the crowd while the golf match was going on. But note that there has been no testimony as to where the golf match was. Obviously the crowd was not on the 18th green at 4 o'clock when they arrived and still there at 6 o'clock when they came back in.

The jury should find for the plaintiff.

XVIII. *Judge's instructions to the jury.* The major question presented for the consideration of the jury is: "Did the defendant, Bear Mountain Club, by its conduct, reasonably lead the plaintiff to believe that it was offering the accommodations customarily provided by an inn or hotel?" Incidental to that is the question :"Did the plaintiff reasonably rely upon this representation?"

The burden is upon the plaintiff to establish the truth of her statements by a preponderance of the evidence.

An innkeeper has the duty to receive the valuables of the guest and to keep them safely. An establishment may operate as an inn and incur the liabilities of an inn even though under its corporate powers it was not authorized to engage in such occupation. You must decide whether the Bear Mountain Club was actually doing business as an inn, or whether it held itself out to be an inn in such a manner as reasonably to induce Mrs. Meeker to rely upon its having such an apparent character.

If you find that the Club was actually or ostensibly a hotel, you must decide whether the relation of host-and-guest was ever established. Did Mrs. Meeker come to the Club seeking the hospitality which hotels commonly offer, and did the Club begin to furnish such services? To decide this you must determine whether the Club should be estopped from denying the authority of the supposed clerk to receive the valuables. Did the Club permit this stranger to appear behind its desk and to seem to be its employee in such a way as reasonably to mislead a prospective guest?

"If then you find that the Bear Mountain Athletic Club was actually or ostensibly an inn, if you find that Mrs. Meeker became a guest, and if you find that the ostensible clerk acted with apparent authority, you will find for the plaintiff; otherwise you will find for the defendant."

RESULTS

In the court room. The results fall into two groups: those obtained in the courtroom, and those found in the jury room. The numerical results showing the distribution of the jurors' judgments in the court room on the 9-point scale for each of the installments are shown in Table I. A graph showing the mass tendencies of their judgments for each of the installments is shown in Fig. 1: We have taken the median value as representing the central tendency, and the quartile deviation on each side of the median as representing the variation. The heavy central line in the figure represents the course of the median value of the jurors' judgments; the width of the figure shows the quartile deviation above and below the median. A summary statement of the results shown in table and figure at each stage of the trial follows.

I. Plaintiff's opening statement resulted in a slight belief that the defendant was liable. Six of the 41 jurors had a strong belief that he was liable, 16 expressed doubt, and 6 felt some degree of belief that the defendant was not liable. Of the jurors, 19 had, however, some belief in the liability of the defendant.

TABLE I

THE DISTRIBUTION OF JUDGMENTS ON THE 9-POINT SCALE

Scale values	Stages of the trial																	
	I	II	III	IV	V	VI	VII	VIII	IX	X	XI	XII	XIII	XIV	XV	XVI	XVII	XVIII
9																	1	1
8	6	1	1		1	2	4	6	2	3				1	2		1	2
7	6	2	12	3	5	5	6	9	5	4	3	3	3	3	6	1	4	5
6	7	3	14	4	11	6	14	10	5	3	3	6		3	3		3	3
5	16	22	5	9	16	14	9	12	10	13	7	6	4	4	11	7	8	9
4	3	4	5	14	2	9	6	3	8	5	14	13	7	9	7	3	3	2
3	2	7	1	7	2	1	1		8	10	6	7	16	5	5	4	9	7
2		1	2	1	3	3	1	1	1	1	5	4	6	3	2	19	8	5
1	1	1	1	3	1	1			2	2	3	2	5	2		7	4	7
Median	5.9	5.3	6.4	4.6	5.7	5.4	6.2	6.4	5.1	5.1	4.4	4.5	3.5	4.5	5.3	2.7	3.9	4.7
Q_1	5.2	4.3	5.2	3.8	5.1	4.5	5.2	5.5	3.9	3.7	3.3	3.6	2.8	3.5	4.2	2.1	2.7	2.6
Q_2	7.2	5.7	7.2	5.6	6.6	6.4	6.9	7.5	6.3	5.9	5.3	5.7	4.5	5.8	6.6	4.2	5.8	6.2

II. Defendant's opening statement brought the median down toward doubt. Only 6 jurors had any belief in the liability of defendant, 22 expressed doubt; and 13 had some belief in his non-liability. Thus it appears that the opening statements of the attorneys left the jurors relatively open-minded.

III. The direct examination of the plaintiff brought the median to the highest point in the plaintiff's favor that was reached throughout the trial—a slight to fair belief in liability of defendant. The judgments of 26 jurors fell in this category, with one convinced of liability. Only 5 remained doubtful, with 9 jurors holding to some belief of non-liability.

IV. Defendant's cross-examination of the plaintiff was very effective, not only neutralizing the effect of the plaintiff's testimony but also

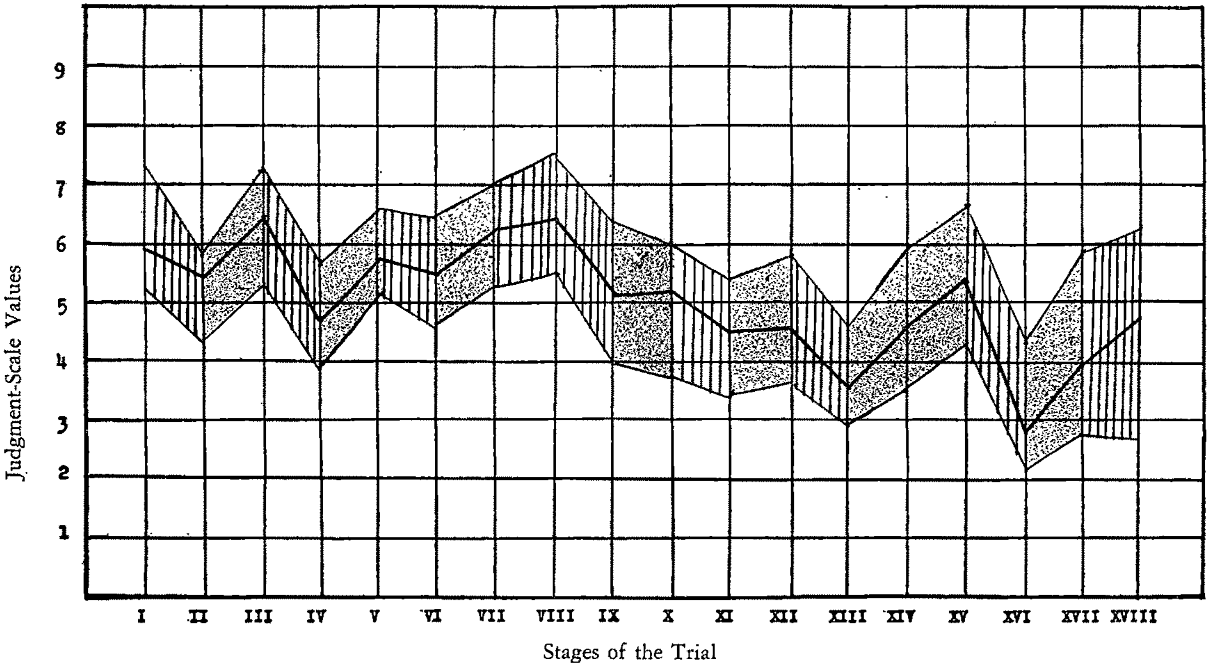

FIG. 1. THE COURSE OF THE JUDGMENTS AT EVERY STAGE OF THE TRIAL

The central black line represents the median value of all the judgments for each stage. The width of the figure represents the quartile deviation above and below the median. The barred portion shows the effect of the evidence for the defendent; the stippled portion that for the plaintiff.

bringing the median judgments down below the middle rating of doubt to slight belief in non-liability. Of the 41 jurors, 25 expressed doubt and 3 were convinced that the defendant was not liable.

V. Plaintiff's second witness brought the median back to almost the same position that was reached after plaintiff's opening—16 jurors were doubtful, and 17 had some degree of belief in the defendant's liability.

VI. Defendant's cross-examination of plaintiff's second witness had only a slight effect, moving downward in the direction of doubt.

VII. Direct examination of plaintiff's third witness moved the median above slight belief in liability, with 4 jurors holding a strong belief in liability, and 20 with a slight to fair belief in liability. Only 8 had any belief in non-liability.

VIII. Defendant's cross-examination of the plaintiff's third witness proved harmful to the defendant. The trend was toward liability, with only 4 jurors showing any degree of belief that the defendant was not liable. This is the second highest point in the plaintiff's favor, the median being only 0.01 below its position following plaintiff's first witness (Stage III).

IX. Defendant's first witness was effective, bringing the median down a whole step, to doubt.

X. Plaintiff's cross-examination of defendant's first witness had little effect, moving the median up only 0.04.

XI. Direct examination of the second witness for the defendant resulted in 28 ratings of some belief in non-liability. Only 6 at this point had some degree of belief in liability.

XII. Again cross-examination by plaintiff had little effect, moving the median up only 0.10.

XIII. Direct examination of the defendant's third witness brought a new low, the median going to slight to fair belief in the defendant's case. Three jurors had fair belief in liability, and 4 were doubtful. All the rest had some degree of belief that the defendant was not liable, with 5 convinced of defendant's non-liability and 6 with a strong belief in non-liability.

XIV. Cross-examination of the third witness moved the median up almost a complete step, thus restoring the position held just before the third witness took the stand.

XV. The rebuttal testimony of Mr. Meeker raised the median another step in plaintiff's favor, to a bit above doubt.

XVI. The summing up by the defendant's counsel was the most effective stage of the entire trial. The median was brought down to between fair and strong belief in non-liability, and only one juror expressed any belief whatsoever in liability. Seven were convinced of non-liability, and 19 had strong belief.

XVII. Plaintiff's summing up, though effective, did not restore the median to the position held at the conclusion of the testimony. It brought the median up to slight belief in non-liability.

XVIII. The judge's charge to the jury proved helpful to the plaintiff's case, raising the median almost a step but leaving it still in the defendant's favor. The ratings had greater scatter at this point than at any other stage. At this final stage only 11 jurors had any degree of belief in liability, 9 were doubtful, and 21 had some belief that the defendant is not liable. Of the latter, 7 were convinced and 5 had a strong belief in non-liability.

In the jury room. None of the juries reached a unanimous decision on the first poll, so that three more polls were taken. Only one juror out of the 41 changed his vote on subsequent polls. That juror, in Jury A, voted for the plaintiff on the first poll and for the defendant thereafter. Thus the final vote at the end of the 30-min. period was as follows: Jury A (men), 10 to 2 in favor of the defendant;[5] Jury B (women), 8 to 4 in favor of the defendant; Jury C (mixed), 11 to 6 in favor of the defendant.

In the jury room all votes, with four exceptions, were forecast by the final judgments in the court room. Of the exceptions three had expressed doubt in their final rating but on every poll in the jury room voted for the plaintiff. These three explained that despite their expression of doubt they had had a slight leaning toward the plaintiff. The other exception was the one juror who changed his vote as a result of the discussion. He explained that this was because he accepted the contention of the other jurors that the testimony of the Meekers was unsubstantiated by other proof.

It should be realized that the judgments in the courtroom were not based on "the preponderance of the evidence." Consequently, a juror who had indicated a judgment of only slight or fair belief in liability could still say there was a preponderance of evidence to establish the defendant's liability. Likewise, a juror who left the courtroom with a judgment of doubt might in the jury room contend that there was no preponderance of the evidence for the plaintiff. In point of fact, there were 5 jurors who voted for the defendant on this basis.

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[5] The vote of Jury A is in New York State sufficient for a decision in a civil case. See 463 A of the Code of Civil Procedure.

Individual differences. Thus far our study of the results has been limited solely to the trends of the mass judgments. The quartile deviation has, it is true, given some idea of the relative distribution and the tables have shown the actual distribution of the judgments. We have, however, not questioned how any individual member of our jury reached his particular judgment. The only way in which anything like complete knowledge of this question could be reached would be either by questioning every member or by asking him to write a commentary on his judgment at the time every judgment was made. Neither could have been done without interfering with the progress of the trial. We can, however, study the complete report of an individual and by judicious questions after the trial get some insight into the basis of his ratings and votes. We have done this in some instances. Whenever we found a report that differed markedly from the others it was put aside for further study. Our findings for some of these cases are informative.

Five of the jurors seldom, if ever, made a judgment below 5. Most of their judgments were either 7 or 8, thus showing a fair or strong belief that the defendant was liable for damages. These jurors formed a slight belief in liability after the opening statement of the plaintiff. For one juror this judgment was strengthened by the opening statement of the defendant because, as he said, "If the defendant should prove what he claimed he would prove he would still be liable." For this group of jurors, all subsequent evidence, the summing up, and the judge's instructions only slightly increased or decreased the certainty of their original judgment, and in the jury room all five voted consistently for the plaintiff.

Four jurors were with the opening statement of the plaintiff left with a slight to fair belief that the defendant was *not* liable; thereafter they never at any time felt that a preponderance of the evidence was in favor of the plaintiff. In the jury room they consistently voted for the defendant.

In one case a juror who began with a conviction of non-liability maintained her belief in the defendant's case until the summing up by the plaintiff, when she was convinced of liability. Her vote in the jury room was for the plaintiff.

Individuals often react differently to the same testimony. Thus, testimony which some considered helpful to the plaintiff was thought by others to strengthen the defendant's case, and still others thought it to be of no importance one way or the other. In the same manner, the reaction to the personality and behavior of some of the lawyers varied markedly with individual jurors. According to statements of the jurors, judgments were sometimes affected by admiration for or antagonism toward certain members of counsel.

All the jurors did not reach decisions through a logical analysis of the case; some could not explain or justify their conclusions. Thus several reported that they voted as they did because the party "just didn't have any business recovering in a case like that," or "it just didn't seem right to me." An examination of the reports of the jury foremen reveals, however, that the juries as a whole were well aware of the important issues in the case as set forth by the judge.

Sex differences. No outstanding differences were found between the men and the women jurors. When the medians of the men's judgments were plotted on the same graph with the medians of the women's judgments, the curves were found to coincide in most places and to be quite similar throughout. In the jury room, 13 out of 19 girls, and 16 out of 22 boys voted for the defendant; the ratio is almost exactly the same.

Discussion of results. We cannot assume of course, that other juries would, in all respects, give the same results as our juries did. We may, we think, dismiss the possibility that our jurors were influenced in some unusual way because the trial was a moot and not a real trial. It was held in a room designed and furnished like a conventional courtroom, and the proceedings were conducted with the decorum and seriousness of an actual trial. There are, however, two factors which might have distinguished the work of our juries from that of other juries. In the first place our jurors were probably superior in intelligence and education to the average juror. In the second place, the fact that the case was in part fabricated and the small number of witnesses may have simplified the task.

On the other hand, when we consider the psychological processes involved in reaching a verdict, certain of our results which agree with the results of other experiments, may be regarded as typical of the average juror. We have substantiated the finding of Weld and Roff that "the direction of the mass tendency as represented by the median is in general a function of the testimony,"[6] *i.e.* the judgments of the jurors tend to support the contention of the plaintiff or of the defendant as the testimony favors the one or the other side. The first witness for a side is usually the most effective; and although we found that subsequent witnesses were usually less we did not find with Weld and Roff that they tended to become progressively less effective. It is probable that our number of witnesses was too small to show this result, and it is possible that the cross examination interfered with the trend.

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[6] *Op. cit.* 622.

Our results show, however, that the oscillation of the judgments during the course of the trial is influenced not only by the testimony but also both by the opening and closing statements of counsel and by the personality of the witnesses or of members of counsel. As instance of the former, the opening statement of the plaintiff set up a belief in the liability of the defendant exactly as the testimony of a witness for the plaintiff would have done. Similarly when the defendant summed up his case he did more for it than any one of his witnesses had done; and he did it in part by laying less stress upon his proof and more upon the fact that the Meekers had no proof that they had in fact deposited valuables at the desk, thus casting suspicion on the honesty of the Meekers—a bit of strategy for which the plaintiff apparently had no defense.

As regards the influence of personalities, from the first some jurors were convinced of the sincerity and honesty of the Meekers, whereas others mistrusted them; the men were slightly more influenced by Mrs. Meeker's testimony than were the women jurors; the attorney for the plaintiff who made the opening statement was by voice and manner more persuasive than was his colleague who did the summing up.

We also found, as Weld and Roff found that early in the trial many jurors reached a fairly definite decision, and that thereafter the effect of the testimony was merely to change their certainty. This happened with at least 25% of our jurors, half of whom were disposed in the one, and half in the other way. Even those who as the trial progressed developed an attitude of doubt were still inclined to favor the one side or the other and in the jury room did so. We found no juror who attempted to maintain an attitude of doubt on the theory that he should make no decision until he had heard all the evidence, but we can substantiate the earlier findings of the cautious juror, and of the juror who is easily swayed by the testimony. In addition we discovered jurors who were influenced in reaching their verdict less by the proof than by their notion of what under the circumstances seemed to them to be "right" or just.

The fact that our results show no significant sex differences need occasion no surprise. They agree with Burtt[7] as against Münsterberg that women are no more tenacious than men in maintaining their opinions during discussion. Our results from the jury room do not agree with those of several experiments made on the formation of opinion by discussion. As we have shown, only one of our 41 jurors changed his vote in the jury room whereas in most if not all experiments on group opinion changes in opinion resulting from discussion are frequent.[8] The reason for our result is probably due to the fact that our jurors had already reached a decision when they came to the jury room and they were prepared to defend their opinion. Our results also give positive evidence in support of those advocates who question the value of cross-examination except for special reasons.[9]

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[7] H. E. Burtt, Sex-differences in the effect of discussion, *J. Exper. Psychol.*, 3, 1920, 390-395; *Legal Psychology*, 1931, 159 f.; H. Münsterberg, *Psychology of Social Sanity*, 1914, 181-202.

Finally we agree with Weld and Roff as regards the general nature of the thinking process in reaching a trial-verdict. In addition we have discovered the existence of personal standards of evaluation which the juror brings to the trial (p. 533), and we also have evidence that if the prestige of a member of counsel is sufficient, some members of a jury may accept his opinion and maintain it as their own. In no other way can we explain the effect on some jurors of the summing up by the defendant counsel.

SUMMARY

(1) The judgments rose and fell with the evidence.

(2) The opening and closing statements of the attorneys were important.

(3) The effect of cross-examination was variable. In some instances it weakened or even nullified, and at one point (Stage VIII) it strengthened, the effect of the testimony given in the direct examination.

(4) No significant sex differences were revealed.

(5) Some opinions were formed on the basis of the opening statements. In some cases a decision was reached early in the trial and all subsequent evidence was interpreted in the light of that decision. In others, decisions were not reached until the end of the trial. The weight given to any particular testimony was influenced by all the preceding judgments, and particularly, by the immediately preceding judgment.

(6) The juror reached a decision before going into the jury room and the arguments of his fellow jurors who did not agree with him were not effective in changing his decision.

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[8] For references see Weld and Roff, *op. cit.*, 610.

[9] See J. H. Wigmore, *Science of Judicial Proof*, 1937, 279 ff.

THE APPARENT SIZE OF THE MOON AS A FUNCTION OF THE ANGLE OF REGARD: FURTHER EXPERIMENTS

By ALFRED H. HOLWAY and EDWIN G. BORING, Harvard University

In a previous paper we have shown that the apparent size of the moon diminishes as the angle of regard of the head and eyes is increased above the primary position, if the primary position be here loosely defined as what would be horizontal regard for an erect *O*.[1] We have shown further that the functions relating apparent size to the angle of regard hold for the supine *O* as well as the erect, so that the supine *O*, with head toward the moon, sees the horizon moon as small and the moon in culmination as large. We worked for the most part with direct observation of the full moon, and found the ratios of the greatest to the least linear size to vary from 1.25 to 2.00.

The present paper deals with the apparent size of mirrored images of the full moon (and the sun). Such images are quite as satisfactory for observation and for the production of the illusion as is direct observation, provided the right conditions obtain.[2]

In the present research we controlled the position of the head by the use of an adjustable biting-board. The need of such control seems obvious enough now that we are sure that the illusion depends upon the movement of the eyes, but it was not obvious at first.

We have used two methods of measurement. (1) The first, described in our earlier paper,[3] consisted in equating the size of a circle of light, projected by a lantern on a screen 3.5 m. away, to the size of the full moon. In our previous experiments we placed the projection screen at one side of *O*, so that, erect or supine, he had to turn his head through 90° to see the comparison stimulus. The criticism was made,[4] however, that this turning of the head might vitiate the comparison. In the present experiments we placed the screen directly in front of *O*, omitting observations in the supine position, which loses its importance once one is convinced that the orientation of the body with respect to the earth is no longer to be considered relevant to the illusion. (2) We also introduced two simultaneous images of the moon (or sun) for direct comparison, having *O* estimate the ratio of the diameter of one to the diameter of the other.

The use of mirrors speeds up observation because with them it is possible to get the moon at any elevation at any time during the evening. On the other hand, there

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[1] A. H. Holway and E. G. Boring, The moon illusion and the angle of regard, this JOURNAL, 53, 1940, 109-116.

[2] *Op. cit.*, 114 f.

[3] *Op. cit.*, 110 f.

[4] At the Princeton meeting of the Society of Experimental Psychologists, 1939.

is always the delay of waiting for good weather at the full moon. There are only about three consecutive nights when the perceived moon is near enough to a circular disc to be matched to the circular comparison stimulus. One has, on that account, to wait for a full moon on a perfectly clear and not very windy night. There must be no clouds across the face of the moon nor must there be haze in the sky, for in such cases the mirrors become visible and reduce or abolish the illusion. On a hazy night one sees the moon, set in a golden misty square, corresponding to the shape of the mirror, upon the black background of the unillumined sky. On a windy night the mirrors may shake too much. It would be much more convenient, therefore, to use the sun, since a 'full' sun is to be had oftener than a full moon, especially during normal working hours. We got the illusion by viewing two mirrored images of the sun simultaneously through dark filters, but the method proved less satisfactory than we had hoped.

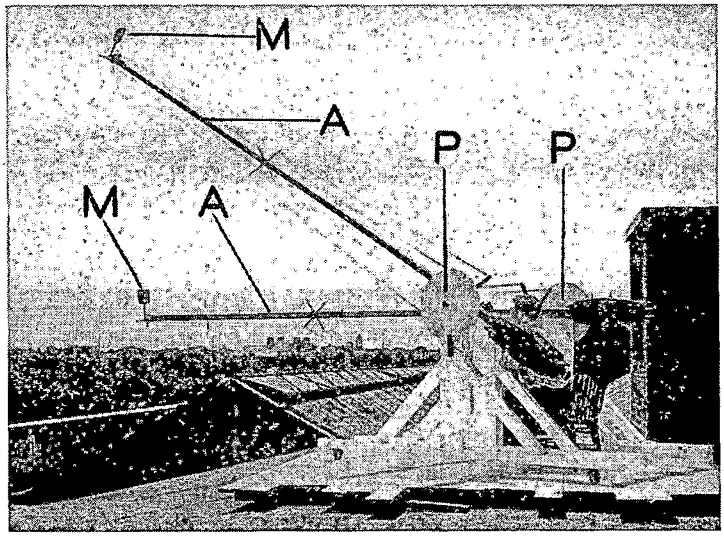

FIG. 1. APPARATUS
MM = mirrors, adjustable about two axes of rotation. AA = mirror-arms, each 5 m. from fulcrum to mirror. PP = protractors for measuring the elevation of the arms.

APPARATUS

Figs. 1 and 2 show two views of the apparatus. The long arms, 5 m. from fulcrum to mirror, were made of two sections of corrugated iron conductor pipe, brazed together and stiffened by steel guy wires. We also had short arms, 2.5 m. long. The mirrors at the ends of the arms were supported by two worm wheels. One wheel rotated the mirror about the axis of the arm, the other about the axis of the mirror support, which was at right angles to the axis of the arm. The worms were attached to spools about which fish-line was twisted, so that the worms could be turned by pulling on the lines when the mirrors were aloft. The mirrors were front-aluminized, 6 in. square. It was always possible to get the image of the moon simultaneously for both eyes within the square of the mirror, provided the moon itself were in the quadrant of the heavens behind the observer. Since the apparatus was moveable, it could be shifted as the moon southed, but it would have been more convenient to have had larger mirrors, so that the worms would not have had to be constantly readjusted as the moon's image moved off the mirror for one eye or the other.

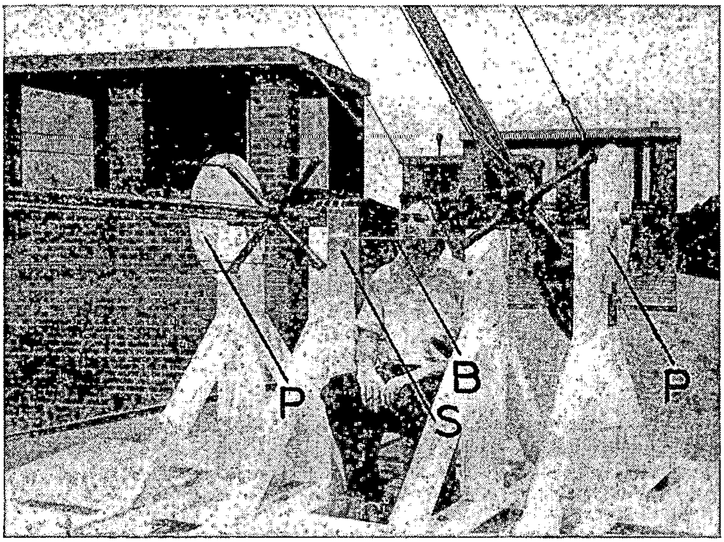

FIG. 2. APPARATUS
PP = protractors for measuring elevation of the arms. B = biting-board, pivoted at the ends, for controlling angle of head. S = stop-plate for biting-board, arranged so that board can be locked at any 15°-angle, or can be freely moved back and forth through an angle which is a multiple of 15°.

The biting-board was fixed to the apparatus between the axles of the two arms and a few inches below them. It consisted in a long plate of cadmium-plated brass, pivoted at each end so that it could be rotated into different planes. A set of stops was arranged to lock the biting-board in the horizontal position, the vertical position, or any 15°-interval between. The stops could also be arranged to allow the board to rotate freely through some predetermined angle that is a multiple of 15°. The erect *O* sat on a chair and bit the biting-board. The supine *O* lay on a table adjusted so that he too could bite the biting-board.

Other details of procedure are given above or in the previous paper there cited, or below in connection with the discussion of the results.

EYE-MOVEMENT VS. HEAD-MOVEMENT

The eyes can be elevated above the primary position between 40° and 45°. Upward vision through spectacles may be limited to 30° above the horizontal. If the moon is viewed at 60°-elevation, the neck must bend back at least 20°. To view the actual zenith an erect *O* must raise his eyes 45° and bend his neck 45°, or else, if he finds this impossible, he must also bend his body. Similarly the supine *O*, viewing the horizon moon beyond his head, must bend his neck and body 50° to see the moon with eyes raised 40°, altogether a very uncomfortable posture, which one *O* in our last experiment was unable to maintain without nausea.

It has been suggested that both the elevation of the eyes and the backward tilting of the head, being positive in character, act to shrink the apparent size of the moon,[5] since both occur in the free viewing of the illusion. They should, therefore, be separately controlled, and we find by such an analysis that the illusion is dependent entirely upon the eye-movement and not at all upon the head-movements.

O, erect, fixed his head by biting firmly upon the biting-board locked in the horizontal position (Fig. 2). Only one arm of the apparatus was used. *O* observed the mirrored image of the moon at 0° (horizon), 5°, 10°, . . . 30° or 35° elevation. He matched the moon for size by a projected comparison stimulus on a screen 3.5 m. in front of him. The screen was placed so that the horizon moon and the comparison appeared side by side. The results appear in Table I and Fig. 3, a smooth function for each of the two *O*s, with the maximal illusion measured by the ratios 1.8 and 2.1. These ratios in themselves, being in each case slightly larger than the largest ratio found for each of these *O*s in the previous experiment, indicate that the entire illusion can be derived from eye-movement alone.

We also arranged the biting-board so that it would rotate freely between stops at 0° (horizontal) and 30° elevation. *O* first matched the moon at 0° to a comparison stimulus, also at 0°. Then he viewed a moon at 30° by bending his head back 30° so that the biting-board was pressed against the 30°-stop, and matched this moon to the comparison stimulus with his head at 0°, bending his neck back and forth through 30° until he was satisfied with the match. Since the angle of elevation of the moon was taken up entirely by movement of the head, the eyes viewed both the moon and the comparison stimulus from the primary position. The result is shown in Table I and Fig. 3. There was no illusion.

It is now plain why the observed points in the previous experiment did not always fall consistently on smooth curves.[6] The scatter was undoubtedly due to the uncontrolled movements of the head. By keeping the head rigid we can get smooth, consistent functions, like the curves of Fig. 3.

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[5] E. Schur, Mondtäuschung und Sehgrosskonstanz, *Psychol. Forsch.*, 7, 1925, 44-80, esp. 75 f.

[6] Holway and Boring, *op. cit.*, 110, Fig. 1.

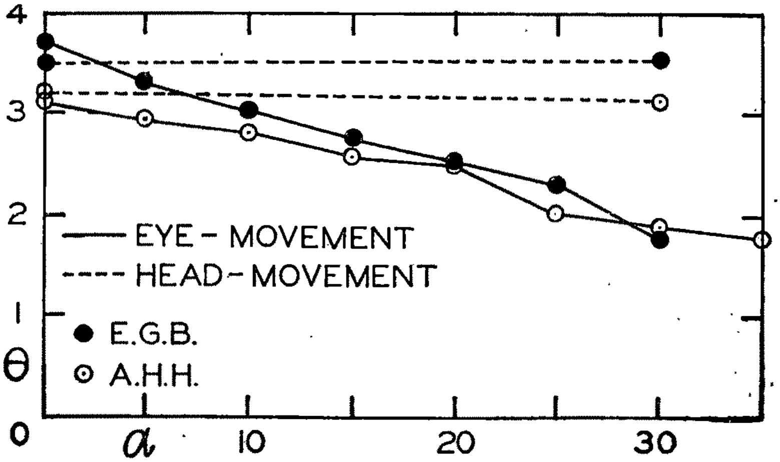

FIG. 3. APPARENT SIZE OF MIRRORED MOON AS A FUNCTION OF EYE-MOVEMENT AND HEAD-MOVEMENT

α = degrees of elevation of eyes above horizontal for erect O. θ = apparent size of moon in degrees subtended. These are the data of Table I, q.v.

TABLE I

APPARENT SIZE OF THE MIRRORED MOON AS A FUNCTION OF EYE-MOVEMENT AND OF HEAD-MOVEMENT

α = angle (degrees) of elevation of the mirrored image of the moon from the horizontal. θ = visual angle (degrees) subtended by comparison stimulus at 3.5 m., equated in size to moon's image. σ = standard deviation of the distribution of θ. Erect posture. For eye-movement: head is fixed by fixed horizontal biting-board. For head-movement: biting-board moves through same angle as moon's image. Each θ is the average of 5 observations. Long 5-m. mirror-arms. Max. ratio = ratio of θs for maximal and minimal αs. September 1, 1939. See Fig. 3.

	E.G.B.				A.H.H.			
α	Eye-movement		Head-movement		Eye-movement		Head-movement	
	θ	σ	θ	σ	θ	σ	θ	σ
0	3.72	.08	3.50	.08	3.16	.31	3.21	.22
5	3.31	.12			2.95	0		
10	3.02	.17			2.81	.06		
15	2.77	.12			2.59	.10		
20	2.56	.08			2.46	0		
25	2.29	.18			2.01	.11		
30	1.77	.12	3.57	.12	1.90	.09	3.15	.19
35	Too high				1.79	.14		
Max. ratio	2.12		0.98		1.77		1.02	

It is a corollary of this finding that the entire illusion must occur within 40° of elevation. The moon at 60°, as in the last experiment, is invisible to head fixed to a horizontal biting-board. In fact, the form of the curves in that experiment indicate that the eyes moved for the ascending moon and erect *O* up to about 25° elevation, for in that region the apparent size decreases steadily, as in the curves of the present Fig. 3. After 25° the head-movements would seem to have come in so that the change in the illusion was less. Above 40° almost all the adjustment must have been due to head-movement. The supine *O*s (especially A.H.H. in 1936 and L.M.H. in 1938) seem to have relaxed the neck for the first 30° and then to have begun to lower the eyes.

DIRECT COMPARISON OF TWO MOONS

It is a common experience that 'absolute' judgments are neither so positive nor so accurate as relative judgments, which reveal differences that had not been suspected. Two colors that look alike when seen at different times in different places may contrast shockingly in juxtaposition. So it seemed that the moon illusion might be enhanced by placing two moons simultaneously in the sky at different elevations for direct comparison. We had the necessary apparatus: two arms and two mirrors.

At once, however, a difficulty arises because neither moon can be varied in size to match the other; yet the most accurate measurement depends ultimately upon the judgment of equality. This is a common dilemma. When it is necessary to compare objects of different size, when neither can be altered to match the other and there is no variable comparison stimulus available, the best that can be done is to estimate the size of one object in terms of the other. We had our *O*s (with heads immobilized by the biting-board) judge the diameter of one moon in terms of the other, and we left them free to choose whether they would judge the larger in terms of the smaller, or conversely. The *O*s found these comparisons difficult. Mostly they used the ratios 1.0, 1.1, 1.2, 1.3, . . . 1.8, occasionally intermediate values like 1.15, 1.25. . . .

The results of this method appear in Table II, which shows the diminishing apparent size of the moon in elevation from the primary position (horizontal) to +30°, all for the erect *O*. The maximal ratios for the illusion are, for the three *O*s, 1.6, 1.5, 1.2. For the supine *O* with the primary position vertical it is possible to investigate both the upper (0 to +30°) and the lower (0 to —30°) quadrants. It will be seen from the columns for the supine position that the apparent size of the moon diminishes, not only as the moon 'ascends' from the primary position, but also as it 'descends' from the primary position. *Apparent size is maximal when regard is from the primary position.* So far as we know, this is the first time that the lower quadrant (the moon below the horizon) has been investigated. The table shows that the amount of the illusion (maximal ratios) in the lower quadrant is of the same order as the amount in the upper quadrant.

This method of direct comparison of two moons is much less satisfactory than the matching of one moon with a variable comparison stimulus, because it is so much harder to estimate a ratio than to make a judgment of equality. The *O*s were uncertain of their estimates, and the results check (cf. the double values for +30 and for −30 in Table II) only approximately. The method is useful, however, for preliminary investigation, and for observing the illusion with the sun, when the use of filters would render a comparison stimulus invisible.

TABLE II

RATIOS OF APPARENT SIZES OF MIRRORED MOONS AT DIFFERENT ELEVATIONS

Observed ratios of apparent diameters of pairs of mirrored images of the moon at different elevations. α = angle (degrees) of eyes above (+) or below (−) the primary position. 0 = primary position = horizontal for erect *O*, vertical for supine *O*. All ratios are relative to size at primary position = 1.00. The ratios without parentheses at +30 and −30 are products of the ratios for 0:15 and 15:30, whereas the ratios within parentheses are direct check observations 0:30. The two ratios should be the same. Long 5-m. mirror-arms. The "Max. ratios" are the reciprocals of the least ratios above them, separately for the upper and lower quadrants. July 1, 1939.

α	E.G.B.		A.H.H.		L.M.H.	
	Erect	Supine	Erect	Supine	Erect	Supine
+30	(.62)	(.72)	(.67)	(.67)	(.83)	(.74)
+30	.62	.64	.69	.69	.83	.70
+15	.80	.83	.83	.83	.91	.91
0	1.00	1.00	1.00	1.00	1.00	1.00
−15		.91		.87		.83
−30		.76		.76		.64
−30		(.77)		(.83)		(1.00)
Max. ratio upper quad.	1.61	1.56	1.49	1.49	1.20	1.43
Max. ratio: lower quad.		1.32		1.32		1.56

ILLUSION ABOVE AND BELOW THE HORIZON

To investigate the illusion below the primary position we first employed the estimation of ratios with an erect *O*. For this purpose we were obliged to use the short 2.5 m. mirror-arms, for the long arms could not have been depressed very far below the horizontal without being stopped by the roof on which the apparatus rested. Even then we could not get the mirrors lower than 25° below horizontal. The short arms are not so good as the long. With them there is a greater chance of the illusion's breaking down, for it is not so easy to make the mirror and its support invisible as it is when the mirror is at a greater distance. An alternative to making the mirror completely invisible is for the *O* to assume the perceptual attitude of looking through the mirror to a far-off distant object. The fact that the three principal *O*s in this experiment had considerable practice in assuming this attitude may account for the fact that the illusion, as finally measured, is not less than had been found with the long arms.

TABLE III

RATIOS OF APPARENT SIZES OF MIRRORED MOONS AT DIFFERENT ELEVATIONS

This table gives data for both upper and lower quadrants for the erect O only. Short 2.5-m. mirror-arms were used. Otherwise it is like Table II. See the legend of Table II for explanation of the entries. October 28–29, 1939.

α	E.G.B.		A.H.H.		L.M.H.	
	Oct. 28	Oct. 29	Oct. 28	Oct. 29	Oct. 28	Oct. 29
+30	.68	.58	.39	.47	.43	.61
+20	.80	.72	.50	.57	.54	.72
+10	1.00	.80	.71	.74	1.00	.90
0	1.00	1.00	1.00	1.00	1.00	1.00
−10	.80	.75	.91	.83	.70	1.11
−20	.76	.60	.70	.69	.49	.88
−25	.66		.63		.49	
Max. ratio: upper quad.	1.47	1.72	2.57	2.13	2.33	1.64
Max. ratio: lower quad.	1.51	1.54	1.59	1.41	2.04	1.43

The data of the estimated ratios are given in Table III, in which the figures have the same significance as the data of Table II. The diameter of the moon in the primary position is taken as unity. The extreme ratios are cumulated, that is to say, they are related to size at the primary position by being the products of three successive ratios. The functions show no inversions, for there is in all six instances continuous diminution on both sides of the maximum, which in all cases but one occurs in the primary position. In general, these functions seem to us to be conclusive as to the fact of diminution of the apparent size of the moon both above and below the horizon, but not reliable as indicating the form of the function or the exact amount of the maximal illusion.

For more precise results we, therefore, returned to the method of matching the moon to a comparison stimulus, still of necessity using the short mirror-arm on the apparatus. These results are shown in Table IV and Fig. 4. The trends are fully significant. Successive differences average about three times their standard deviations. Every maximal difference is

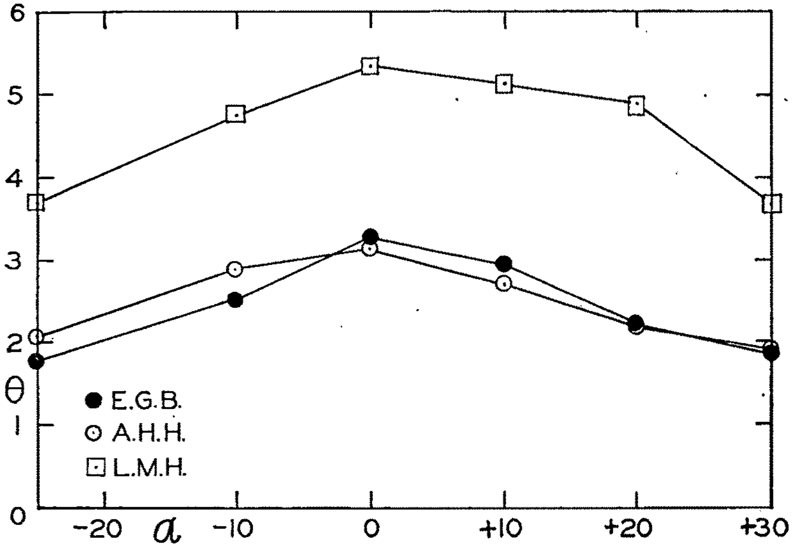

FIG. 4. APPARENT SIZE OF THE MIRRORED MOON ABOVE AND BELOW THE PRIMARY POSITION

α = degrees of elevation or depression of regard above (+) or below (—) the primary position, which is horizontal, being for the erect *O*. θ = apparent size of moon in degrees subtended. These are the data of Table IV, q.v.

TABLE IV

APPARENT SIZE OF THE MIRRORED MOON ABOVE AND BELOW THE HORIZON

α = angle (degrees) of mirrored image of moon from the primary position (horizontal). θ = visual angle (degrees) subtended by comparison stimulus at 3.5 m., equated in size to moon's image. σ = standard deviation of the distribution of θ. Erect posture, with head fixed by horizontal biting-board. Short 2.5-m. mirror-arms. Each θ is the average of 5 observations. Max. ratio = ratio of θ for 0° to θs for highest and lowest positions. October 29, 1939. See Fig. 4.

α	E.G.B.		A.H.H.		L.M.H.	
	θ	σ	θ	σ	θ	σ
+30	1.85	.10	1.88	.20	3.66	.21
+20	2.21	.11	2.18	.04	4.88	.22
+10	2.95	.25	2.71	.05	5.16	.16
0	3.26	.25	3.13	.13	5.34	.30
−10	2.51	.11	2.88	.10	4.77	.15
−25	1.77	.14	2.06	.17	3.70	.12
Max. ratio: upper quad.	1.77		1.67		1.46	
Max. ratio: lower quad.	1.84		1.52		1.44	

about five times its standard deviation. There are no inversions. The illusion always appears for a 10°-difference and always in the direction that makes the moon appear larger when nearer the primary position.

The moon, apparently so large upon the horizon to the direct regard of the erect *O*, always appears small—astonishingly small—to the *O* who, leaning over and inverting his head, regards the moon between his legs. This casual observation, familiar to most psychologists who have been interested in the moon illusion, is, of course, an instance of what happens 'below the horizon,' for the moon is seen from this posture with 'lowered' regard. It is our judgment, based upon casual observation, that the apparent size of the horizon moon is not reduced at all if the head is completely inverted, so that the regard is again in the primary position. Complete inversion of the head is not, however, accomplished casually; it requires aids or acrobatic skill.

Fig. 4 shows individual differences. L.M.H. matches the moon to larger comparison stimuli than do E.G.B. and A.H.H., whose matches are about alike. There has been this consistent difference among these three *O*s since they began working together in 1936. Always the matches of L.M.H. have been the largest, whereas A.H.H.'s matches have been equivalent to E.G.B.'s or slightly larger.[7] This difference is compulsory and unaffected by suggestion. Sometimes, L.M.H. would take the *O*s position and view a match already made by E.G.B. or A.H.H.; then he would exclaim with amused surprise at the great inequality that the others had judged equal. Conversely, L.M.H.'s matches seemed quite impossible to the other *O*s. This difference does not concern us here, because the moon illusion is a matter of relative, not absolute, apparent size for any given *O*. The individual difference is quite another psychological problem, for it pertains to the way in which apparent size varies with distance. For instance, if apparent size depends upon apparent distance, as some have thought, then L.M.H. sees the moon in every position about twice[8] as far away as do the other two *O*s.

APPARENT SIZE OF THE SUN AS A FUNCTION OF THE ANGLE OF REGARD

We undertook to work with the sun, partly because the sun is more often available than the full moon and partly because we thought that confirmation of the lunar function by a solar function would extend the generality of our findings. We used a heavy black cloth to throw over *O*'s head. In it was inserted a small wooden window-frame, into which could be slid a dense black filter (uniformly exposed photographic film). The window permitted binocular vision, being about 12 cm. long × 4 cm. high. The filters came inside the point of near vision, being about 3 cm. from the eye-ball; hence they were invisible as objects. The sun appeared through them as a bright disk on a dark field.

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[7] Cf. Holway and Boring, *op. cit.*, 110, 112.

[8] The average ratios of the apparent sizes in Table IV are L.M.H./E.G.B. = 1.93, L.M.H./A.H.H. = 1.88, and A.H.H./E.G.B. = 1.01.

TABLE V

RATIOS OF APPARENT SIZES OF MIRRORED SUNS AT DIFFERENT ELEVATIONS

This table gives data for upper and lower quadrants for the sun observed by an erect *O*. Short 2.5-m. mirror-arms were used. Otherwise it is like Table II for the moon. See the legend of Table II for explanation of the entries. August-September, 1939.

	Primary position horizontal					Primary position 30° above horizontal				
α	E.G.B.		A.H.H.		S.S.	A.B.	E.G.B.		A.H.H.	
	Sept. 12	Sept. 12	Sept. 12	Sept. 12	Sept. 14	Sept. 16	Aug. 24	Sept. 11	Aug. 24	Sept. 11
+35	—	—	.42	.57	.74	—	—	—	—	—
+30	.40	.41	—	.63	—	.57	.61	.60	.62	.63
+25	.57	.59	.50	.63	.87	—	—	—	—	—
+20	—	.84	—	.76	—	.72	.76	.86	.69	.76
+15	.72	.85	.65	.75	.97	—	—	—	—	—
+10	—	1.05	—	.91	—	.90	.90	.95	.83	.91
+ 5	1.03	.95	.91	.97	1.00	—	—	—	—	—
0	—	1.00	—	1.00	1.00	1.00	1.00	1.00	1.00	1.00
− 5	.97	1.05	1.09	1.02	1.00	—	—	—	—	—
−10	—	1.00	—	.87	—	1.11	1.03	.85	.98	.95
−15	.87	.74	.91	.93	.95	—	—	—	—	—
−20	—	.80	—	.79	—	1.23	.82	.76	.89	.76
−25	.74	.74	.83	.77	.85					
−30	—	—	—	—	—	—	.78	.65	.85	.69
Max. ratio: upper quad.	2.50	2.44	2.38	1.76	1.35	1.76	1.64	1.67	1.61	1.59
Max. ratio: lower quad.	1.35	1.35	1.20	1.30	1.18	.81	1.28	1.54	1.18	1.45

This method has, however, two drawbacks. In the first place, it necessitates the estimates of ratios, since there is no simple way in which a variable comparison stimulus can be arranged to be visible to the shielded eyes. In the second place, the judgments proved to be more difficult than for the moon. We were inclined to blame this difficulty upon the filters, believing that, although they were invisible in the sense that their microstructure could not be brought into focus by the eye, they nevertheless seemed to fix the distance as too little, in the way that an ordinary visible mirror destroys the illusion for the horizon moon. The experienced *O*s remarked that with this method the sun looked too small.

The results appear in Table V, which is comparable to Tables II and III for the moon. *The apparent size of the sun, like the apparent size of the moon, is maximal for regard in the primary position, and diminishes when the eyes are raised or lowered.* There are almost no inversions (cf. E.G.B., Sept. 12). The "max. ratios" are about what we found for the moon. There is some indication that the maximal size tends to occur a little below 0°, a displacement that might account for the tendency of the maximal ratios for the upper quadrant to be greater than those for the lower quadrant. The data do not, however, permit of such precise interpretation.

The table shows the functions for two new *O*s, S.S. and A.B. The function for one of them confirms the general rule, but the other function contradicts it. A.B. estimated the sun's image as larger below the primary position than in the primary position. We checked this difference in several observations and on two different days. It is a fact that can not, of course, be explained so long as there is no theory to explain illusion in general. Both S.S. and A.B. found the observations difficult and unsatisfactory.

The last four columns of Table V confirm our previous finding that the function moves with the head. *O* was tilted back 30° in his chair, and the biting-board was tilted up 30°, so that the primary position was moved to 30° above the horizontal. The data show that the maximum size occurs near the primary position and not near horizontal, which is now at —30°.

The fact that the illusion can be got at all with dark filters which show only the sun's disk on a dark field is one more piece of evidence that the phenomenon does not primarily depend upon the perception of objects on the earth's surface.

Fig. 5 summarizes the chief fact of this investigation. It shows the relative projected sizes of the moon and of the equivalent comparison stimulus for the horizon moon and for the moon at the 30°-elevation. These diagrams presumably represent the spatial patterns on the retina, since the perceptual determinants here under consideration lie almost certainly in the central nervous system. The relations of Fig. 5 approximate the findings for E.G.B. and A.H.H.: moon's diameter = 1, equivalent comparison diameter for moon on the horizon = 7, equivalent comparison diameter for moon at 30°-elevation = 4, height of moon above the horizon at 30°-elevation = 32. (The comparison stimuli for L.M.H. were considerably larger.) The figure makes it clear that two 'illusions' are being measured. (1) The illusion of size due to difference in distance ("size constancy"!) is the ratio of *C* to *M* in *A*, 7:1. (2) The illusion of size due to elevation of the eyes ("moon illusion") is the ratio of *C* in *A* to *C* in *B,* 7:4. In *B* the two principles work against each other, so that the ratio 4:1 in *B* is really the product of 7:1 (the 'size-distance' phenomenon) by 4:7 (the 'elevation-distance' phenomenon).

THEORY

The moon illusion is not an *external phenomenon* for its determinants lie within the perceiving organism. The astronomer's plates do not show the illusion.

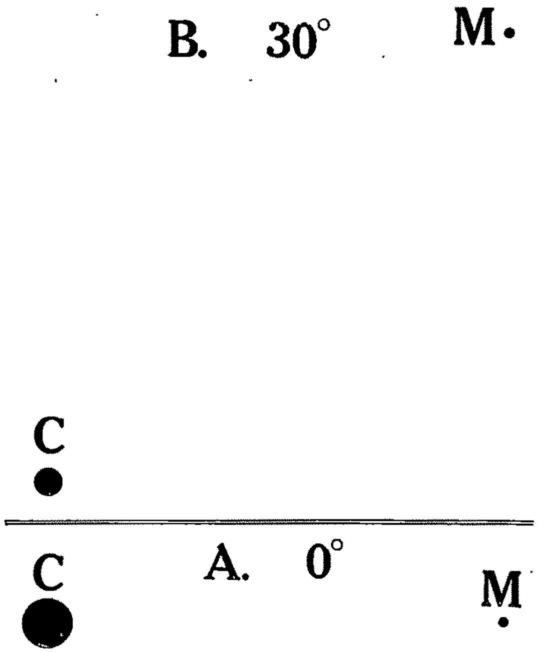

FIG. 5. PROJECTION DIAGRAMS OF THE MOON ILLUSION

M = moon. C = comparison stimulus, projected on screen 3.5 m. from *O* and equated in apparent size to M. A shows relative sizes of M and C, presumably as projected on the retina, with *O* erect and with M on the horizon. B is like A, except that M is elevated 30° above the horizon. The amount of the illusion is shown by the relative sizes of C in A and in B. These relations represent the data for E.G.B. and A.H.H.; for L.M.H. the Cs would be even larger.

The *object-contrast theory* is the one most commonly held: the moon, it is said, looks large at the horizon because it contrasts with visible terrestial objects. If there were small objects on the earth and large objects in the heavens, there would be meaning to this explanation; yet there are no objects at all in the heavens except the stars which are smaller than most important terrestial objects seen on the horizon.

The case against both the external physical theory and the contrast theory is as follows. (a) The horizon moon, seen by an erect *O* is large, but (b) seen between the legs with partially inverted head, it is small, although terrestial objects remain visible. This change can not be due to the objects' being seen upside down, because (c), if the head is completely inverted by some contortionistic aid, the moon again becomes as large as for the erect head, although the objects are still seen upside down. Another argument is that the large horizon moon (d) becomes small if viewed in an ordinary visible mirror which is near the *O*, but (e) becomes large again if the mirror is made invisible, even though the visible mirror reflects terrestial objects as well as the moon. It is true, moreover, that (f) the horizon sun or moon may appear large through a mist that obscures the sight of objects, and that (g) in our experiments the illusion occurs for the sun's disk seen through a dark filter which obscures the sight of all other objects.

Another classical explanation is the *apparent-distance theory*. It is supposed that the arch of the heavens appears ellipsoidal so that the zenith seems nearer than the horizon, and the moon, subtending a constant angle but appearing near in elevation, seems smaller. In so far as this theory depends upon the belief that apparent distance to the horizon is enhanced because it is filled with visible terrestial objects, the argument of the preceding paragraph tells against the theory. The relation of the theory to movements of the eyes is discussed below.

There remain four modern theories which require consideration. The convergence theory and the torsion theory are both dependent on binocular vision and both relate apparent size to apparent distance. The illumination theory and the inhibition theory apply as readily to monocular as to binocular vision. No final decision amongst them is possible at the present time.

(1) Convergence theory. Since the raised eyes tend to diverge (exophoria) as the result of muscular imbalance, the innervation for convergence is necessary if the binocular impressions of seen objects are to fuse and diplopia is to be avoided. For the lowered eyes there is increased convergence (esophoria) with diplopia avoided by innervation for lessened convergence.[9] Zoth's theory of the moon illusion is that this compensatory convergence for the raised eyes causes the moon to be seen as nearer and

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[9] F. B. Hofmann, Die Lehre vom Raumsinn, 1920, in *Handbuch der gesamte Augenheilkunde*, 2 ed., III, 1925, chap. 13. See sect. 4 on eye-movements, 259-351, esp. 279-289, 312-320; also Zoth, *loc. cit. infra.*

thus, since it still subtends the same visual angle, as smaller.[10] It is a well-established fact that convergence of the eyes makes near objects appear nearer when their angular size remains unaltered.[11] The theory encounters three difficulties. (a) The moon illusion is seen by monocular persons, for whom compensatory convergence to avoid diplopia seems improbable. (b) Observers all agree that the small moon in elevation looks further away than the large moon on the horizon. (c) Our observations contradict the theory, because esophoria below the primary position should make the moon appear larger still, whereas we find its apparent size diminished in the lower quadrant.

(2) Torsion theory. When the eyes are raised, the right eye tends to rotate clockwise (as seen from behind) with respect to the left eye. Such torsion of the eyes means that the fields of vision, as seen by the subject himself, also rotate, the field of the right eye counterclockwise with respect to the field of the left eye. The resultant angular displacement is called excyclotorsion, and a little consideration will show that, according to the elementary principles of stereoscopy, it would tend to bring the top of the field of vision near to *O* as compared with the bottom of the field.[12] This is the condition that the moon illusion is said to require, that the elevated moon should seem nearer to *O* without loss of angular size. For lowered regard the relations are reversed. There is incyclotorsion which makes the bottom of the field of vision seem nearer than the top, and thus the moon below the horizon should appear smaller than on the horizon. This theory, which is due to Adelbert Ames of the Dartmouth Eye Institute, has, therefore, an advantage over the convergence theory in that it requires the reversal of the direction of change in apparent size at the primary position.

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[10] O. Zoth, Ueber den Einfluss der Blickrichtung auf die scheinbare Grösse der Gestirne und die scheinbare Form des Himmelgewölbes, *Arch. f. d. ges. Physiol.*, 78, 1899, 363-401, esp. 393-400.

[11] H. Meyer, Ueber die Schätzung der Grösse und der Entfernung der Gesichtsobjekte aus der Konvergenz der Augenaxen, *Ann. d. Phys. u. Chem.*, 85, 1852, 198-207, showed that the convergence of the eyes to fuse two adjacent identical wall-paper patterns makes the pattern appear nearer and smaller. Any repetitive pattern can be used, like a cane chair-seat, a typewriter key-board, postage stamps; see E. C. Sanford, *A Course in Experimental Psychology*, 1898, 273; E. B. Titchener, *Experimental Psychology*, I, ii, 1901, 262; R. S. Woodworth, *Experimental Psychology*, 1938, 676. A recent study is H. Frank, Ueber den Einfluss inadäquater Konvergenz und Akkommodation auf die Sehgrösse, *Psychol. Forsch.*, 13, 1930, 135-144.

[12] See E. E. Maddox, *Tests and Studies of Ocular Muscles*, 1898, 392-395, (3 ed., 1907, which we have not seen). Maddox describes the Meissner test in which, by a reversal of this principle, the cyclotorsion of the eyes is measured by noting how much a fine line has to be tilted from the actual vertical in order to appear vertical. See also Hofmann, *op. cit.*, 276, and esp. L. Howe, *The Muscles of the Eye.* 1907, I, 348-365.

It is known that these size-distance effects from cyclotorsion occur only when they are free of other spatial cues. Perspective motives ordinarily overcome them completely. In the same way the moon illusion occurs only at considerable distances where perspective and other cues to distance are lacking. The consistency between these two sets of facts, therefore, increases the plausibility of the torsion theory. On the other hand, the monocular illusion makes trouble for this theory. The *Raumlage* for the raised or lowered eyes might persist for a time after one eye is removed from function, but it seems hardly likely that a differentiation, that was originally dependent upon binocular disparity, could persist indefinitely in a person who has permanently lost the sight of one eye. Against this theory too is the fact that the moon in elevation does not look nearer than the horizon moon.

(3) Illumination theory. We have shown elsewhere that apparent size is diminished when retinal illumination is diminished.[13] If raising or lowering the eyes reduces the retinal illumination, we might find ourselves in possession of an explanation of the illusion—an explanation which might apply as well to monocular as binocular vision.

It is true that, when the eyes are elevated to an extreme position (ca. 40°), there is a diminution of illumination which may be as small as one-twentieth of the illumination in the primary position. The pupil, as we found by observation through a long-focus microscope, has in this position only 40% of its area left uncovered by the lid. A Macbeth illuminometer indicated that the shading by the lids, lashes, and brows reduced the illumination of the remaining part to 30% of its original value. Stiles and Crawford[14] have shown that light entering the periphery of the pupil is less effective than light at the center, "perhaps due to the angle at which light strikes the retina." The reduction here would be to 40%. Now 0.40 × 0.30 × 0.40 = about 0.05. With artificial moons at approximately 100 and 200 ft. and with a reduction of illumination to one-twentieth, we found apparent size on the average reduced to a diameter about 90% of the original diameter.[15] This is too small a reduction in size to account for the moon illusion, in which the apparent diameter may be reduced to 90% by raising or lowering the eyes only 10° or to 50% by raising them

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[13] Holway and Boring, The dependence of apparent visual size upon illumination, this JOURNAL, 53, 1940, 587-589.

[14] W. S. Stiles and B. H. Crawford, The luminous efficiency of rays entering the eye pupil at different points, *Proc. Roy. Soc.*, B, 112, 1933, 428-450.

[15] Holway and Boring, *loc. cit.* (The dependence of apparent visual size upon illumination).

30°. A more exact investigation than ours, however, is needed to evaluate the effect of intensity.

(4) Inhibition theory. Schur has suggested that the effort of raising the eyes or head may be the essential condition for the loss in size of the perceived moon.[16] It is clear that head movements are irrelevant, but it remains possible that raised or lowered eyes may cause the illusion simply because such eye-movement requires more effort than lateral motion. This theory has the advantage over the convergence and torsion theories in that it is applicable to monocular vision, but it is somewhat vague in that it does not suggest how such a dynamic relation between muscular effort and perceptual size could be mediated by the nervous system.

At the moment the monocular theories seem to us more promising than the binocular. What we should like to find is some means whereby eye-movement above and below the primary position diminishes the total excitation of the visual perceptual field.

SUMMARY

(1) The moon illusion does not depend to any great extent upon physical conditions outside the body of *O*.

(2) It does not depend upon the posture of the body or head.

(3) It does depend in its full amount upon the position of the eyes within the head.

(4) The apparent size of the moon is maximal when the eyes are in or near the primary position.

(5) The apparent size diminishes continuously as the eyes move above or below the primary position.

(6) The full illusion is, therefore, realized when the eyes are maximally raised or lowered (about 40°) above or below the primary position.

(7) These generalizations are based upon the observations of mirrored moons compared with each other or matched to an artificial moon, and of mirrored suns compared with each other through a filter.

(8) The measurement of the apparent size of the moon is a function both of perceived size as dependent upon distance and of perceived size as dependent upon the elevation of the eyes (cf. Fig. 5).

(9) The convergence theory of the illusion (Zoth) is contradicted by the discovery that the apparent size of the moon is diminished in the lower quadrant, by the fact that monocular persons have the illusion, and by the fact that the small moon in elevation does seem to be further away than the large moon on the horizon.

(10) The torsion theory (Ames) accounts for the illusion in both upper and lower quadrants, but not for the monocular illusion, nor for the fact that the small moon in elevation appears to be relatively distant.

(11) The illumination theory (ours) does not show why the illusion should be as great as it is.

(12) The inhibition theory (Schur) remains uncontradicted, but needs the support of some specific neurological hypothesis as to how such inhibition might occur.

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[16] Schur, *loc. cit.*

REACTION-TIME DURING DISTRACTION AS AN INDICATOR OF ATTENTION-VALUE

By J. P. GUILFORD, University of Southern California,
and EDWIN EWART, Purdue University

Studies of attention-value of stimuli, particularly in the field of advertising, have used as criteria principally: (1) Subjective estimates by rank order; (2) memory tests, including recognition- and recall-tests; and (3) the observation of eye movements and fixations. Many approaches to the measurement of attention have, on the other hand, depended upon reaction-time, and variations of the reaction-time method as criteria of the degree of attention at the time of the reaction. The present study represents an attempt to apply reaction-time in a new way as a criterion of the attention-value of color in advertisements.[1]

An excellent review of work done on color in advertising, beginning with the work of Gale in 1900,[2] can be found in Poffenberger's *Psychology in Advertising*.[3] The more recent work is that of Nixon.[4] Using his "method of visual fixation," Nixon concluded that "it seems probable . . . that colored advertisements attract attention somewhat strongly for a very brief period, after which color as an attention and interest factor ceases to exert any marked influence." He reports a "very, very slight and unreliable tendency" for the colored advertisements to fail to hold attention after a few seconds exposure.[5] In regard to memory tests, however, he states that "the chances are that the colored (advertisements) will excell in recall." The differences, it should be added, were not particularly reliable.[6]

Investigations into the relation of degree of attention to reaction-time considerably antedated the applied psychological investigations in advertising. Obersteiner in 1879 held that "retardation of the reaction stands in inverse proportion to the intensity of attention."[7] Reaction-time was

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* Accepted for publication June 7, 1940.

[1] From the Psychological Laboratory of the University of Nebraska. This study is based upon part of a master's thesis by the junior author, a copy of which is filed in the University of Nebraska Library.

[2] H. Gale, *Psychological Studies from the University of Minnesota*, 1900, 55 ff.

[3] A. T. Poffenberger, *Psychology in Advertising*, 1925, 260-271.

[4] H. K. Nixon, Attention and interest in advertising, *Arch. Psychol.*, 9, 1925, (no. 72), 54.

[5] *Ibid.*, 53.

[6] *Idem.*

[7] H. Obersteiner, Experimental researches in attention, *Brain*, 1, 1879, 439-453.

used as a criterion with varying success, by Wundt,[8] Swift,[9] Cattell,[10] Geissler,[11] and Dallenbach.[12] Woodrow, in a very thorough study of the relation of reaction-time to attention stated that the evidence that reaction-time does vary with degree of attention is "so overwhelming that it is unnecessary to argue the matter."[13] Having expressed this view, however, he proceeded to develop and to establish his "detraction-time" technique, which he demonstrated to rest upon the principle that "the absolute detraction effect of a given detractor of attention varies inversely with the degree of attention upon which the detractor acts."[14]

PROBLEM

In some preliminary experiments, we found that with our stimulus material neither the absolute simple reaction-time nor Woodrow's detraction-time were adequate measures of attention-value. We consequently developed the new procedure which represents a reversal of the visual-attention reaction-time technique. Instead of measuring the attention-value of an advertisement by reaction-time to it in the presence of distractors, we proposed to let the advertisement itself be the distractor, and to measure the attention-value of the distractor by the amount of increase in reaction-time to a constant stimulus. The constant stimulus was a buzzer, sounding at an intensity that was not far above the stimulus-limen. The special problems to which we directed ourselves are: (1) Will a complex stimulus, such as an advertisement, be sufficiently distracting as to increase the reaction-time to a faint sound stimulus? (2) Will such an increase in reaction-time be related to the attention-value of the distractor? (3) Will the distractor effect different increases in reaction-time at different times during the observation of the advertisement and so indicate waxing and waning of attention to it? (4) Will the method show any consistent difference in the attention-values of colored versus uncolored advertisements? Our experiments answered some of these questions, but not all, and demonstrated the technique to be a sensitive indicator by virtue of the systematic differences found among reaction-times under distraction. Some subsidiary questions arose during the course of the experiments, one or two of which will be treated in this report.

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[8] W. Wundt, *Grundzuge der Physiologische Psychologie.* 1st ed., 1874, 745-749.

[9] E. J. Swift, Disturbance of the attention during simple mental processes, this JOURNAL, 5, 1892, 1-19.

[10] J. McK. Cattell, The time taken up by cerebral operations, *Mind,* 11, 1896, 220-242.

[11] L. R. Geissler, The measurement of attention, this JOURNAL, 20, 1909, 473-529.

[12] K. M. Dallenbach, The measurement of attention, this JOURNAL, 24, 1913, 465-507; The measurement of attention in the field of cutaneous sensation, *ibid.,* 27, 1916, 443-460; E. E. Cassel and K. M. Dallenbach, An objective measure of attributive clearness, *ibid.,* 29, 1918, 204-207.

[13] H. Woodrow, The measurement of attention, *Psychol. Monog.,* 17, 1914, (no. 76), 8.

[14] *Ibid.,* 99.

METHOD AND PROCEDURE

Materials. The stimuli were advertisements of the full-page type, selected from the 1936 and 1937 issues of the *Women's Home Companion, Delineator,* and *McCall's* magazines. All were in color, and since they were to be photographed both in Kodachrome and in black-and-white, care was taken to use only those with sufficient brightness contrast for good photographic results. The photographs were taken on 35 mm. film and mounted in 2 x 2 in. lantern slides.[15] Thus, there were 100 slides in all, the same advertisement appearing in colored and uncolored form. Great pains were taken to equate colored and uncolored stimuli with regard to total brightness effect. Ordinary light meters were not found adequate for this purpose. We therefore resorted to subjective methods of equating them. Three judges observed the slides in successive pairs projected on the same screen. They first equated the pairs, colored and uncolored for total brightness effect, and then matched all pairs with a single standard. A rheostat in the circuit of the projection lamp controlled the illumination. The judges were very consistent with one another and with themselves at different times, so we felt that the stimuli were fairly well equated for total brightness effect. Brightness contrasts within an advertisement were approximately the same on colored and uncolored stimuli. The addition of color contrast to the colored stimuli had to be accepted as a natural part of the colored advertisement and if effective for attention its potency is merely added onto that of the mere presence of color.

Apparatus. S was seated in a small, unlighted room, and faced a 21 × 24-in. ground-glass window which served as a projection screen. The room was free from all disturbing distractions. The projection lantern was located in *E*'s room and mounted within a light-proof housing that fitted around the projection screen. The screen was covered with a cardboard frame to fit exactly the projected advertisements, leaving no diffusion or uneven margins. The slides themselves were also carefully framed with Scotch tape for the same purpose.

The timing device for measuring reaction-times was a Stoelting impulse counter in which the unit on the scale is 8.33 ms. The timer was connected in series (using an 8-v. transformer) with *S*'s key and with *E*'s key. The sound stimulus was the sound of the timer itself. The instrument was enclosed in a wallboard box to make its buzzing sound just audible to *S*, and this intensity was maintained throughout the experiments. *S* was instructed to press his key down when the advertisement came on the screen and to react to the buzzer by a release movement. Thus when *E* desired to give the sound stimulus he closed his switch, starting the timer. *S*'s release of the key broke the circuit and stopped the timer. The method had the advantages of simplicity and accuracy.

A recognition-test was developed to measure differences in retention of the colored and uncolored advertisements after 24 hr. This was intended to give us a criterion of attention-value that has already been employed, with which to compare our reaction-times under distraction. For this purpose, short sentence descriptions of 34 of the advertisements were prepared. An additional 34 descriptions of advertisements not presented were prepared to mix with them for the recognition test.The recognition-test was administered 24 hr. after the presentation of the advertisements.

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[15] We are indebted to N.Y.A. assistance for photographic preparation of the lantern slides.

Procedure. Twenty *S*'s, 8 men and 12 women, were employed in these experiments, all except 2 being selected from classes in beginning psychology. Each advertisement was seen by every *S* in both its colored and uncolored form. In one series (Series A) advertisements 1 to 25 were colored and advertisements 26 to 50 were uncolored. They were presented in the following counterbalanced order, 'C' denoting colored and 'U' uncolored: C U C U C U U C U C U C. The order was repeated through the 50 advertisements. In the other series (Series B) advertisements 1 to 25 were uncolored, and advertisements 26 to 50 were colored. The order of presentation of colored and uncolored slides was reversed. Half the *S*s (Group I) saw Series A at one sitting and Series B on the following day. The other half (Group II) saw Series B at one sitting and Series A on the following day. In the manner just described the effects of certain extraneous factors, such as size, contrast, human interest, and novelty were controlled.

Instructions. The instructions to the *S*s were as follows:

> You are going to be shown a series of advertisements. When an advertisement is flashed on the screen, press down the reaction-key. While you are looking at the advertisement a buzzer will sound. As soon as you hear the buzzer, release the key, then press it down again so you will be ready for the next reaction. Pay close attention to the advertisements. You will be given a test over what you have seen.

It was felt that the last part of the instructions was important in order to insure a less ambiguous attitude toward the advertisements.

Every *S* came for three experimental periods, on three consecutive days. In the first period *S* was given 200 practice reactions with the buzzer alone, without any advertisements being presented. After a 5-min. rest period 100 more reactions were taken to determine *S*'s average reaction-time without distraction. In this series the reaction-stimulus was given in groups of 25 trials with an irregular interval of 11 to 15 sec. between stimuli. No separate warning signal was given before each stimulus.

In the second and third experimental periods the advertisements were presented. A practice series of 25 reactions to the buzzer alone was given, following which the advertisements were shown. During the exposure of each advertisement the buzzer was sounded three times, once each during the intervals of 3-6, 12-15, and 21-24 sec. after the first moment of exposure. The position of the reaction-stimulus within these three intervals was varied at random. Each advertisement was shown for a total of 25 sec.

RESULTS

Reaction-times. A summary of the average reaction-times obtained under the various conditions of our experiments will be seen in Table I.

We are especially to note whether the reaction-times under distraction of the advertisements are longer than normal; whether there is any difference in reaction-times under the influence of colored versus uncolored advertisements; whether the latter difference, if any, is correlated with a corresponding difference in recognition scores; and whether there is any systematic change in reaction-time as *S* continues to observe the advertisements during the 25-sec. period.

It is quite clear that the average reaction-times taken during obervation of the advertisements are longer than normal. This result held true for every *S* as well as for the averages for all *S*s. Even the shortest reactions, obtained during the third interval (21-24 sec.), are about 10 units greater than normal. The longest reactions, obtained during the first interval (3-6 sec.), are about 20 units greater than normal. In terms of milliseconds the difference is about 83 ms. in the first case and 167 ms., in the second. To what may the differences be attributed? Since the only difference in the conditions is the presence of the advertisements and the instructions to observe them for later recall in the one case, and their absence in the other, the inference is hardly escapable that the competing influence of the advertisement detracts from attention to the reaction to the buzzer. We do not know what the real functional relationship is between the degree of attention demanded by the advertisement and the change in reaction-time. All we can say is that the longer the reaction-time the greater the distracting effect of the advertisement.

TABLE I

General Summary of Normal Reaction-Times, Reaction-Times with Distraction, and Recognition-Scores

(Reaction-time unit=8.33 ms.; C=colored; U=uncolored.)

	Group I (Av. normal RT=34.4 units)				Group II (Av. normal RT=33.1 units)			
Interval	2nd sitting		3rd sitting		2nd sitting		3rd sitting	
	C	U	C	U	C	U	C	U
1	56.8	60.8	51.7	50.4	54.7	54.9	52.2	53.4
2	46.8	46.4	43.6	43.7	43.9	45.1	45.4	45.1
3	46.6	46.5	44.1	44.4	42.3	43.1	45.5	44.8
D_1-D_2	10.0	14.4	8.1	6.7	10.8	9.8	6.8	8.3
CRσ	7.3	8.5	5.8	5.0	5.3	14.6	4.9	4.9
Recog.-scores			8.4	2.4			7.2	6.0
Diff.			6.0				1.2	
CR$_\sigma$			2.1				0.4	

As to the second question, any difference between the colored versus uncolored stimuli, we find no marked or consistent differences in Table I. This holds true at all intervals and sittings. The means for the data of presentations 1 and 2 combined, first interval, 1, are 53.8 units for colored and 54.9 units for uncolored advertisements (see Table II). The difference of 1.1 units is in favor of the *uncolored* advertisements, but it cannot be regarded as statistically significant. From this we should have to conclude either that there is no difference in attention-value (or distracting power) as between colored and uncolored advertisements or that our new method was incapable of discovering it. In view of the strong general detracting effect shown for all advertisements, we are disposed to favor the first of these two inferences. The fact that there is a marked change in reaction-time during the time of observation of the advertisement, as we shall see later, shows the possibility of the method's differential power and leads us to the same conclusion.

TABLE II

COMPARISONS OF AVERAGE REACTION-TIMES DURING DISTRACTION WITH COLORED VERSUS UNCOLORED ADVERTISEMENTS DURING THE DIFFERENT PRESENTATIONS AND INTERVALS

(Reaction-times in units of 8.33 ms.)

Presentation	First interval			Second interval		
	C	U	Diff.	C	U	Diff.
First	55.7	57.8	2.1	45.4	45.8	0.4
Second	51.9	51.9	0.0	44.5	44.4	—0.1
Both combined	53.8	54.9	1.1	44.9	45.1	0.2

Recognition-tests. The data from the recognition-tests are somewhat different. The recognition scores were based upon the number of correct recognitions minus the number of advertisements not recognized. The average scores were 7.8 and 4.2 for the colored and uncolored stimuli, respectively, when data from Groups I and II are combined. Twelve *S*s had a higher recognition-score for colored stimuli, 5 had a higher score for the uncolored, and 2 had equal scores for the two kinds of stimuli, one *S* not having taken the recognition test. The critical ratio of the difference for all *S*s combined is 1.79, which is below the level of significance.[16] The strong suggestion of a general difference in favor of the colored advertisements here, as contrasted with a slight reversal of the difference, if anything, in the case of the results of the distraction-test, is in line with the earlier findings.[17] It is our view that recognition-scores, as compared with other criteria of attention to stimuli, are heavily contaminated with factors other than differences in attention and hence cannot be used as unadulterated indicators of attention-value of stimuli.

There is a possibility that the distraction-test failed to discriminate between the two kinds of stimuli in a manner consistent with the recognition-test due to the fact that the attention of the *S*s was too nearly maximal in both cases. The instruction for *S* to give close attention in order to be able to recall them may have brought this about. The factor of hue would then be relatively impotent to produce an increment of attention. It might even be that the *S*s were set to give close attention to the uncolored advertisements as a compensatory measure because of a vague realization that uncolored material was harder to grasp and remembered than colored. In opposition to the last hypothesis is the fact that, if anything, they did retain more from the colored stimuli. In opposition to the idea that when attention to the advertisements is maximal there can be no increment due to color is the fact that during the later intervals of reaction, when the reaction-times regressed back toward the normal we cannot say that attention was maximal by any means, and yet the reaction-times tell the same story as regards any difference between colored and uncolored stimuli. We doubt whether attention was actually maximal during the first interval, for had it been so there would have been many failures to react. This was not the case. We are therefore brought back to a faith in the method as a fair criterion of attention and to the conclusion that colored stimuli have no clear advantage over uncolored ones as claimants for attention. It is true that Nixon's result showed a slight advantage for colored advertisements during the first few seconds. Our first reactions, coming 3–6 sec. after the beginning of the exposure may have come too late to catch this early difference. A more searching exploration of these early seconds is desirable, not only to determine any possible early difference between the two kinds of stimuli, but also to study the general early shortening of reaction-time as compared with the later decrement.

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[16] In all tests of significance we have observed the small-sample principles.

[17] Particularly those of Nixon previously cited.

Decrement during distraction. The fact that the reaction-times were longer on the average during the first interval than during the second and third intervals has already been mentioned. The averages during the last two intervals are approximately equal, the grand averages being 45.0 and 44.7, respectively, as compared with an average of 54.3 for the first interval. A summary of the differences between intervals one and two is given at the bottom of Table I, along with the critical ratios for those differences. Under all conditions the differences are decidedly significant.

Such a decrement in the reaction-time between first and subsequent intervals could arise from two sources: (1) A decrease in distracting power of the advertisement as time goes on; and (2) a change in the set of the individual because of his reaction during the first interval. If the decrement is due to the second factor alone, then the reactions of *S* to a stimulus during the second interval, none having been given during the first, should be as long as the usual first-interval reactions. In order to test this deduction a control experiment was introduced. Five new *S*s, all but one of whom were graduate students, were given the reaction-time test during observation of one series of the advertisements. All procedures were the same as for the earlier *S*s except that during half of the observations reactions were called for during the first interval as usual but during the other half no reaction was called for until the time of the second interval, that is, 12–15 sec. after the beginning of the observation. The two kinds of observations were given in counterbalanced order, 25 reactions being obtained under each kind for every *S*.

The results of the control experiment are given in Table III. The average reaction-time during the second interval when a reaction was called for during the first interval was 35.9 units. When no reaction was called for during the first interval, the average was 37.9. In general, there is only a slight tendency toward a lengthening of the reaction-time during the second interval when response during the first interval is omitted. The mean difference is only 2.0 units, to be compared with a difference of 11.4 units between average reaction-times for first and second intervals for these same *S*s. Two of the 5 *S*s actually have a slightly larger average reaction-time during the normal second interval than during the control second interval. A preponderance in favor of the second control interval such as exists in the total averages might be ascribed to the longer total preparatory interval (about 12 sec. as compared with about 3 sec.).

TABLE III

Average Reaction-Times during Distraction With and Without a Reaction during the First Interval

(Reaction-times in units of 8.33 ms.)

S	First interval	Second interval	Second interval (Control)
L	47·9	34·9	32·4
C	54·6	39·2	46·5
ED	50·1	38·1	41·8
W	44·1	33·2	32·7
FD	39·9	34·1	36·0
Mean	47·3	35·9	37·9
Diff.		11·4	9·4

The results of the control experiment definitely point to our first hypothesis rather than to our second. More parsimoniously, they show that our second hypothesis is incorrect. Having no better theory than our first, we are inclined to take the decrease in reaction-time during observation of the advertisements to mean a decrement in attention. If this be true, the degree of attention drops most rapidly during the first 12 sec. of observation and then more slowly, if at all, during the next 10 sec. At the end of the 25 sec. there is some attention to the advertisements remaining, since the reaction-time for the third interval is then still some distance from the normal reaction-time, or the reaction-time found without the presence of the advertisements. There should be a systematic study of the general picture of decreasing reaction-time, extending the time of observation beyond 25 sec. Such a study would probably yield some quantitative laws of attention under such circumstances. In view of the fact that *S* is really under a double instruction so far as attention is concerned, one can view his decrement in reaction-time as a continuous readjustment as regards these competing demands upon his reactive mechanisms.

Corollary results. (a) Effect of color. When comparing the decrements in reaction-time that occur with colored versus uncolored advertisements, some slight differences appear. These differences are noticeable in Table II. It appears that the decrements from interval one to interval two are greater for uncolored than for colored advertisements. The average decreases are 10.3 and 12.0 units for colored and uncolored stimuli, respectively, or an average difference of 1.7 units. This difference amounts to 14 or 15 ms., and though not statistically significant, it occurs quite consistently for different *S*s. If such a difference were to hold up in further tests, it would probably mean something with regard to the relative power of colored versus uncolored advertisements to hold attention. The same difference failed to show up in the second presentation of the advertisements, so we are unjustified in urging any interpretation in this particular use of the attention-test. In other situations, however, we might expect that the relative holding powers of different kinds of stimuli could be determined by our method.

(b) Factor of familiarity. As in most experiments on attention, adaptation effects almost inevitably set in. *S*s form habits of attending which may detract from differences found early in the experiments. By comparing the results during first and second presentations of our material we can test the effect of adaptation or familiarity in our test.[18] In this we will confine our attention to the first interval, where the greatest evidence of attention-value were found. The mean reaction-time for the first presentation was 56.8 units, and for the second 51.9 units. The difference is 4.9 units, or about 40 ms., with a critical ratio of 4.6, which is very significant.

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[18] It should be remembered that the *S*s saw the same advertisements during the third sitting as in the second, except that the colored advertisement seen in sitting 2 were uncolored in sitting 3, and conversely.

This decrease could possibly be attributed to a practice effect, but against this hypothesis is the fact that for the second and third intervals no such differences appear. Since there was still much room for improvement, as compared with the normal, undistracted, reactions during the second and third intervals, and no apparent learning appeared in them, we are inclined to rule out learning as the explanation of improvement in reaction-time during the first interval. We are more inclined to attribute the change during the first interval to a lowered attention during the second presentation, due to the factor of familiarity. This decrement, though smaller, is of the same kind as occurs during the same period of observation, even though the advertisement as seen the one time was colored and the other time uncolored. Had the repetition been identical, the decrement might have been greater than it was. We are inclined to take all such indications as support for the validity of the method and of its rather sensitive qualities for detecting differences in attention to stimuli.

SUMMARY AND CONCLUSIONS

A new method for measuring the attention value of stimuli was developed and tested with colored versus uncolored advertisements as stimuli. The principle of the method is that reaction-time to a faint sound stimulus is lengthened in proportion as the individual attends to another stimulus which he is instructed to observe in order to recall or recognize it later. The results showed that:

(1) Reaction-time is considerably lengthened while observing an advertisement during a 25-sec. period.

(2) There is a decrease in reaction-time during the course of the observation of the advertisement, rapid at first then slow or stationary, but during a 25-sec. interval the reaction-time does not reach the normal level which prevails without the competing effect of the advertisement. This decrement appears not to be due to the fact that *S* makes reactions repeatedly through the interval, but rather seems related to the waning of attention to the advertisement.

(3) The reaction-times were significantly shorter during a 3 to 6-sec. interval upon a second showing of the same advertisements, probably due to a lowered attention and not to practice.

(4) Reaction-times were about the same during observations of both colored and uncolored advertisements, if not slightly longer during the uncolored. There was slightly, but not significantly, more decrease in reaction-time during uncolored than colored stimuli.

(5) Recognition-tests gave colored advertisements a higher average score than uncolored ones, though the difference was not statistically significant. In line with earlier findings, this suggests again that recognition-tests are not pure measures of attention-value.

(6) All indications point to the fact that the new method of reaction-time during distraction by observation is really a measuring instrument, and perhaps a delicate one, of attention-value of stimuli. A checking against other established methods is needed, however, before its validity can be thoroughly established.

OCULAR PATTERNS AS AN INDEX OF THE ATTENTIONAL VALUE OF SIZE

By HERMAN F. BRANDT, Drake University

Numerous techniques have been employed to evaluate the attentional values of size. Among those most commonly employed are the recall, recognition, and coupon return methods. The recall-method consists simply in having a *S* look through a magazine and later attempt to recall respective advertisements. The amount of recall and the variable in question are computed to arrive at the ratio between the two. The recognition, another method, is similar to the recall technique except that the *S* is required to identify advertisements in the second examination seen during his first observation. A third method is based on the relation between certain variables and the number of coupon returns of a certain advertisement in a publication.

Other methods have been designed to arrive at conclusions dealing with the problem as stated above. These may be identified by such terms as tachistoscopic, visual-fixation, and techniques in which illumination is varied. Only brief reviews of representative studies are made to illustrate different experimental approaches to evaluate the variable of size as a determinant of attentional advantage.

Recall. Scott employing the recall-method concludes that the half page including twice the area of the quarter-page secures three times the attention, and the full page which is four times the size of the quarter-page receives almost seven times the attention.[1]

Recognition. Strong, resorting to the recognition method, finds that the half page is only forty per cent better than the quarter, and the full page is a little more than twice as good as the quarter-page. His findings suggest that attention value is proportional to the square root of the area.[2]

Coupon return. Rudolph concludes on the basis of 880,620 replies from 441 magazine advertisements that the size of space and reply ratios is 69.32, 73.01, 100.00, and 150.13, for one-half page, two-thirds page, one page, and double page respectively.[3]

Here as in similar studies the reply ratio lagged behind the increases of size. Rudolph found that in some products the effectiveness varies in direct proportion

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* Accepted for publication May 21, 1940.

[1] Quoted from H. E. Burtt, *Psychology of Advertising*, 1938, 159.

[2] E. K. Strong, Jr., The effect of size of advertisements and frequency of their presentation, *Psychol. Rev.*, 21, 1914, 136-152.

[3] F. H. Rudolph, *Four Million Inquiries from Magazine Advertising*, 1936, 25.

to size, while in other cases the smaller advertisements were almost as effective as the larger ones.

Basing his conclusions on 1,395,800 returns from 907 advertisements, Starch concluded that the space, size, and returns yielded ratios of one page:one-half: one-fourth:one-sixth page, to 100:50::25:17.[4] According to his findings the ratios would seem to indicate that the returns increase as space increases making the relation linear.

Falk's study of the returns from advertising conducted by Gordon Van Tine Company in which over one million returns from keyed advertisements were analyzed seems to show that neither the linear rule nor the square-root rule held.[5] He evaluated the relative efficiency of different size advertisements by computing the cost in terms of mail-order returns. His cost ratio of one-half page: one page: two-page is 6:10:16, and would seem to indicate that the relative effectiveness of an advertisement becomes smaller as the size increases. Three different mediums of selected advertisements yielded ratios of 100, 83, 65; 100, 65, 48; and 100, 55, 43; for one, one-half, and one-quarter pages, respectively. In case of increased volume the large space was more effective than the smaller one.

Tachistoscopic. Adams experimented with colored-paper squares (colors used for identification only), 1, 1½, 2, and 3 in. sq.[6] This gave areas in the ratio of 1, 2.25, 4, and 9. A tachistoscope was employed to expose the materials. Four squares of varying size were mounted on the exposure-card in the form of quadrants, and the color seen first represented the size of the paper seen first. (All colors used appeared equally often in every position and size in order to average out all variables except size.) A record was made of the size seen first and the comparative frequency with which the various areas were seen first was established as an index of attentional value. Such figures were obtained for all *S*s and then reduced to ratios.

Adams concludes that an area four times larger than another gets only about twice the attention, while the 3 in. sq. (nine times the area) has an attention ratio of 2.75. To get double the attention it was necessary to increase the area four times, and to get three times the attention the area had to be increased nine times.

Visual fixation. Hackman and Guilford employed a certain type of eye-movement technique to determine the degree of attensity (attention).[7] Their *S*s looked at a rack on which were displayed two advertisements while the experimenters looked through a one-way screen and recorded the eye-movements. They noted the proportionate number of times the attention of the eyes were directed to one or the other advertisements. When, for example, a full page and a half page advertisement were presented simultaneously, they discovered by means of their technique the relative amount of time devoted to each layout.

The results indicated that the first fixation went to the full page 16% more frequently than to the half page. During the first 10 sec., the *S* spent 23% more time looking at the larger advertisement, and for a 30-sec. period he spent 32% more time. They conclude that the larger advertisement received considerably more attention than the smaller one, but it did not by any means command the attention given to the smaller stimulus-card.

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[4] D. Starch, *Analysis of 3,000,000 Inquiries*, 1927, 1-30.

[5] A. T. Falk, Analyzing advertising returns, *Harvard Bus. Rev.*, 1929, 312-317.

[6] H. F. Adams, *Advertising and Its Mental Laws*, 1921, 107.

[7] R. B. Hackman and J. P. Guilford, A study of the visual fixation-method of measuring attention-value, *J. Appl. Psychol.*, 20, 1936, 44-49.

Illumination varied. Curtis and Foster attempted to compare the attention-value of size by varying the intensity of illumination of three stimulus-areas—Greek crosses, 28, 56, and 112 sq. cm., respectively, in size.[8] Using the middle size of these areas as a standard, with a constant intensity of illumination, the other two areas were each in turn compared with it, by means of a projection lantern with a tachistoscopic attachment, at varying intensities of illumination. The areas were exposed horizontally with a fixation-point midway between them. The time of exposure was 110 ms., and the method of constant stimuli was used in the experiment.

Three *O*s (*F, B,* and *D*), all trained in observation, served in study. Every one gave judgments in 200 series: 50 with each of the two comparison crosses in each of the two spatial positions to the right and left of the standard. The results were as follows. For *D,* size seemed to have an effect. To be equal to the standard in attention-value, the smaller cross had to be greater than the standard in illumination and the larger cross had to be less. For *B* and *F,* however, this relation did not obtain. One showed a negative effect of size while to the other size was apparently indifferent. Curtis and Foster consequently concluded that size was not a definite determinant of attention.

Bowman repeated the experiment of Curtis and Foster.[9] She used the stimulus-areas employed by them but a different apparatus and a different method of procedure. Her apparatus was constructed to avoid the technical difficulties met by Curtis and Foster, and she determined the point of subjective equality of the attentional value of the stimulus-areas by the method of limits.

Four *O*s, trained in the observation of clearness (attention), served in four series of experiments: two in each space order with each of the variable stimulus-areas. The results for all the *O*s were constant in direction. The small cross to be as clear as the standard (which was twice its area) had to be more intensely illuminated, and the large cross (which was twice the area of the standard) had to be less intensely illuminated. Bowman found that there was a large space error. When this was canceled out by averaging the results of the two space orders, the conclusion, that size was a determinant of attention, was unescapable.

Because Bowman had varied the method of procedure—had used the method of limits in place of the constant method—it was necessary, before reaching a decision regarding her conclusions, that the experiment be repeated with Curtis and Foster's method of procedure. This was done, with an improved apparatus,[10] by Dewey and Dallenbach.[11] These experimenters used circular openings, disks of light, instead of Greek crosses as the comparison-areas. These areas, two instead of three in number, were respectively 4 and 8 cm. in diam. They were exposed in a horizontal position, to the right and left of a fixation-point which was centered equidistant (10 cm.) between them. The large and the small areas were taken in turn as standard and variable in each of the two spatial positions.

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[8] J. M. Curtis and W. S. Foster, Size versus intensity as a determinant of attention, this JOURNAL, 28, 1917, 293 ff.

[9] A. M. Bowman, Size versus intensity as a determinant of attention, this JOURNAL, 31, 1920, 87 ff.

[10] K. M. Dallenbach, An apparatus for the study of the conditions of clearness, this JOURNAL, 34, 1923, 94 f.

[11] Doris Dewey and K. M. Dallenbach, Size versus intensity as a determinant of attention, this JOURNAL, 35, 1924, 121-125.

These results corroborated those obtained by Bowman. When the effect of position, which they too discovered to be large, was canceled out by averaging, they found that size was a determinant of attention. With the small area at 16 intensity-units of illumination, the large area needed only 5.7 units for *D*, 6.7 for *G*, and 7 for *W* to equal it in clearness. Similarly, when the large area was illuminated at an intensity of 16 units, the small one needed 40.8 units for *D*, 47.8 for *G*, and 48 for *W*.

Purpose.—The purpose of this study is to evaluate by means of ocular photography the relative attention-value of size. The location of fixations and time spent in respective areas will be considered the unit of measurement and is expected to provide an objective criterion for the evaluation of the attensity of the variable under consideration.

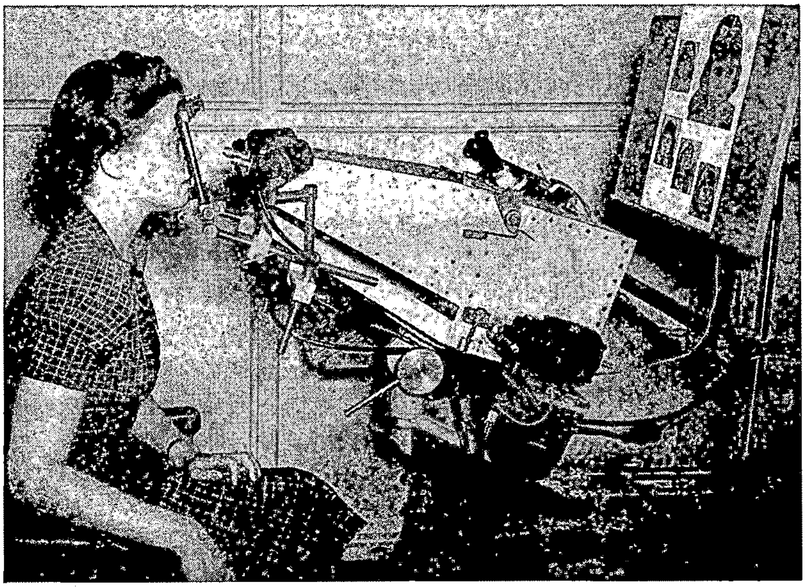

FIG. 1. APPARATUS IN USE.

Procedure: (a) Exposure cards. Two sets of exposure cards were used in this experiment. The first set of four cards (one of which is shown in Fig. 2) consisted of five designs mounted on a card 10.5 x 13.5 in., *i.e.* the size of the page of a standard magazine. One of the designs has four times the area of the other four.[13]

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[13] The size of the large area, designated as Area 5, was 6.6 × 6 in. The smaller areas designated as Areas 1, 2, 3, and 4 were 3.3 × 3 in.

The second set of four cards (one of which is shown in Fig. 3) was identical with the first set with the exception that the individual designs were replaced by pictures. The large design and picture were rotated (as shown in Figs. 4 and 5) so that the *S*s viewed it in all four positions on the card and the smaller ones were reversed in their order for each viewing. This procedure eliminated the variable of position.

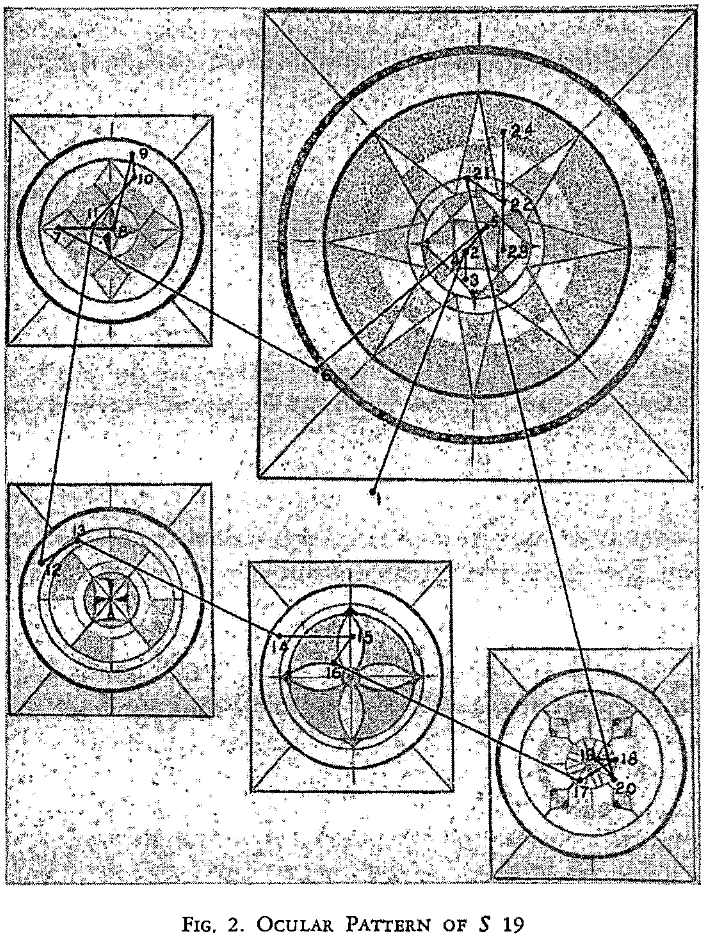

FIG. 2. OCULAR PATTERN OF *S* 19

(b) Subjects. A total of 200 college students (119 men and 81 women), chosen at random, served as *S*s. Of these, 61 men and 39 women observed the designs, while 58 men and 42 women viewed the pictures. Every *S* was given once the following instructions: "You will see a number of pictures or designs, look at

them as naturally as you would when reading a magazine at home." Although the reading time for this card was limited to 10 sec., the *Ss* were not informed in advance of the time-limit. Each card was inspected by 25 *Ss*. No *S* was shown more than one card.

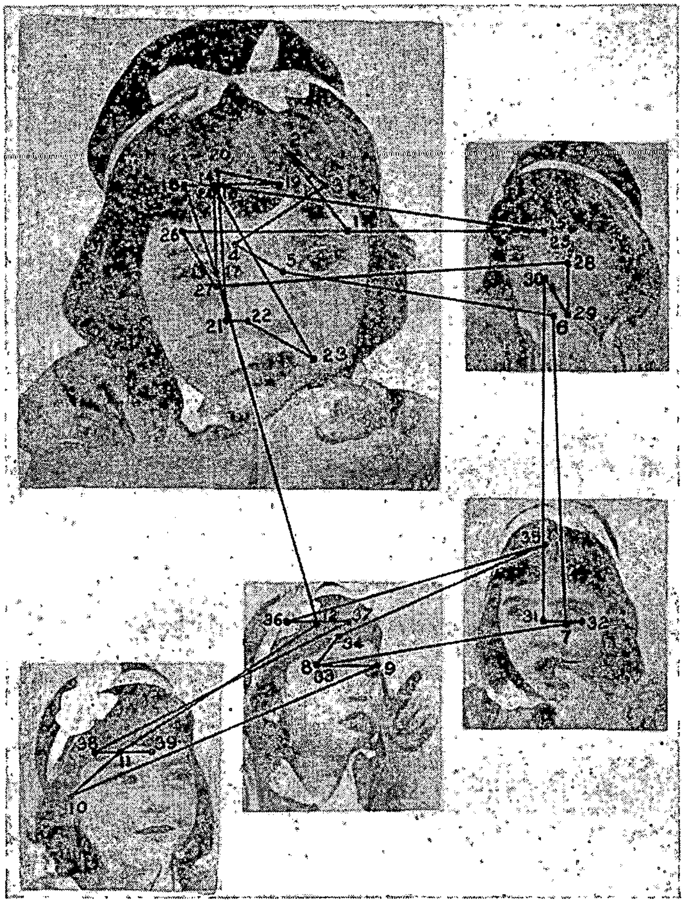

FIG. 3. OCULAR PATTERN OF *S* 24

(c) Apparatus. A bidimensional eye camera was employed in this experiment.[13] After the eye movements of the *Ss* were photographed, every record was projected and the ocular pattern was superimposed on the original content revealing the location and sequence of the fixations and relative amount of time spent on each area.

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[13] H. F. Brandt, A bidimensional eye movement camera, this JOURNAL, 49, 1937, 666-670.

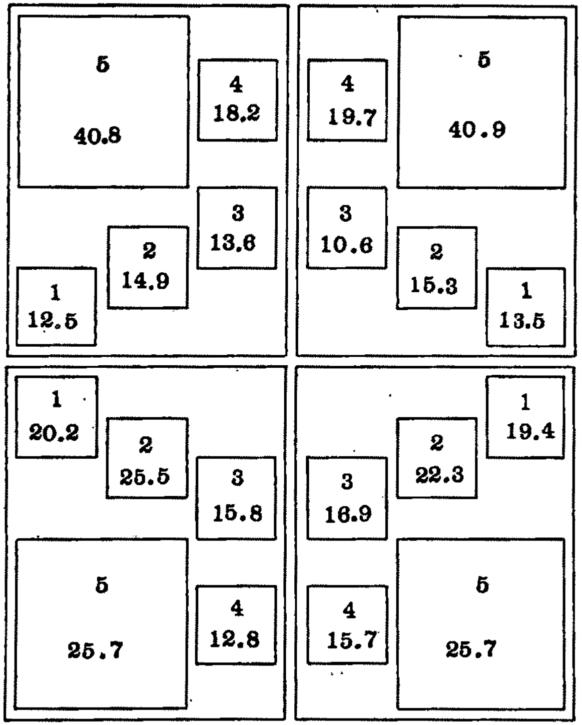

FIG. 4. PERCENTAGE OF TOTAL TIME SPENT ON RESPECTIVE DESIGNS

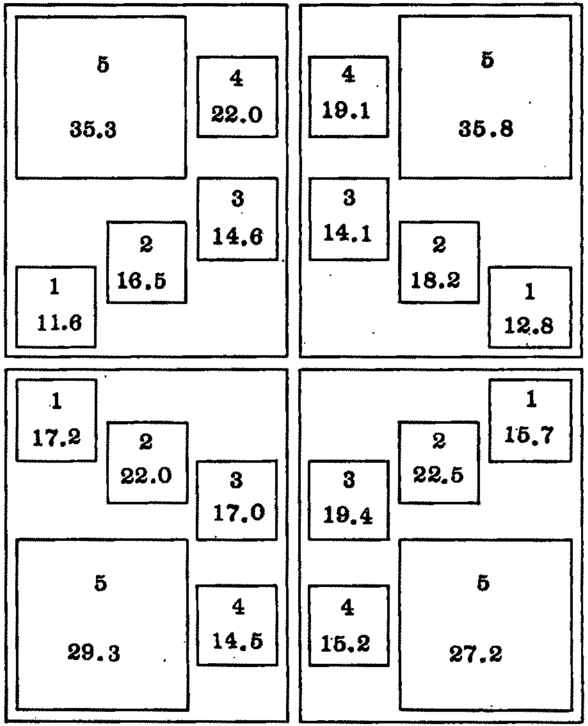

FIG. 5. PERCENTAGE OF TOTAL TIME SPENT ON RESPECTIVE PICTURES

Results. (a) Ocular patterns. While a total of 200 *Ss* were used in this investigation, the ocular patterns of only 2 *Ss* are presented here to illustrate the major characteristics. (Fig. 2 represents an ocular pattern of one of the four exposure cards of the designs, and Fig. 3 represents one of the pictures.)

The relative time spent in respective areas of each exposure-card is indicated in Figs. 4 and 5. Here as in previous studies position is a potent factor of attensity.[14] Considerably more time is spent in the large areas when placed in the upper half of the field than when located in the lower part of the exposure-card. Position as indicated in Figs. 4 and 5 affects the attensity of the smaller areas also but in no case does the time spent on the smaller areas equal that of the time devoted to the larger ones.[15]

TABLE I

TOTAL TIME SPENT IN AREAS 1, 2, 3, 4, AND 5 OF THE DESIGNS

(Time in sec.)

Position of large area	Area 1	2	3	4	5
Upper left	113.8	135.4	123.4	165.6	371.6
Upper right	123.1	139.4	96.7	179.8	373.6
Lower left	190.9	240.5	148.8	120.8	243.1
Lower right	176.9	203.2	154.5	143.0	234.4
Mean	151.2	179.6	130.8	152.3	305.7

TABLE II

TOTAL TIME SPENT IN AREAS 1, 2, 3, 4, AND 5 OF THE PICTURES

Position of large area	Area 1	2	3	4	5
Upper left	106.3	150.1	132.9	200.2	321.6
Upper right	144.9	163.6	127.3	172.4	322.9
Lower left	154.6	197.9	153.3	130.4	264.4
Lower right	148.4	212.8	183.0	143.8	256.8
Mean	131.1	181.1	149.1	161.7	291.4

Table I and II represent the total time spent on respective areas for both the design and pictured areas. From the analysis of the data in the tables a significant difference in time spent exists between the attentional value of the smaller and larger areas of the exposure cards.

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[14] Brandt, Ocular patterns and their psychological implications, this JOURNAL, 53, 1940, 260-268.

[15] The number in the upper half of each area in Figs. 4 and 5 represents the respective design or picture area, while the numbers below represent the relative time in percentage.

It is apparent from Table III that approximately 17% of the total time was devoted to each of the small areas while 32% was devoted to the larger one.

This difference represents an attentional ratio as based upon time spent on small and large areas of 1:2 while the size is in the ratio of 1:4.

The results of this study show that regardless of the position or the character of the mass, significantly more time is spent on the larger areas than on the smaller ones.

TABLE III

PERCENTAGE OF TOTAL TIME SPENT IN AREAS 1, 2, 3, 4, AND 5 REGARDLESS OF POSITION

Exposure cards	AREA				
	1	2	3	4	5
Designs	16.2	20.4	14.1	16.4	32.9
Pictures	14.3	19.8	16.3	17.7	31.9

Summary and conclusions. The present study is an attempt to evaluate by means of ocular photography the attentional value of size. The appraisal of the attensity of size by means of ocular photography is more objective than many of the techniques of analysis employed earlier. Psychology employing this new device (as illustrated by this study) aims to minimize the reliance on introspective measures and establishes an objective criterion of evaluation.

TABLE IV

MEAN DIFFERENCE OF AREAS 1–4 AND 5 AND THE CRITICAL RATIOS OF DESIGNS AND PICTURES

Exposure cards	Areas	M	S.E.	$M_{Diff.}$	$S.E._{Diff.}$	C.R.
Designs	1–4	153.49	3.57	152.17	13.79	11.03
	5	305.66	13.22			
Pictures	1–4	155.74	2.94	135.69	12.18	11.01
	5	291.43	11.96			

Since some of the investigations on size vs. attentional value, reviewed in this study, differ in their conclusions we may surmise that the descrepancy is due to one or more of the following reasons: (1) that different experimental procedures were employed; (2) that variables other than size were not adequately eliminated; and (3) Or that both procedure and variable control were responsible. From all indications it seems to follow that the more adequately the size variable was isolated the greater was the lag of attention relative to increased size areas.

The results of this study are corroborated by the studies of Strong, Dewey and Dallenbach and supports the conclusion that large areas have attentional advantages over smaller ones, but that such advantage lags as the size of the area is increased. The findings also indicate that position and the character of the area are independent variables and potent determinants of attracting and sustaining attention.

Each of the 200 ocular patterns obtained in this study tell a story in terms of fixations and excursions, thus indicating location, duration, and sequence of fixations as well as direction, distance, and frequency of excursions. Each implies a type of attention or interest with its accompanying psychological processes of selection, elimination, comparison, and interpretation.

The results coincide rather closely with the square root effect advocated by Strong, who on the basis of his findings theorized that an area four times the size of another would have only about twice the attentional advantage. What the difference of attensity and size would be for size ratios of 1:2, 1:3, 1:9, 1:12 or some other combination cannot be determined from the results of this investigation. Subsequent studies will provide information which will have a direct bearing upon this problem.

Implications. The writer assumes that the time spent in respective areas of a given field is indicative of the relative attention of an individual's observation. He accepts the term 'attention' simply as that which for the moment seems to be the most vivid portion of the conscious state. Due to the relevance of the source of attentional experience, the writer contends that in any form of sensory appeal two types of incentives operate: one, the mechanical consisting of sensory data such factors as position, size, contrast, isolation, color, or the novelty of the stimulus; and the other, the psychological such as curiosity, interest, or a purpose resulting from the memory residues and present interpretation of the physical stimulus.

On the basis of such an explanation the character of visual experience known as visual consciousness would evidently consist of a fusion of data coming from the eye on the one hand and from the mind on the other. Attensity and cognition operate simultaneously for the most part, but it is likely that the former appeal takes precedence over the latter in the initial period of observation as is the case in this study. In classified advertising, no doubt, the purposed incentive has greater potency and therefore yields returns to the observer in terms of ends sought.

This study has made no attempt to separate the two types of incentives, but assumes that in the initial period of observation, attention is more the result of sensory appeals than of central processes.

Intense stimuli due to the stimulation of a larger number of nerve fibers evidently release greater nerve energy causing as a result an intensified and prolonged activity. The assumption is that in the presence of situations arousing attention the area fixated visually may be considered the object of attention. Of two or more attention provoking situations presented simultaneously, the one eliciting the longer periods of visual fixation over a period of time may be taken to possess the greater attention value. Evidently the larger area has the advantage of greater magnitude, less distraction, and added prestige over that of the smaller ones.

On the other hand, the reason the larger area does not increase the attensity in proportion to the increase in size may be due to the division of the *S*'s time between areas, not in proportion to size but in proportion to respective items in the area.

Even if the ratio between attention and size is only a symbol of relations, it nevertheleses may serve as a basis for the evaluation of space. Size and dollars and cents have much in common in advertising, and it follows that good accounting would be based on an accurate knowledge of the relative effectiveness of physical variables. Certain factors essential in an art production are physical in nature and may be controlled by the designer if their relative attensity is known. A relative attention value may thus be attached to various sizes which may determine an esthetic as well as an economical aspect of the problem.

CHILDREN'S REACTIONS TO ELEMENTS OF SIMPLE GEOMETRIC PATTERNS

By CLARENCE LEUBA, Antioch College

In 1929 Köhler reported some experiments by Hertz with two jay birds indicating that these birds reacted to simple geometric configurations (circles, lines, etc.) as unanalyzed wholes.[1] The birds failed to react selectively to the elements of which these configurations were composed. Though the jays would go directly to a single inverted flower-pot, under which they had seen Miss Hertz conceal some food, tip the pot over, and secure the food, they were unable to go directly to a flower-pot concealing food when that pot was incorporated in some configuration of flower-pots, as in a line or circle of them. The jays seemed to react to these configurations as unanalyzed wholes; when confronted with one of these flower-pot patterns, they tipped over whatever pot they chanced upon first. If, however, the food-concealing pot was on the outside of a line or circle of pots, or if there was a sufficiently wide space between it and the other pots in the configuration, the birds usually picked it out correctly at once.[2]

The experiments to be described in this article were designed to discover whether young children perceived common geometric configurations like lines, circles, squares, and triangles, even when these were composed of discrete units, as unanalyzed wholes in the same fashion as the jays.

PRELIMINARY EXPERIMENTS

Hertz's experiment with the jays was adapted for use with a young child (the author's son) as follows: while *D* (the child) sat in a chair about 6 ft. away, *E* (the father) placed a bit of chocolate inside one of a series of small safety match boxes (¾ × 1½ × 2¼ in.) from which the box part containing the matches had been removed. The match boxes were upside down, plain under side showing, and were arranged in one of three configurations: a straight line, usually parallel to the wall in front of the child, a circle, or a square. The number of boxes used in any one configuration varied from 6 to 14; the distance between two adjoining boxes was never more than ½ in. *D* was fond of chocolate and always spontaneously followed the piece of chocolate as it was placed in one of the boxes. To make doubly certain of attention to this process, however, *E* always described it verbally and looked at *D* while the box containing the chocolate was being replaced in the pattern; *E* made sure that *D*'s eyes were on that box. To prevent *D* from maintaining a bodily orientation toward the box containing the bit of chocolate, he was then asked to turn away from the match boxes and with his back to them to wait 1 min. before seeking the chocolate.[3] *D* was given several trials during each of three sessions when

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* Accepted for publication March 1, 1940.

[1] W. Köhler, *Gestalt Psychology*, 1929, 158-162.

[2] A more recent and complete description of these experiments is given by Hertz in W. D. Ellis *A Source Book of Gestalt Psychology*, 1938, 238-252.

[3] In children as well as in animals, bodily orientation is an important factor in reacting correctly to one of a number of objects. Cf. L. L. Emerson, The effect of

he was 29 mo. old, and during each of two sessions at 31 mo.

In nine trials with the boxes arranged in a line the number of boxes used varied from 6 to 14. Numbering the boxes from left to right, the chocolate was placed twice in box 3 and once in each of the following positions: 4, 5, 6, 7, 8, 10, and 14. Seven times *D* went to the wrong box and twice to the correct one. With the circle (the number of boxes again varied from 6 to 14) he made five wrong first choices and one correct. A square with three boxes on a side was, however, easy for him; he made three correct first choices and no wrong ones.

From *D*'s behavior it could reasonably be inferred that he perceived both the circle and the line as unanalyzed wholes; he seemed to know that the chocolate was in the circle or in the line and not in some other part of the environment, but the specific location in a particular element of these configurations was not usually perceived. In the case of the square, however, with three boxes on a side, the corners and the middle of each side (the box between two corners) seemed clearly distinguishable to him.

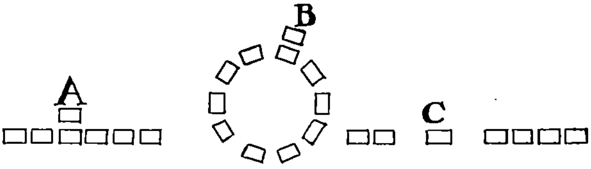

FIG. 1. ARRANGEMENT OF THE MATCH BOXES
Boxes A, B, and C containing the chocolate were picked on the first choice.

When the box containing the chocolate was just outside the line or the circle, *D* made four right and no wrong first choices. Likewise when the chocolate box was face-side up (design showing) in the line, or had a 2-in. space on either side of it, he went directly to it (three trials) without first picking up any of the other boxes (see Fig. 1). He could pick out correctly one specific box, even though it touched another box, if it was not included in the pattern forming the line or the circle.

When 11 boxes were scattered in a disorderly arrangement on the floor in close proximity to one another, *D* was again unable to pick out the box containing the chocolate correctly on the first choice (three trials). He approached the bunch of boxes and then seemed to pick one at random.

Though *D* was probably a typical child, as far as the functions tested were concerned, just as Hertz's two jays were probably typical jays, it was desirable to make more certain of this by repeating the experiment using many children. Several possible sources of error were present in this preliminary experiment and could be avoided. Possibly *D* had not been sufficiently motivated to make the first choice a correct one since he usually got the chocolate eventually anyhow; perhaps the lines between the floor boards were guides and were partly responsible for the few correct first choices made; perhaps the *E*'s personal equation had something to do with the results.

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bodily orientation upon the young child's memory for the position of objects, *Child Develop.*, 2, 1931, 125-142.

MAIN EXPERIMENTS

Experiments, similar to those performed with *D*, were carried out by several experimenters at the Antioch Nursery School. The boxes were placed on a uniform background (wrapping paper) and the children were permitted to eat the chocolate only if they picked the correct box on their first choice.[4]

Experimenters *L* and *M* used 11 children for three sessions; experimenters *L* and *S* used 10 children during one session. The children were from 1½ to 5½ yr. old. They were taken singly to a small room; after rapport had been well established the child was shown one of the small boxes (usually little, round pill boxes instead of the match boxes) and encouraged to practice opening and shutting it until he could do so easily. He was given a small piece of chocolate to eat. While the *E* arranged the boxes (from 6 to 21 in number) in a pattern—a line, a circle, a square, or a triangle—on a large sheet of paper on the floor, the child was seated in a chair about 10 ft. away. He was shown a bit of chocolate as it was placed in one of the boxes and told that he could have it, if after a minute he could pick up the box containing the chocolate without touching any of the other boxes. A screen was placed for one minute between the child and the pattern while his attention was directed to the hand of a stop-watch as it made one revolution.[5] If on several trials the child failed to find the chocolate on his first choice, to prevent discouragement, it was placed on the next trial where it could be found with relative ease, as in the box at one of the ends of a line of boxes. Not all the children in each group were used for each pattern.

TABLE I

NUMBER OF RIGHT AND WRONG FIRST CHOICES

Arrangement	Line	Circle	Square	Triangle	Haphazard
No. boxes	6 to 21	12	36	12 or 18	12
No. Ss	21	9	17	13	4
Right	70	4	44	36	2
Wrong	103	5	42	42	2

TABLE II

NUMBER OF RIGHT AND WRONG FIRST CHOICES

	Chocolate in box	
	Just outside line (12 Ss)	Just outside square (2 Ss)
Right	10	2
Wrong	2	0

Results. As indicated in Table I, the children made about as many wrong as correct first choices with all the patterns except the line; in that case, the wrong first choices greatly exceeded the correct ones. When the chocolate was placed in a box just outside, but touching, the line or the square the first choices were almost always correct (Table II).

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[4] The writer is grateful to Dr. Jane Cape and Dr. Virginia Nelson for permission to use the children at the Antioch Nursery School and to Augusta McMurray, Dorothy Lunt, and Gladys Sterick who acted as experimenters.

[5] When the choice to be made is one out of three objects, Skalet found that children from 2 to 4 yr. old could make correct selections even after several days delay. (M. Skalet, The significance of delayed reactions in young children, *Comp. Psychol. Monog.*, 7, 1931, (no. 34), 1-82.)

Discussion

Though not a single one of the 21 children could react dependably to the various individual boxes composing the line, the circle, the square, or the triangle, most of them reacted correctly to a much greater extent than might be expected on the basis of merely chance successes. It was apparent both from the children's movements and their remarks that these configurations were not completely unanalyzed wholes. The line had ends and a middle; the square and the triangle included lines having ends and middles. When the chocolate was concealed in a box at one of these points, especially at the ends of a line, it was usually secured without hesitation on the first choice. When the chocolate was not at one of the positions described, the child seemed usually to know at least the general direction in which to look, as for instance, near the middle or on the right. The successes in the case of the circle were due mainly to the fact that the chocolate was concealed every time in the box directly opposite and furthest from the child.

In one session in which 9 children were given a total of 35 trials with the line, of the 12 correct first choices, 11 were when the chocolate was hidden at one of the two ends. Occasionally a child would plead with *E* to "please put the chocolate at one of the ends." The position next to a well recognized one, also yielded a relatively high number of correct first choices. Thus, when the chocolate was in a box next to the end one, it was usually secured on the first choice, but not as frequently as when it was put in the end box itself. Though all the children were asked after each trial how they made their choice, only one child mentioned counting: he said the chocolate was in "the box next to the three at the end."

We had too few children at each age-level to draw any conclusions regarding the relationship of successful first choices to age.[6] There was also a tendency on *E*'s part to include a smaller number of boxes in the configurations used with the younger children than in those used with the older ones, and possibly to place the chocolate in easier positions for the younger children so they would not become discouraged.

We conclude that though these children between 1½-5½ yr. of age did not react discriminatingly to each of all the units composing the lines, circles, squares, and triangles with which we experimented, it was possible for them sometimes to single out various aspects of those configurations such as the center, the right, or the left, and even certain specific elements, such as the end or the corner box, and react specifically to them. The configurations were not always completely unanalyzed wholes. The number of correct first choices seemed to depend upon several variables: the number of elements in the configuration, the type of configuration, the particular child, and especially the particular place in the configuration where the chocolate was concealed.[7]

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[6] In the investigation by Emerson (*op. cit.*), the accuracy of selection was positively correlated with age.

[7] *E* secured almost 100% wrong first choices on the circle and the line in the preliminary experiment with *D* probably because the chocolate was not usually concealed in any one of the distinctive positions in those configurations. For instance, it was very rarely placed at the ends, or even next to the ends of the line.

LIBRARY UNIVERSITY

THE RECOGNITION VALUE OF THE STEPS OF THE DIATONIC SCALE

By HENRY WUNDERLICH, University of Texas

The notes or steps of the major diatonic scale may be designated by the familiar names *do, re, mi, fa, sol, la, si.* The object of the present experiment was to study the relative recognition values of these seven scale notes. The equally tempered diatonic scale was used.

A scale note, like *mi* or *sol,* is not a tone of a fixed frequency but a pitch that has a definite relation to the key-note. As a stimulus it exists, therefore, only in the tonal context that defines the key. The behavior of tones in a melodic series is governed by a number of laws, and, if these laws are broken, the tones lose their character as scale notes and the series of tones loses its established key.

The stimuli used in this experiment were melodic fragments, seven notes long. They are called melodic fragments because each note in the series obeys the melodic laws that govern the scale-step that the note represents. In any such series, the seven notes would mutually define one another as scale-notes, the first three or four notes bearing nearly all the burden of establishing the key. In order to keep this task entirely outside the series proper, each stimulus was preceded by a perfect cadence, which completely established the key.

Each scale-note was used once in each tune. All the notes in a tune lay within the compass of an octave. A tune might, for example, involve the seven scale steps as follows: *si, do, mi, re, sol, la, fa;* with *mi* occupying the highest pitch position and *fa* the lowest. The tune might be pitched at any absolute level that is within a reasonable vocal compass. The procedure was to establish the key first with a perfect cadence (the dominant and tonic chords), then play the tune, pause, and then repeat the tune with one of the notes changed half a step toward the next higher or lower scale note. *S* was instructed to write down the number of the note which he thought had been changed, whether it was the first, second, third, etc.

In the tune any one of the scale notes could be crucial, *i.e.,* changed on the repetition of the tune. Let us suppose that it is the second scale note, *re,* that is crucial. Then *S* should answer with the number "4," because it is the fourth note in the series that was changed. A system of numbering was devised so that the tunes could be catalogued according to their use. The tune we are discussing now would be catalogued as 24, because the second note of the scale, occurring as the fourth note in the tune, is the crucial note.

A test consisted of 49 tunes. This number was chosen so that each note could appear as crucial in each of the seven serial positions. After every seven problems *S* was allowed to rest a little. In each of these subgroups of seven tunes each note appeared but once. All other matters of arrangement were settled by chance, so that no principle of order could be discovered by *S.* The crucial note could have any rank anywhere in pitch with reference to the other six notes in the tune. Every note occurred once as crucial in each of the seven pitch-ranks in the course of any one test. The relation of pitch-rank and serial rank of the crucial note was settled by chance in each test, but, since seven tests were used, it was possible to exhaust all 49 possibilities for each scale step. The absolute pitch of the tune was also determined by chance, but no tune was allowed to go below the *G* an octave and a fourth below middle *C*, or above the *G* an octave and a fifth above middle *C*. In other words, no tune went below the easy compass of a bass voice or above that of a soprano voice. This method of deciding the absolute pitch of the tune by chance resulted in the use of all 12 major keys at random during the test.

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ADMINISTRATION OF THE TESTS

The tests were given to undergraduate classes in psychology, which varied in size from 30 to 60 students. The administration of a test lasted about 40 min. Every *S* received a blank having seven rows of seven spaces each. The method of taking the test was first explained carefully, and then a practice problem was played and repeated until all the *Ss* could hear the change. They were then required to record the number representing the sequential position of the note changed. These numbers were entered in the spaces from left to right so that when seven problems had been played, each blank in the first row contained an answer. At the end of every row, a short rest was taken, during which the *Ss* might ask questions. No omissions were allowed. The *Ss* were required to guess when they did not know the proper answer.

Immediately before each pair of tunes two chords were played. These chords served a double purpose. In the first place, they were a warning that the problem was to be presented and *S* was thereby prepared for the beginning of the problem. In the second place, the chords, constituting a perfect cadence in the key of the melody, served to establish its tonality. The second of these two functions was essential. Unless the key is established before the problem is played, the first few notes of the standard have to bear the burden of establishing tonality.

If the previous problem has been in another key, then the first few notes of the new problem may easily be misunderstood as members of the other key. The warning chords, however, were sufficient to destroy the perseveration of the previous key and made it possible for every note to appear as a member of the intended key. Since the rigid specifications under which the tunes were written sometimes interfered with their conforming to melodic laws to the fullest degree, a firm establishment of the key prevented any ambiguity that might result from slightly implausible melodic sequences.

The tests were played on a harmonium, with a single eight-foot stop drawn. This instrument was preferable to a piano because, when the bellows are kept full, it gives tones of equal loudness and there is no chance for *E* to accent any of the notes. A metronome, set at 80 beats per minute, was used as a guide. The tests followed the metronome one note to a beat, with a short space occurring between the warning signal and the standard, and also between the standard and the variant. When the *Ss* were asked for comments, a few of them complained that the metronome was a distraction, but most of them said that it was not. At any rate it is probable that the metronome did not affect the results since it was a constant factor throughout the experiment. There were, however, other distracting factors which probably did affect the results. The tests were given in a room with a high ceiling and plaster walls. It was near the street and there was a great deal of traffic noise. Coughing and other disturbances in the room probably reverberated. It may be that distractions like these disturbed the regularity of the results. The erratic behavior of one or two problems in otherwise reliable tests might easily be a consequence of such distractions.

RESULTS

Every test had 49 problems. Every note was changed seven times, once in each of seven problems. The noticeability of the change can be measured by the number of *Ss* who answered the problem correctly. Every problem received a score which is the number of people in the group who answered correctly. For example, Problem 52 might get a score of 19 on the first test, meaning that *sol* is the note that is varied, that it occurs as the second note in the tune, and that 19 people in the group tested noticed a change in the second note and answered 2 in the proper blank on their test sheet. In the second test, Problem 52 might get a score of 28, if more people noticed the change. These raw scores cannot be compared directly. It may be that the second group is superior; or it may be that the pitch-rank of the crucial note is more advantageous; or it may be that the relation of the pitch-rank and the serial position makes the crucial note more prominent. It is the relation of the score to the other 48 scores for a specific test of a specific group that matters. This relation can be expressed by a number that is the quotient of the raw score's deviation from the mean, divided by the standard deviation. This new number is called the x/σ-score. The recognition value of each scale step was measured 49 times in the course of the seven tests, so that each scale step received 49 of the x/σ-scores. It is possible to take an average of these scores because they all represent the degree of difficulty of the change measured in a way that shows the comparative difficulty under the same conditions. Given below is a list of the average x/σ-score for each scale step, and with it is given the standard deviation of the distribution of the 49 scores.

Do Mean = .25 ± .08 S.D. = .84
Re Mean = .27 ± .08 S.D. = .83
Mi Mean = .19 ± .08 S.D. = .80
Fa Mean = —.42 ± .09 S.D. = .97
Sol Mean = —.19 ± .10 S.D. = 1.08
La Mean = .27 ± .08 S.D. = .88
Si Mean = —.33 ± .08 S.D. = .88

An examination of the distributions of the x/σ-scores for the seven notes shows that the average x/σ-score in the case of *do, re, mi,* and *la* is better than the general average for all 343 problems. In fact, the distributions for *do, re,* and *la* practically coincide, and that of *mi* deviates very slightly. The distribution for *sol* has a mean that is a little below the general average, and a standard deviation that is larger than that for any of the other six distributions. The distribution for this note shows a tendency toward bimodality. A positive value for x/σ-score means that the problem was easier than average, while a negative one means that the change was difficult to detect. The means for *fa* and *si* fall definitely below the general average. The pronounced negative tendency of *fa* and *si* is the most striking feature of the whole array of data.

Of course, there is much overlapping of distributions, even between the highest and the lowest. This was to be expected. Even a tune of only seven notes is a complex of many factors, of which scale step is only one, probably one of the weaker ones from the standpoint of recognition value. Tunes will vary greatly in difficulty, and hence the scores of the notes will also vary. The other factors should tend to cancel out in a large number of tunes and allow the influence of scale step to be shown in the central tendency of each distribution.

The fact that changes in *fa* and *si* tend to be difficult to detect is probably related to the function of these changes in modulation. *Si,* whenever changed, was lowered a semitone, resulting in a modulation to the subdominant key, a modulation which is very frequent. *Fa,* whenever changed, was raised a semitone, resulting in a modulation to the dominant key, also very frequent. Since these two modulations are to the two most closely related keys, we should expect such a change easily to pass unnoticed.

The bimodality of the distribution for *sol* suggests a double function for that interval. One possible explanation lies in the dual rôle of this step as a member of both tonic and dominant harmony. More extensive investigation is needed at this point.

CONCLUSIONS

Under the conditions of the present experiment, the seven notes of the diatonic scale were found to differ reliably in their average recognition value. The notes that are changed for the simplest modulations are the ones that have the lowest recognition value, *i.e. fa* and *si. Do, re* and *la* have the highest recognition value, whereas *mi* and *sol* are closer to average.

Recognition value is defined by the conditions of the experiment. Only the major mode in equal temperament was used and the change in the crucial note was always a change of a semitone to a note outside the key. Perhaps the method of substituting some other note within the key, or the use of melodic fragments in the minor mode would give different results. The present experiment is a first step in the study of the psychological properties of the scale and its parts, and also in the study of melodic structure.

PREFERRED REGIONS IN MUSICAL COMPOSITIONS AND THE EFFECT OF REPETITION UPON THEM

By HELEN K. MULL, Sweet Briar College

The purpose of this study is, first, to discover how much agreement exists among a group of 'musical' individuals as to which parts of a musical composition are especially pleasing, and, secondly, to note some of the characteristics of these passages which might account for their 'popularity.' Much has already been discovered about the sources of musical enjoyment through the work of Washburn and Dickinson, Gatewood and others.[1] The present study limits itself in this regard to a further examination of one of the known sources of pleasure, the factor of design, the characteristics of the popular passages which relate to their place in the composition as a whole. A third part of the study furnishes certain facts of interest concerning the way in which pleasure grows as the composition becomes more familiar.

The *O*s were 30 undergraduate students, 20 of them from the Eastman School of Music,[2] and 10 from Sweet Briar College recommended by the Department of Music on the basis of musical sensitiveness and experience. The compositions selected for investigation were Bach, Sarabande from the Second English Suite for Piano; Chopin, Mazurka, Op. 50, No. 3; and Brahms, Intermezzo and Romanze, Op. 118, Nos. 4 and 5. They were presented by a victrola. Piano compositions were chosen in order to eliminate pleasure based on instrument preference alone; and unfamiliar ones were selected to eliminate differences in familiarity. None of the 30 students had played any of the numbers, and only two remembered having heard any of them before.

The method has the virtue of allowing immediate judgments, made upon material in its context. The *O*s were given the following instructions:

> I shall play you a record, and I should like you to listen to it with the purpose of getting esthetic pleasure *directly* from it. Music, as you know, arouses our feelings, but it may do so in more ways than one. It may do so directly, because of intrinsic qualities, or, on the other hand, indirectly, because of personal associations. It is the *direct* esthetic response that I want. Listen appreciatively but discriminatingly. Whenever you find a passage you think especially beautiful, raise your hand and keep it up until the beauty begins to diminish. In other words, indicate what you consider 'high spots' in the composition. Be as prompt as you can in making these signals. I shall play each record three times, and each time you are to indicate what you consider to be 'high spots.' There is no obligation to indicate the same place each time, nor is there any *a priori* reason for not doing so. Just indicate what you honestly like especially well each time the composition is played.

A stop watch was started with the first note of the record and the periods of preference were noted. These could then be matched with like regions on the score,

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* Accepted for publication April 5, 1940.

[1] M. Schoen, E. L. Gatewood, M. F. Washburn, G. L. Dickinson et al., *The Effects of Music,* 1927, 121-130.

[2] I wish to express my thanks to Dr. C. W. Fox of the Eastman School for making available both laboratory and observers.

which had been previously marked in terms of seconds to correspond to the record. The purpose of playing each record three times was actually, though the *Os* were kept in ignorance of it, to enable a selection of high spots on the basis of their common occurrence in all three hearings. To determine high spots on the evidence furnished by three hearings is, of course, arbitrary, but the first hearing was included for its freshness, and the third ended the series so as to avoid fatigue and affective adaptation.

RESULTS

Are there, then, any regions in these compositions which qualify as 'high spots' for all *Os*? No region was universally chosen, but some regions rise decidedly above the average of 'popularity' for the whole composition. In all three compositions there are 14 regions found especially pleasant by 15 or more of the 30 *Os*. Here follows a brief analysis of these regions, counting, as the beginning of each region, that place just before its peak of popularity where the average of popularity of the composition is first exceeded, and, as the end of the region, that place after its peak where popularity falls again to average level.

BACH: The first region considered to be a high spot by 15 or more *Os* (by 18 at its peak of popularity) extends from measure 13 to 17. In these measures there is a return to the first part of the first theme, with considerable ornamentation.

The second such region (peak at 15 cases) extends from measure 21 to 24, and is a return of a later part of the first theme, ornamented.

The third region (peak at 20 cases) extending from measure 29 to 33 is a section leading to the most stressed cadence in the whole composition, regarded musically as a climax.

The fourth region (peak at 15 cases), measures 44 to 49, is a repetition of the third region, elaborated.

CHOPIN: The first region (peak at 20 cases), measures 9 to 19, may be described as contrasting with foregoing material by being more definite melodically and rhythmically, and therefore simpler to comprehend.

The second region (peak at 23 cases), measures 46 to 67, is well introduced by a few anticipatory measures, and involves a sophisticated and somewhat surprising change of key, together with a change of mood. It is a bright, simple theme, coming after somewhat diffuse material of indefinite mood.

The third region (peak at 19 cases), measures 78 to 91, is a very slightly ornamented repetition of the second region.

The fourth region (peak at 15 cases), measures 139 to 144, is the same as the first region, elaborated.

The fifth region (15 cases) is just an instant near the end, measure 192, where there is a reminiscence in the tonic of former material in the dominant.

BRAHMS INTERMEZZO: The first region (peak at 18 cases), measures 53 to 71, is new material, simple to grasp, contrasting with antecedent material in mode (major) and key.

The second region (peak at 15 cases), measure 92 to end of composition, is a repetition of the first theme, highly elaborated and intensified.

BRAHMS ROMANZE: The first region (peak at 17 cases), measures 10 to 13, is a repetition of the first theme, with changes.

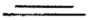
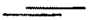
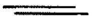
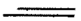
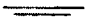
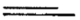
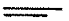
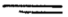
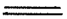
The second region (peak at 20 cases), measures 18 to 20, is the second theme, contrasting with the first in key and mood. It is marked *allegretto grazioso,* in contrast to the first which is marked *andante.*

The third region (peak at 18 cases), measures 50 to end of composition, is the second repetition of the first theme, with changes at the end.

Summing up these results, it appears that there in all are 14 regions especially liked by half or more of the *Os.* Nine of these 14 are cases of repetition of thematic material, with changes. Of these nine repetitive regions, five are repetitions of themes not considered especially pleasant when first heard; and four of them are repetitions of themes found highly pleasant the first time they occurred. These four themes, found especially pleasant on first hearing, show features of contrast with preceding material; one of them is merely simpler to grasp, while the other three show contrasts in key and mood as well. The mood of these three regions is in all cases brighter than the antecedent mood. Finally, one of the total 14 cases is a stressed cadence, a marked point of arrival, in other words, in this case a musical climax.

The data of the experiment also furnish facts concerning the manner in which pleasure increases with repeated hearings of the composition as a whole. Considering the first and third hearings, we find the following increase in average length of high spots: Bach, from 16 to 21 sec.; Chopin, from 23 to 25 sec.; Brahms (Intermezzo) from 22 to 32 sec. and (Romanze) from 25 to 30 sec. The average increase for all compositions is from 20 to 29 sec., the longer period in the third hearing being true for 24 of the 30 *Os.* Of the other six *Os,* three show equal periods in both hearings; two, a decrease of 2 sec. in the third hearing; and one, a decrease of 1 sec.

In a great many cases the greater extent of the high spots in the third hearing appears as a *spread about the initial focus.* (By initial focus is meant the corresponding high spot in the first hearing.) Altogether there are 300 such cases out of a total of 468 high spots in the third hearing, while only 52 new, isolated high spots appear. (The remaining 116 cases spread about foci in the second hearing, many of which themselves extend from foci in the first hearing.) A spread in both the anterior and posterior direction from the initial focus is the most common case. Also, anterior spread is more frequent than posterior. Following is a diagram of all the relations between corresponding high spots of the first and third hearings, together with the frequency of their occurrence.

1.		Bidirectional expansion	123 cases
2.		Anterior expansion, posterior contraction	102 cases
3.		Anterior contraction, posterior expansion	61 cases
4.		Anterior expansion, posterior equality	58 cases
5.		Bidirectional contraction	42 cases
6.		Anterior equality, posterior expansion	33 cases
7.		Anterior equality, posterior contraction	29 cases
8.		Anterior contraction, posterior equality	21 cases
9.		Equality	20 cases

From these figures it may be seen that the total number of cases of anterior expansion is 283 (items 1, 2 and 4); of posterior expansion, 217 (items 1, 3 and 6); of posterior contraction, 173 (items 2, 5 and 7); and of anterior contraction, 124 (items 3, 5 and 8).

There are a number of points here about which it is interesting to conjecture. First, why does the increase of pleasure in the third hearing of the composition appear very largely as spread about a focus? And second, why does anterior spread preponderate over posterior? Some of the spread may be due simply to the abandonment, with familiarity, of an initial cautious attitude, and the consequent greater readiness to acknowledge a region as highly pleasant. But this would not explain the greater frequency of the anterior spread, unless, indeed, one supposed that once the hand was up in the first hearing, there was caution also about lowering it, so that on the third trial when caution had been abandoned, the posterior spread did not show for so much. This supposition, however, does not take into consideration why so few new, isolated high spots appear in the third hearing. Another explanation, which does take this fact into account, suggests itself. Might it not be that learning to like a composition has features similar to those involved in an animal's learning to run a maze—that is to say, in the case of our experiment, an original high spot (corresponding to the maze goal) may be thereafter anticipated and a pleasure gradient extend backward? Thus, pleasure would actually spread from a focus, rather than appear *de novo.* Once the climax is reached, there would be relaxation of interest and some tendency for the pleasure to drop off. This would explain the relatively large number of posterior contractions. Posterior expansion, where it does occur, might be due to familiarity, or to hang-over from the climax.

CONCLUSIONS

(1) There is no region in any of the compositions studied especially liked by all *O*s, even though the *O*s belong to a fairly homogeneous musical group. The most popular region found involved 23 of the 30 *O*s.

(2) Counting as popular those regions especially liked by half or more of the *O*s, it is found that there was a total of 14 such regions, nine of which were repetitions, with changes, of previous themes. Four of the 14 contrasted with antecedent material, one chiefly in degree of simplicity (preferred region simpler), and three others, not only in the greater simplicity of the preferred part, but in greater brightness of mood, and in change of key. One region culminated in a highly stressed cadence, regarded musically as a climax.

(3) Regions of special pleasure are of longer duration in the third hearing of each composition than in the first.

(4) The greater duration of high spots in the third hearing appears as a spread about the initial focus.

(5) This spread is most frequently found at the anterior end of the focus.

(6) The explanation of this anterior characteristic of the spread may be that it is a case of a goal gradient, in which high spots are goals which extend their influence backward.[3]

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[3] M. F. Washburn, *The Animal Mind,* 1936, 373-381.

THE DEPENDENCE OF APPARENT VISUAL SIZE UPON ILLUMINATION

By ALFRED H. HOLWAY and EDWIN G. BORING, Harvard University

In the course of certain other experiments on the relation of apparent visual size to the distance of the perceived object[1] and on the apparent size of the moon,[2] we undertook to determine the dependence of apparent size upon illumination. We report the result here since we are not in a position at present to extend the investigation to a greater number of illuminations, distances, and *Os*.

O was seated at the right-angle junction of two long corridors in the Harvard Biological Laboratories. He sat so that he could look down either corridor by turning his head 45° to one side or the other. The experiments were made after midnight in corridors entirely dark except for the faint light furnished by the two stimuli.

The standard stimulus, placed remotely from *O* in the corridor at his right, was a projected circle of light, 53 cm. in diam., with an intensity of 2.4 ml. at a distance of 92 cm. This is the intensity for the rows labelled "a" in Table I, where the "relative illumination" is called 1.00. By introducing filters into the projection beam we could reduce this intensity to 0.10 and 0.01 of its original intensity ("b" and "c" in the table). The standard was first placed at 30.5 m. from *O*, a distance at which its diameter subtends an angle of 1.0°. Later the distance was doubled so that the subtended angle was 0.5°.

The comparison stimulus was placed in the corridor to *O*'s left at the same distance from *O* as the standard stimulus. It was a projected circle of light, of variable size and constant intensity. By altering electrical resistance in the circuit of the projector, the comparison intensity was equated to the intensity of the standard without filters. Its size was varied by running through the projector a strip of film on which was a series of 50 apertures, for the most part differing from each other in successive frames by just not noticeable differences.[3]

O looked back and forth between the standard and comparison stimuli. He called to *E* to adjust the size of the comparison stimulus until he was satisfied that the two stimuli were the same apparent size. Then *E* measured the actual size of the comparison stimulus. Since the two stimuli were equidistant, little difficulty was encountered in equating apparent sizes of the objects.

The results appear in Table I. Row "a" in both halves of the table and with all 3 *Os* shows that there was a small constant error in favor of the comparison stimulus, which, with distance and illumination both constant, was judged slightly larger than the standard stimulus (1.15, 1.05 and 1.02 for 1.00°; 0.55, 0.51 and 0.52 for 0.50°).

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* Accepted for publication March 30, 1940.

[1] A. H. Holway and E. G. Boring, Determinants of apparent visual size with distance variant, to be published later in this JOURNAL.

[2] Holway and Boring, The moon illusion and the angle of regard, this JOURNAL, 53, 1940, 109-116; and more especially the latter paper by us. The apparent size of the moon as a function of the angle of regard: further experiments, *supra*, 537-553.

[3] Holway and Boring, *op. cit.*, 110 f.

Each half of the table shows that dimming the standard stimulus decreases its apparent size, and that the decrease is greater for greater dimming. At a distance of 30.5 m. an illumination-ratio of 10 gives on the average a size-ratio of 1.12, and an illumination-ratio of 100, a size-ratio of 1.38. The corresponding average size-ratios for the stimuli at a distance of 61 m. are 1.15 and 1.60.

Schur found that the variation in the apparent size of artificial moons depends upon the distance of the moons from *O*, that there is very little variation at 3 m., and that maximal variation is reached at about 33 m.[4] The implication is that some of the determinants of apparent size are inoperative at great distances, so that the effect of other determinants might be more easily demonstrated. For this reason we chose at first to work at 30 m., expecting to find even greater differences in apparent size than we did find. Later we doubled the distance, but we are not prepared to assert on this evidence that the size-intensity phenomenon is more pronounced at 60 m. than at 30. It is true that no ratios at the longer distance are less than the corresponding ratios for the shorter distance, and the ratios for the longer distance are considerably larger for the illumination-ratio 100 than for the illumination-ratio 10. Nevertheless knowledge of the exact form of the general function, and in particular the answer to the question as to whether the effect diminishes at distances much less than 30 m., must depend upon further research.

TABLE I

DECREASE IN APPARENT VISUAL SIZE WITH DECREASE IN ILLUMINATION

θ = size (degrees) of comparison stimulus which matches standard = apparent size of the standard. Each θ is average of 5 observations. σ = standard deviation of distribution of θs. Illumination of comparison stimulus (and of standard stimulus = 1.0) is 2.4 ml. at 92 cm. The ratios, θ_a/θ_b and θ_a/θ_c, show the amount of change in the apparent diameter of the disk due to change of illumination.

Standard stimulus			E.G.B.		A.H.H.		L.M.H.	
Size and distance	Rel. illum.		θ	σ	θ	σ	θ	σ
1.0°	1.00	a	1.15	.045	1.05	.038	1.02	.032
at	0.10	b	1.06	.032	.90	.021	.93	.026
30.5 m.	0.01	c	.81	.032	.79	.028	.75	.029
		θ_a/θ_b	1.08		1.17		1.10	
		θ_a/θ_c	1.42		1.35		1.36	
0.5°	1.0	a	.55	.021	.51	.015	.52	.011
at	0.10	b	.51	.043	.42	.025	.45	.015
61.0 m.	0.01	c	.32	.011	.35	.026	.32	.011
		θ_a/θ_b	1.08		1.21		1.15	
		θ_a/θ_c	1.72		1.45		1.62	

We undertook this experiment originally in hopes that its results would throw light upon the nature of the moon illusion. The size-ratios for illumination-ratio 100 at 60 m. are of the order of the size-ratios in the moon illusion for 0° and 30°

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[4] E. Schur, Mondtäuschung und Sehgrosskonstanz, *Psychol. Forsch.*, 7, 1925, 44-80, esp. 53-57.

elevation of the eyes.[5] At present, however, it is not clear to us how raising the eyes 30° could diminish illumination at the retina to one per cent of the value that it has when the eyes are in the primary position.[6]

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[5] Our second paper (*supra,* 537-553) shows that the moon illusion is dependent only upon movements of the eyes and not upon movements of the neck and body, and that most of it occurs, therefore, when the head is fixed, in the first 30° of elevation of the eyes.

[6] A casual demonstration of the effect of intensity upon apparent size can be made by viewing the full moon binocularly and monocularly. Most persons will agree that the diameter of the moon in monocular vision is about 0.9 its diameter in binocular vision, and certain unpublished experiments by us show that the monocular reduction may be as great as 0.85 or even 0.8.

PHANTOM LIMBS *

By S. FELDMAN, Cornell University

So large was the number of limb-amputations in the World War—it was estimated at 60,000—that psychologists were called upon to help deal with the many problems growing out of this mass-mutilation.[1] These problems entailed, among other things, a study of the phantom or hallucinated limb that appears after amputation. For, strange as it may seem, this hallucination functions in a practical way. By inducing an apparent movement of the phantom arm the subject is able to innervate the biceps and triceps muscles remaining in the stump.[2] The phantom functions also in the control and appreciation of the movements of the artificial limb. At first unrelated, the two come together, arrive at spatial coincidence, and the lifeless appendage is animated by the living phantom.[3]

What is the basis of the phantom? Does it perhaps rest upon a confusion with the other, unamputated limb? No; for in cases of double amputation, where there is no possibility of confusion, the phantom still appears—a double phantom.[4] Nor does it suffice to refer to the fact that the parts of the body are represented in the brain in topographical detail, since this topographical representation is only one factor in the perception of the body and its parts, present or missing. In the case of missing parts of the body, we still do not know how and where perception is initiated.

I suggest *(1)* the paradox that it is the absence of the limb that is the cause of its perception, and *(2)* that the perception of the phantom is initiated by movements of the body—any movement of any part of the body may be responsible.

(1) The paradox. As a rule, we are not conspicuously aware of the several parts of the body. These generally obtrude themselves only when something goes wrong or something out of the ordinary happens to them. The stiff limb, the numb limb, calls attention to itself; the normal member does not. Just so it is the absent limb that makes a show of itself, not the limb that is still present. It is characteristic of the phantom limb that it appears of its own accord, spontaneously and involuntarily, and recurs so frequently as to cause annoyance to the subject; it haunts him. This insistence of the absent limb contrasts markedly with the inconspicuousness of its normal mate.[5]

The limbs behave, in this respect, as familiar objects generally do. The more permanent fixtures in one's surroundings—say, a picture hanging in its accustomed place on the wall—receive little or no attention until they are missing. An analogy

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[1] The best psychological account, a study of more than a 100 *Ss*, is by David Katz. (1) Psychologische Versuche mit Amputierten, *Zsch. f. Psychol.*, 85, 1920, 83-117. (2) Zur Psychologie des Amputierten und seiner Prothese, *Zsch. f. ange. Psychol.*, Beiheft 25, 1921, 1-118. (3) Psychologische Erfahrungen an Amputierten, *Bericht ü. d. VII Kongress f. exper. Psychol.*, 1922, 49-75 (includes a bibliography of 58 items). All further footnote references to Katz in this article are to (2).

[2] Katz, 49 f.

[3] Katz, 24.

[4] Katz., 42 f.

[5] Katz, 15.

that is, in some ways, even closer is the after-image of touch following the removal of one's hat or spectacles. A person may search for his spectacles while they are resting—unfelt—on the nose; the object is there, but it is not perceived. At another time he may find himself reaching up for his glasses after he has just taken them off; so insistent can the after-image illusion be.[6]

(2) The kinetics of the limb. How is the normal limb perceived? Light on this question comes from an experiment on the perception of lifted rods. This is not surprizing, since an object held in the hand functions as an extension of the limb itself.

How, then, do we perceive an object extending out from the hand? How, for example, does the fisherman feel the fishing rod? In the experiment, an *amputated rod*—that is to say, a mere handle—was placed in the subject's grasp. Then, by applying to the handle a down pull at one end and an up pull at the other, it was found possible to create a *phantom rod,* the illusion of a rod extending beyond the hand.[7] This experimental achievement demonstrates that the perception of the rod depends upon the rod's lever action, or rather upon the adjustment of body, arm, and hand to the lever action of the rod.

Now a limb is a balanced system of weights and levers. The upper arm, for example, is suspended from the shoulder, the forearm from the upper arm and also from the shoulder, the hand and fingers from all three. When an object is held in the hand, another element is added to the system. What has been shown to hold for the rod must, therefore, also hold for the limb: the perception of a limb depends upon the body's adjustment to the limb's lever action. If a limb continues to be perceived after it has been amputated, it means that an essential condition of its perception has survived: the body continues to adjust itself as if the lever actions formerly exerted by the arm and its several parts were still present. Amputation does not change the body's habitual adjustment.

For the direct initiation of the phantom limb we must hold movement primarily responsible. Whenever one moves any part of the body, a readjustment takes place involving all the limbs. As long as the readjustment proceeds without a hitch, the limbs remain unperceived. We become aware of them only when a hitch occurs. When an arm has been amputated, almost every movement that is attempted entails a hitch. Important in this connection is the fact that the arms are organs of emotional, and also of intellectual, expression.[8] We emote and think with our arms, some of us more than others. It is no wonder then that the phantom arm is described as recurring frequently and persistently.[9]

The phantom limb, so it is reported, is subject to change. The hand gradually clenches into a fist. It also gets smaller and moves closer to the stump. In its final stage it is described as a fist of child's size nestling *inside* the stump.[10] These changes are evidence of gradual adjustment to the lacking limb.[11] The body wakes up but slowly to the fact that a part of it is really missing.[12] In one case reported, the phantom was still present after 56 years![13]

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[6] For a detailed description of this phase of perception see Boring, Langfeld and Weld, *Introduction to Psychology,* 1939, 413 ff.

[7] L. B. Hoisington, On the non-visual perception of the length of lifted rods, this JOURNAL, 31, 1920. 136 ff.

[8] Katz (9 f.) deals with the intellectual and emotional disturbances that follow the loss of an arm.

[9] The fact that stimulation of the stump may affect the phantom is not inconsistent with our analysis.

[10] Katz, 21-24.

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[11] It it significant that the stump suffers a parallel apparent shrinkage (Katz, 58-63). In the rod experiment, different lengths of the illusory rod were obtained by varying lever action. Lever action is basic to the perception of body-parts and of objects in contact with the body, including tools and clothing (Hermann Lotze, *Mikrokosmus,* 2nd ed., 2, 1869, 202-212; H. C. Sanborn, The function of clothing and bodily adornment, this JOURNAL, 38, 1927, 1-20). The student who wears his or her hat at a tilt, the man who pushes his hat back when in an argument, gain thereby a feeling of added height and confidence.

[12] Our analysis probably applies also to the phantom limbs reported in cases of thalamic and other brain lesions. See Paul Schuster, Beiträge zur Pathologie des Thalamus Opticus, *Arch. f. Psychiat. u. Nervenkht,* 105, 1937, 358-622; 106, 1937, 1-233. The effect of a lesion, however, is variable; one has to reckon with progressive deterioration and also with occasional compensation. When the use of a limb is lost gradually, as through freezing or tuberculosis, no phantom appears (Alfred Gallinek, The phantom limb, *Amer. J. Psychiat.,* 96, 1939, 414).

[13] Katz, 17.

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF CORNELL UNIVERSITY

LXXXIX. General Practice in Mirror Tracing

By T. A. Ryan and Frances Schehr

The most important skills which we gain in everyday life are probably those which involve learning a labile mode of performance which readily changes to fit the needs of a new situation. Such skills are rarely learned by a step-wise process of learning first one performance for one specific situation, then another, and finally putting all of these specific 'responses' together into a composite which we call skilled performance. Instead, one learns many different aspects of tennis, writing, or typing all at once.

The present study deals with mirror tracing because it lends itself to the development of such labile performances and because it is a fairly simple and readily observed kind of activity. The general question is, what is learned when one repeatedly traces a given pattern while the visual pattern is inverted by a mirror? How much of the increase in speed is brought about by fixing the specific movements required for a given design, and how much by developing a general skill of coördinating movements with an inverted visual pattern?

The large number of experiments upon transfer of training from one bodily member to another indicate that there is an appreciable amount of this new, general coördination built up.[1] The fact that the transfer may go from hand to foot of the same side indicates that the transfer is not an incidental result of the bilateral symmetry of the body.[2] Because of the necessity of using the feet in Bray's experiment, however, the task had to be so simplified that it may have especially favored the development of a general "adjustment to the reversing effect of the mirror" which Bray suggests as one of the major determinants of transfer.[3] It may be that this adjustment is less evident when an *S* must trace a more or less complicated design. This might involve almost entirely the development of skills specific to the pattern traced. In such a case there might be less general reversal of movement with respect to the visual pattern.

The development of the same sort of skill involving reversal of customary relations between vision and motor activity is to be found in experiments like that of Stratton in which the visual stimulus is inverted or shifted for all objects over a long period of time.[4] Here practice is of the general, unsystematic kind typical of every-day learning, and the new adjustment seems to be non-specific. Wooster, in a study involving a small amount of lateral shift of the visual field by means of

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* Accepted for publication March 13, 1940.

[1] *E.g.* P. H. Ewert, Bilateral transfer in mirror-drawing, *Ped. Sem.*, 33, 1926, 235-249.

[2] C. W. Bray, Transfer of learning, *J. Exper. Psychol.*, 11, 1928, 443-467.

[3] *Op. cit.*, 460.

[4] G. M. Stratton, Vision without inversion of the retinal image, *Psychol. Bull.*, 4, 1897, 341-389, 463-481.

prismatic lenses, found a gradual reduction in errors of localization even without any perception of the accuracy of movements which the subject makes.[5] She explains this as involving an adjustment to the new position of the eyes in the head in forward orientation. In other words, it is a general change independent of the special tasks set for the subject. Something of the same general kind may be involved in mirror-tracing skills.

A study of more direct bearing upon our present problem is reported by N. Förster.[6] She was interested in finding the nature of the change in eye-hand coördination in mirror-tracing. Since there is transfer from one design to another in this task, she regards the change as a modification in the general eye-hand coördination, and wishes to show the relative rôles of kinesthesis and vision in this change. Subjects who had practiced the task were questioned about their methods of drawing after skill was developed. They reported that they finally came to 'forget the mirror' and that the performance was as natural as normal tracing. After the training period, the subjects were asked to draw the design from memory. She finds that the drawing is made in a reversed position *with the eyes closed.* This is taken as evidence that direction of movement of the arm was perceived visually rather than kinesthetically during the learning, so that *S* drew from memory in terms of the visually apprehended pattern even though the arms had to move in the opposite direction from that used during the training experiments. Kinesthetic guidance is therefore supposed to be of relatively slight importance. It would seem from this that the effect of practice was to teach *S* to ignore kinesthetic cues. This finding is worth checking, but our present problem attacks another aspect of the skill.

Problem. This is a study of the effect of a form of practice deliberately designed to favor the development of a labile and adaptible performance. Our problem was: does practice which consists of a mixture of trials upon several different designs favor the development of a general skill more than specific practice upon a single design? The generality of the skill is measured in terms of the amount of transfer-effect to unpracticed designs.

This formulation of the problem may suggest a resemblance to studies like that of Dashiell on the 'complete' versus the 'alternate' methods of learning two habits.[7] In learning two different mazes, or in practicing card-sorting according to two different systems the situation is quite different from that involved in learning to draw two or more designs in mirror tracing. It is doubtful if there is any general skill of maze-running or of card-sorting which runs through the two alternative sequences being learned. Certainly there is no general principle of organization between visual perception and motor performance of the sort involved in mirror tracing. The *Ss* in Dashiell's experiment were learning two conflicting habits or sequences.[8] Our *Ss* were learning the same general system of eye-hand coördination applied to slightly different concrete situations.

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[5] M. Wooster, Certain factors in the development of a new spatial coördination, *Psychol. Monogr.,* 22, 1923, (no. 146), 1-96.

[6] N. Förster, Die Wechselbeziehung zwischen Gesichts- und Tastsinn bei der Raumwahrnehmung, *Psychol. Forsch.,* 13, 1929, 64-78.

[7] J. F. Dashiell, A comparison of complete versus alternate methods of learning two habits, *Psychol. Rev.,* 27, 1920, 112-135.

[8] One pair of tasks used by Dashiell (horizontal adding and vertical adding by alternate numbers, *op. cit.,* 132) might be said to involve both common and conflicting features, so that it is difficult to say what part was played by increase of general adding skill and what by increase in skill in following the two separate orders of procedure.

This experiment grew out of certain observations made in an advanced undergraduate laboratory course at Cornell. Four students who had previously performed a standard experiment in mirror tracing with straight-line figures were given more complicated curved figures to trace. Four different figures were employed and practice was rotated from design to design so that each trial of a particular design was separated by trials of the other three designs.

The amount of improvement in speed of drawing was surprisingly low under these conditions of general or mixed practice. We found that normally, with ordinary uniform practice of a single design, performance was stabilized by the end of thirty trials, and the time was usually halved. With this rotated practice, however, the performance was by no means stabilized after a total of thirty trials, and the reduction in time was relatively small. The following Table I gives the results for the four *S*s, in terms of percentage of reduction in time, comparing the first eight and the last eight trials out of a total of thirty trials distributed over four designs:

TABLE I

Increase in Speed

(Thirty trials, rotated practice)

S	% reduction in time
H	22
I	19
M	14
S	14
Mean	17

In this series of 30 trials, any single design had been practiced, of course, only 7 times, so that if we consider mirror tracing as a process of learning to draw a specific figure, the above results are not surprising. It would appear, therefore, that there is very little general 'learning-to-trace-in-a-mirror' involved here. All of these *S*s had, however, performed another mirror tracing experiment a month or two before. Our first problem which arises from these results is, then, to carry out a similar procedure with completely unpracticed *S*s and using controls of the more common uniform practice.

In two of the above cases we had introduced a fifth design which was traced before and after the rotated or mixed practice. Records on this fifth design were used as a measure of the transfer effect of this mixed practice on a strange design. The rotated practice was carried further here so that a total of 60 trials, 15 of each of the four designs, came between the two practice periods of the control design. There is a hint here that the total reduction in time for any design is the result of general learning rather than specific skill for a particular design. In other words, the transfer effect shows quite a different picture from that derived from a study of the learning curves themselves. There is as much reduction for the unpracticed design as there is for the practiced patterns. The comparisons of the control and practice designs are found in Table II.

These results are to be regarded as no more than a hint, since the 2 *S*s had previously practiced mirror tracing, and since the transfer effect can be computed only on the basis of single trials before and after practice. The reliability is heightened, however, by the uniform progression of the learning curves in each case.

The second part of our main problem is derived from this hint. We wished to study the transfer effect of rotated practice as well as the learning curves. Control series showing the effect of homogeneous practice upon unpracticed designs are essential, if we are to assess the effectiveness of general practice.

The results to be reported below throw further light upon these two aspects of the problem of general learning in mirror tracing. Unfortunately, we cannot give any conclusive answers as yet. The time-consuming nature of the practice periods made it impossible to secure results on the large number of subjects which would be necessary to make the results completely reliable. Because the study was carried on by the junior author as an undergraduate research, limitations of time had to be considered. It is hoped that a further study will round out the picture.

TABLE II

REDUCTION IN TIME AFTER ROTATED PRACTICE

S	Transfer design	Practice designs				
		1	2	3	4	Av.
H	36%	46%	44%	49%	47%	46.5%
M	46%	34%	26%	25%	29%	28.5%
Mean	41%	40%	35%	37%	38%	37.5%

Procedure. Six *S*s, all unpracticed in mirror tracing, served in the main experiments. Practice periods of a half hour three days a week continued until the required number of trials had been reached.[9]

The tracing apparatus consisted of the usual horizontal drawing board with a vertical mirror in front of *S* and a screen over the hand. The designs to be traced were made with single lines. They consisted of curved and slanting lines so that almost the whole course of the drawing was more or less oblique with respect to the plane of the mirror.

S was given the following instruction:

> Take the pencil. When I say 'go' proceed to trace the figure. Take care to keep on the line until you have completed the figure: your pencil is not to leave the line; if you go off the line, retrace the part you have missed. Try to go as fast as possible, but make sure that you are accurate.

The *S*s were divided into two groups. The first group (3 *S*s) was given the rotated practice, tracing first one design and then another as described above. Four different practice designs were used. The second group (3 *S*s) practiced only one design. All *S*s traced the control or transfer design before and after this practice series. Thus we are able to compare the learning curves for rotated and uniform practice, and also the amounts of transfer to a new design resulting from the two kinds of practice. In addition to the 6 *S*s of the main groups, two other *S*s practiced a single design without any transfer test. These results are used in studying the rate of learning.

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[9] In two instances there were unavoidable interruptions in this schedule, but the method of computing improvement makes these interruptions relatively unimportant.

In the rotated practice series, all *Ss* were given a total of 100 trials, 25 for each design. At the end of this time, each of the 3 *Ss* was highly stabilized both in speed and accuracy. The remaining 5 *Ss* were each given 30 trials of a single figure, and then as many additional trials as were necessary for stabilization of the performance to become evident. Since, however, only slight changes occurred after 30 trials had been reached, only the first 30 will be considered in our study of the rate of learning.

Rate of learning. The comparison of rate of improvement under the two conditions involved is difficult because of the lack of a common standard. We shall therefore make the comparison on two different bases. (1) If improvement is due entirely to the fact that *S* is learning mirror-tracing-in-general, the rate of improvement should be the same whether the practice all involves the same design or whether it includes trials with a number of varied designs. Therefore the first comparison is in terms of rate of improvement per trial of practice, regarding all trials as equivalent whether the practice is mixed or uniform.

(2) If improvement is attributable entirely to specific skills for a specific pattern traced, mixed practice should be less effective than uniform practice in terms of the total number of trials of practice. In the extreme case, with no elements of skill common to the designs in mixed practice, improvement in drawing one design would be completely independent of the amount of practice on other designs interspersed during the training period. Thus 25 trials on a single design should bring as much improvement as 25 trials on the same design which are interspersed among 75 other trials devoted to three other designs. In keeping with this possibility, the records for each design involved in the rotated or mixed practice series were considered separately as isolated learning curves.

TABLE III

RATE OF LEARNING IN THE FIRST 30 TRIALS

(Percentage time-reduction, last eight *vs.* first eight trials.)

Rotated Practice		Uniform Practice	
S	% reduction	S	% reduction
1	35	4	49
2	40	5	51
3	16	6	44
		7	48
		8	52
Mean	30.3	Mean	48.8

Table III gives the analysis suggested by the first of these possibilities—that the practice improves only mirror-drawing-in-general. The table shows the percentage of reduction in time of the last eight out of thirty trials, compared with the first eight.[10] The eight trials grouped together in computing improvement during mixed practice involve, of course, two trials for each of four designs, while the first and last groups for uniform practice consist of eight trials of a single design.

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[10] All of the rates of learning are given in terms of reduction of time. Error records were taken, but the unreliability of counting errors involving deviations from a single line is very great. Also, it is certain that errors vary in degree, and the estimation of the degree of error is still more unreliable. Our instructions were so worded that *S* had to retrace any parts of the line he missed during an error. Therefore the time records reflect a composite of speed of movement plus additions to the total time due to errors.

If these results are borne out on further *S*s, they indicate that 30 trials distributed over several designs do not result in the same degree of improvement for any one design, as thirty trials concentrated upon the same design.

Indeed our second assumption seems to be more nearly borne out. If we consider the 25 trials for each design in the rotated practice in the same way we would an isolated practice series, there is no more improvement than there is after 25 trials on a single design with no other practice interspersed. Table IV shows the percentage reduction in time in 25 trials, comparing the first two with the last two in each group. The rotated practice was spread out over a longer interval because of the larger number of trials involved and also because of unavoidable interruptions in the schedule for *S*s 2 and 3. But, if anything, this should have increased the effects of rotated practice.

TABLE IV

REDUCTION IN TIME AFTER 25 TRIALS OF EACH DESIGN

(Percentage time-reduction, last 2 *vs.* first 2 trials.)

Rotated practice		Uniform practice	
S	% reduction	S	% reduction
1	56	4	66
2	63	5	71
3	52	6	46
		7	58
		8	55
Mean	57	Mean	59.2

Thus a total of 100 practice trials on four designs results in no more increase in speed for any one design than would result from practicing that design alone for 25 trials. Of course, *S*s 1, 2 and 3 have learned four designs while the remaining subjects have learned only one, but the four designs required four times as much practice.

Transfer of effect to unpracticed designs. Our preliminary findings had suggested the possibility that rotated practice might result in greater transfer to new designs than uniform practice. In the preliminary experiments we found as much reduction in time for unpracticed designs as for the designs which were actually practiced in rotation. The following results confirm this finding, but indicate that it is not to be attributed to the rotation of practice. Table V shows the percentage of change of time of the transfer design, compared to the percentage change of the practiced designs. In order to make the figures comparable, the reductions are all computed from the first and last trials.

There are a number of implications in these results. First, there is *more* transfer from uniform practice than there is from rotated practice, in spite of the fact that the uniform practice series involved only 30-40 trials altogether, while the rotated practice comprised 100 trials.

In both cases the improvement in the transfer design is greater than the improvement in the practiced designs. This would not be surprising if we regarded the performance as learning mirror-tracing in general. If this is what *S* is doing, the transfer design results could be placed upon the same learning curve as the practiced designs, and, since it comes at earlier and later parts of the curve, we should expect a greater percentage of change. Such an assumption would, however, contradict the earlier analysis of the rate of learning which seemed to show that the learning of various specific designs goes on independently.

Conclusions. While these results are apparently contradictory, it is possible to reconcile them. It must be remembered, however, that this suggestion is only provisional, pending further study designed to test the reliability of the results themselves. The indication seems to be that anyone interested in developing skill for mirror-tracing in general would find it most profitable to learn one specific pattern of performance well first. The generalization will then take care of itself. In other words, our attempt to develop a general mirror-tracing skill by a special system of rotated practice would seem to have been unprofitable.

TABLE V

TRANSFER EFFECT

Rotated practice			Uniform practice		
S	% reduction		S	% reduction	
	transfer	practiced		transfer	practiced
1	69	61.5	6	71	43
2	59	66	7	78	63
3	74	57	8	82	55
Mean	67.3	61.5	Mean	77	53.7

The explanation for this may be that shifts from one design to another confuse the issue by preventing the subject from disposing of certain non-essential but troublesome details involved in the specific designs. By repeatedly practicing the same design, peculiarities of the particular design may soon become unimportant so that the later part of the practice is devoted exclusively to mastering the general technique of mirror-tracing. When the designs are changed after each trial, the new special pattern may draw the emphasis away from the general technique. This would account for both the slowness of learning with rotated practice, and also for the fact that rotated practice is less effective in determining transfer than is uniform practice.

Even if further data substantiate these findings, we would still have several problems to be solved. For example, it is possible that other systems of practice involving more than one design might be more effective than uniform practice. In addition, there is the question whether similar relationships can be found for performances other than mirror-tracing.

These results are also significant from the theoretical side. A treatment of transfer in terms of identical elements runs counter to our findings. An individual who has learned four designs has certainly learned a great many more specific connections between perceived direction of line segment and direction of movement to be made. Therefore those who had engaged in rotated practice should, by chance alone, have learned more components of the unpracticed design used in testing transfer than those who had practiced only a single design. Yet the transfer under the two systems of practice is not significantly different.

It would seem much more plausible, therefore, to conceive of the change resulting from practice as involving a reorganization of the total system of relations between visual pattern and direction of movement. This system must be regarded as more or less unitary, rather than as a patchwork of elemental connections.

APPARATUS

AN INEXPENSIVE SLOW FREQUENCY OSCILLATOR FOR DRIVING ELECTROMAGNETIC MARKERS

By Roy M. Dorcus and George E. Mount,
University of California at Los Angeles

In many psychological experiments where either tape recording or photography is used for securing records, some type of device is employed for indicating the time intervals. Many kinds of such devices are available but almost all of them have some disadvantage in actual operation. The Jacquet clock, the Harvard clock, synchronous motor driven commutators and a number of oscillating circuits, each have some practical difficulty. In some cases, it is a question of contacts; in others, a matter of maintaining a constant voltage; and in still others, either a problem of cost or difficulty in securing machine shop facilities for construction. The timer proposed in the following diagram can be constructed from standard parts and can be assembled without any machine work. It is almost noiseless, has a wide range of time intervals, can be used on any 110 v. A.C. circuit, and requires no care once it is assembled. The time intervals are varied by means of fixed resistors R_1-R_2, R_a-R_b, R_c-R_d, R_e-R_f, R_g-R_h, R_i-R_j which can be thrown in or out of the circuit by switch 1 (S_1)—a six-point double pole non-shorting type (see Fig. 1). The following time intervals may be secured by use of the various resistors. The manufacturers ratings of the resistors and condensers may differ by as much as 10%

Explanation of symbols of Wiring Diagram

R_1-R_2 = 30 meg. Ω, ½ w., IRC
R_a-R_b = 10 meg. Ω, ½ w., IRC
R_c-R_d = 5 meg. Ω, ½ w., IRC
R_e-R_f = 1.25 meg. Ω, ½ w., IRC
R_g-R_h = 0.75 meg. Ω, ½ w., IRC
R_i-R_j = 0.35 meg. Ω, ½ w., IRC
R_3 = 2,500 Ω, 10 w., Ohmite
R_4 = 600 Ω, 10 w., Ohmite
R_5 = 500,000 Ω, ½ w., IRC
R_6 = 100,000 Ω, 1 w., IRC

R_7 = 500,000 Ω, ½ w., IRC
R_8 = 500 Ω, 2 w., IRC
R_9 = 50,000 Ω, 10 w., Ohmite
R_{10} = 10,000 Ω, 2 w., IRC
R_{11} = 10,000 Ω, 2 w., IRC
R_{12} = 10,000 Ω, 1 w., IRC
R_{13} = 40,000 Ω, 20 w., Ohmite
T_1 = R.C.A. 6 J 7
T_2 = R.C.A. 6 F 6
T_3 = R.C.A. 5 W 4

C_1 and C_2 = 0.03 μfd, Mica Aerovox
C_3, C_4 and C_5 = 8 μfd, Dry Electrolytic 450 v., Cornell Dubilier
C_6 = 1 μfd, Paper 450 v., Cornell Dubilier
C_7 = 4 μfd, Dry Electrolytic 450 v., Cornell Dubilier
C_8 = 16 μfd, Dry Electrolytic 450 v., Cornell Dubilier
Tr_1 = Inca C 61, 660 v., center tapped
L_1 = Inca D 1, 45 ma. choke
Relay = 10,000 Ω, 0.015 w., Leach
S_1 = two-gang, 6 pole non-shorting, Centralab

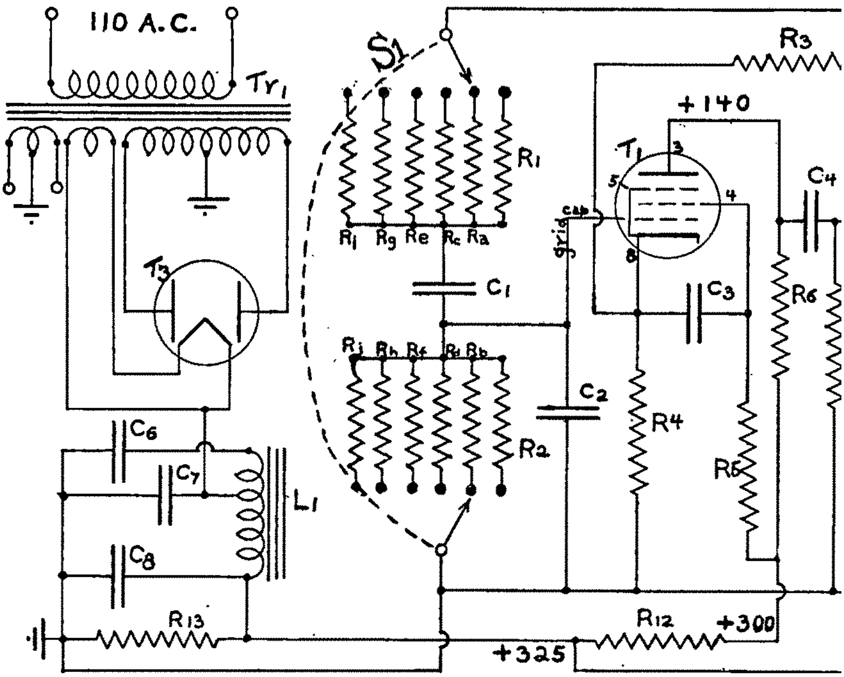

FIG. 1. WIRING-DIAGRAM OF OSCILLATOR
(For explanation of symbols see p. 600.)

which would necessitate some slight initial adjustment of the resistors to secure the exact times indicated.

Resistors	Oscillation rate
R_1-R_2	1 in 2 sec.
R_a-R_b	1 per sec.
R_c-R_d	2 per sec.
R_e-R_f	5 per sec.
R_g-R_h	10 per sec.
R_i-R_j	20 per sec.

Much slower rates of oscillation are not practical with this circuit since the capacity of the condensers and the magnitude of the resistors would have to be made disproportionately large. Faster rates are quite possible but most markers will not respond above 20 per sec., although there are a few on the market that will follow the 60~ current. We have arbitrarily adopted the above range since we feel that it is satisfactory for most purposes.

The circuit has been thoroughly checked and we find that the warming up effect, which is one of the difficulties with most of the slow frequency oscillating circuits does not exceed 1%. Neither line voltage fluctuations of as much as ± 10 v., nor a change in the number of cycles which would normally produce errors in a synchronous motor timer, will influence the accuracy of the timer. Apart from its accuracy and ease of assembly, one factor that should recommend the instrument, is its cost. The materials, not including the labor in assembly, will not exceed $20.00 which is only a fraction of the cost of most timers in general use.

A NEW APPARATUS FOR THE STUDY OF BIRDS' LEARNING

By Joseph E. Zerga, University of Southern California

Previous investigations of the learning-behavior of the bird in mazes have been restricted to patterns such as have been used with the rat, *e.g.* the horizontal multiple T-maze. Results of these investigations have shown the necessity of constructing a maze which will be suitable to the characteristic bodily structure and adaptive behavior of the organism concerned. Inasmuch as the bird is a perching animal, it is obvious that a reliable maze, if any maze may be said to be reliable, should contain perches.

The apparatus that is pictured and described here (see Fig. 1) was constructed especially for use with small birds; such as, the canary and sparrow. It is not only easy to construct but after it is made it may readily be changed from a maze to a multiple-choice box by removing the alley panels which are hooked onto the perches. The perches, which are 9 in. long and ⅜ in. in diameter, are inserted into ⅜ in. holes which are bored into the back of the box. Every perch is 3½ in. distant from the next (there being 85 perches in number) and is easily removable from its socket thereby making it possible to create problem designs with the perches themselves.

The apparatus, excluding the goal and entrance cages, which are depicted with dotted lines, is 9 in. deep, 4 ft. high, and 4 ft. wide. The goal or entrance cages are constructed with wire mesh and have a wooden bottom. Each cage is hung before an entrance or exit that is cut in the sides of the apparatus. The front of the apparatus is covered with a wire netting, enabling the experimenter to follow every movement of the bird from the entrance to the goal cage. The easiest method of timing the 'runs' is to use a stop-watch, but the timing may be done automatically by attaching the goal perches to an electrical clock.

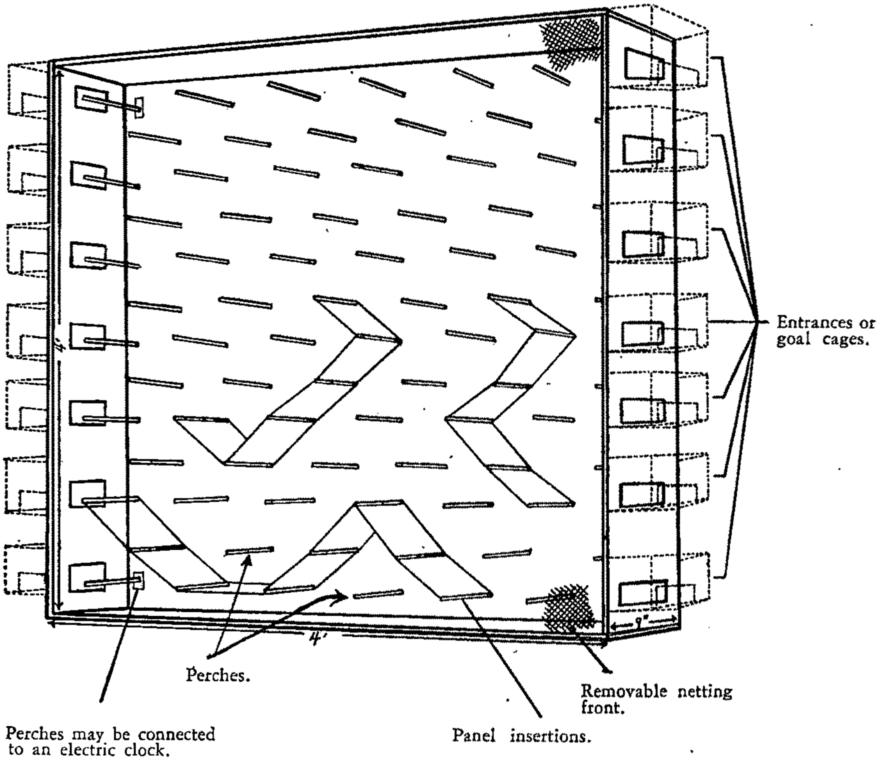

FIG. 1. FRONT VIEW OF THE APPARATUS FOR THE STUDY OF LEARNING IN BIRDS

NOTES AND DISCUSSIONS

PROBABILITY: A FUNCTION OF IDEOLOGY

Despite the vast amount of work in probability and predictability, the rôle of ideology, though an essential determinant in this field, has gone almost unrecognized. "The calculus of probability has received far more attention than its logic."[1] Ideology has come into the lime light with renewed emphasis in the last few years with regard to the experiments in extra-sensory perception, but its implications apparently have not been analyzed. Questionable generalizations in the field of chance and probability are applied wholesale to life situations with an astonishing complacency.

Chance of guessing correctly may, apart from considerations of information or psychic powers, vary, like other phenomena, with the individual. This theory does not seek a hearing merely as a criticism of an unfortunate statistical technique as used in the investigation of extra-sensory perception, but as a challenge to critical consideration in its own right.

To begin with, let us see how probability shifts with varying degrees of knowledge. Suppose that you read in the morning paper that a small ship, the *Sea Gull,* has been lost in a storm. Forty of the 50 souls aboard her have been drowned. You know that your friend, Mr. X, a retired business man, had booked passage on the *Sea Gull.* What is the probability that he was drowned? The probability is 40 in 50, or 4 in 5. What is the chance that he was saved?[2] The chance is 10 in 50, or 1 in 5. Information comes in slowly, but the next report states that 20 of the 40 persons drowned were members of the crew. The probability that Mr. X was drowned has now shifted to 20 in 30, or 2 in 3. The chance that he was saved is 10 in 30, or 1 in 3. Another bulletin comes in, reporting that 5 of the passengers drowned were women. The probabilities with reference to your friend have changed again. His chance for life is now 10 in 25 or 2 in 5, and the likelihood that he is lost is 15 in 25 or 3 in 5. At this point, information arrives from the ship's owners in a foreign land, stating that there were 20 passengers and 30 crew on board when the vessel sailed. Since 40 persons were drowned, 20 of whom were crew, all 20 of the passengers must have been drowned, including your friend. Then you learn that Mr. X had changed his mind, after all, and had remained at home. He is safe and well and at no time was there any possibility of his having been lost on the *Sea Gull.* Nevertheless, the *probabilities* shifted at different instants, even though there was only one *possibility* at any instant.

This is an example of the function of varying degrees of knowledge in determining the probabilities of the culmination of a given event. Probability may, then, be defined as the combination of knowledge and hypothesis between the limits of omniscience and nescience. There is no such thing as probability existing as an objective phenomenon in the universe. Possibilities exist objectively—independent of our thinking process, our degree of information, our ideology—but as soon as we introduce the concept of probability we refer to some state of knowledge or hypothesis. This being so, all probability is 'subjective' probability and may be different *and* valid for different persons with respect to the same event if based upon different knowledge or hypothesis.

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[1] J. M. Keynes, *A Treatise on Probability,* 1929, 21.

[2] The words 'chance' and 'probability,' as used in this paper, are exchange terms denoting the likelihood of the culmination of a given event.

To further illustrate the difference between *possibility* and *probability,* assume that a full deck of regular playing cards is thoroughly shuffed and placed face down on the table. No one has any knowledge of the order of the cards. What is the probability that the top card is the Ace of Spades? The probability is 1 in 52, since, so far as is known, any one of 52 cards *might* appear in the top position, of which only one could be the Ace of Spades. After the top card has been removed from the deck, but before anyone has looked at it, what is the probability that the *second* card is the Ace of Spades? The probability remains 1 in 52, since there are still 52 possibilities for that card *so far as is known.* This probability is constant throughout the deck. When the last card is reached the probability of drawing the Ace of Spades is still 1 in 52. There are still 52 possibilities for that card, *so far as is known,* although, as a matter of fact, drawing the Ace of Spades after a certain point may be *entirely impossible.* To anyone lacking the pertinent knowledge of possibility and impossibility, the *probability* remains constant.

If we *know* what is removed from the deck at each trial, and as a consequence, what remains in the deck at each trial, the probability shifts in accordance with this knowledge. If our knowledge remains constant, so does the probability, *for us.* If someone else has more or different knowledge, the probability *for him* will not be the same as the probability for us. This leads inevitably to the conclusion that probability is not the function of the empirical set-up, but of what is *known* or *hypothecated* about the empirical set-up.

In a preceding article,[3] the present author has attempted to demonstrate the operational significance of probability in an empirical situation involving continual shifts in likelihood of occurrence with reference to a series of symbols to be guessed. It was assumed that the *probability* of occurrence for a given symbol varied concomitantly with changes in the *possibility* of occurrence for that particular symbol. Returning, for purposes of illustration, to the set-up previously employed, let us consider some additional aspects of the situation as a necessary preliminary to an understanding of probability as a function of ideology.

The deck of ESP cards[4] consists of five symbols quintuplicated: Star, Circle, Cross, Oblong, and Wavy Lines. Imagine, once more, that the 25 cards are arranged before us in a pile, face down. Chance for a particular symbol to appear on the topmost card is obviously 5 in 25, or 1 in 5. However, each time a card is withdrawn from the deck there is a shift in probabilities *if we know which cards have been removed and which remain.* If we do *not* know which cards remain, probability of occurrence for any card remains a constant, since the possibility of occurrence for any card is also constant, *so far as is known.* This constancy of probability holds true for a person guessing the deck only so long as he responds like a machine, without the influence of memory, inference, or prejudice.

Now, let us suppose that, instead of asking some person to guess the deck, we employ an unbiased revolving disk. The disk is divided into five equal sections, each of which is arbitrarily selected to represent one of the five above mentioned symbols. On each 'guess' the disk is spun at random and a 'match' between a given card in the deck and the symbol indicated when the disk stops spinning constitutes a hit. The entire deck of 25 cards is 'guessed' just as with the human individual. It cannot be denied that *possibility* of *occurrence* for any symbol in the pack still shifts as cards are removed, but will the *possibility* of *correct matching* on the part of the disk be a function of this shifting possibility of occurrence in the pack? It will not!

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[3] E. A. Becknell, The function of dependent probability in ESP data, *J. Gen. Psychol.,* 19, 1938, 373-381

[4] Extra-sensory perception cards employed by Professor J. B. Rhine and his colleagues at Duke University in their investigation of telepathy and clairvoyance.

For instance, suppose that on the 20th trial there are no Stars left in the deck. Consequently, from that point on, it is impossible for a Star to appear in the deck. Suppose that the disk stops at a Star. What is the possibility of its matching the pack? Obviously, zero; but that is not the point. Rather, the point is what possibility the disk has of stopping at whatever symbol *does* appear at that position in the deck. We are interested in whether the disk *can* guess what is *in* the deck, not in whether it *will* guess what is *out* of the deck. No matter what is taken *out* of the deck, it is always equally possible for the disk to match what remains *in* the deck. This differentiation may seem hair splitting or irrelevant at first, but the questions are separate and distinct, and to confuse them is fatal to any real understanding of the problem. So long as the disk is guessing the cards and so long as one of the five cards represented on the disk remains in the pack, the disk has not only a constant *probability* at every point, but a constant *possibility* also. It cannot be otherwise.

However, in the case where one deck of 25 cards 'guesses' an identical deck of 25 cards, the *possibilities* do *not* remain constant. When one pack of cards guesses another pack, a card being removed from each pack after every trial, there may be *no possibility whatsoever* of matching after a particular symbol is used up in one deck but is still represented in the other. The *probability* is still constant so long as we do not know which cards are being removed. The *possibilities* of the two empirical situations may differ, yet the *probabilities* remain the same. This leads to the formulation of an hypothesis of probabilities at the same time quantitatively homogeneous and qualitatively heterogeneous. In other words, probabilities based upon different empirical set-ups might be numerically equal yet qualitatively unequal due to the presence of different *possibilities* in the empirical situations.

Just what significance differences in the possibilities of two set-ups may have in the face of the same probabilities, this writer is not at present prepared to state. It would be by no means a safe assumption that these differences in possibilities play no part in the end results in the presence of equal probabilities. A guessing situation in which the possibilities as well as the probabilities remain constant would seem to be more favorable, so far as correct responses are concerned, than one in which the possibilities shift unpredictably.

For this reason, any experiment in the guessing of one deck of cards by another, involving removal of cards from both decks, might, though it continued over thousands upon thousands of trials, prove decidedly dubious as a basis of comparison with the results of Rhine's students at Duke.[5] Even disregarding, for the moment, factors of memory, rational inference, and prejudice in the human guesser, the two configurations are at variance. Consequently, use of data drawn from thousands of matchings between such decks of cards as an indication of what the end results 'ought' to be for Rhine's subjects in a different set-up, might prove utterly meaningless.

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[5] J. B. Rhine, *Extra-Sensory Perception*, 1935; *New Frontiers of the Mind*, 1937.

On the other hand, suppose that a set of 5 cards, one of each symbol being represented, was matched against the deck of 25. One card from the set of 5 would be matched with one card from the deck of 25 at each trial. Whereas cards would be permanently withdrawn from the deck of 25, the set of 5 would be kept constant by replacing the withdrawn card after each trial, and shuffling all 5. In this case the essentials would be the same as for the disk situation and would approximate the set-up with the human guesser *if* prejudice, inference, memory, and other ideological factors were put aside.

Our problem would be far less bewildering if the human being responded exactly as the disk. In employing the disk we have, of course, a set-up of ideal simplicity. The disk, so far as we know, is not confronted with an ideology. Every card might just as well be present in the deck at each trial, so far as the disk is concerned. In other words, each 'guess' is made without bias and without regard for what may or may not be left in the deck at any particular point. The disk does not seek to remember its calls, employ rational inference, or outwit the pack in any manner. Each response is entirely independent of every other response. This is seldom true of the human subject. The person does not necessarily behave like a machine. Wherever we have a human individual we have also, presumably, an ideology. Hence, in a set-up such as Rhine's, probability becomes incomputable, since the subject may be responding to a series which *exists only in his imagination,* as well as to the actual empirical series.

This point has been touched upon in the preceding paper, but the following more important concept was not previously formulated: probability becomes incomputable, *not* by reason of the shift in possibility of occurrence, *but wholly by reason of the ideology.*

Here is how the situation stands: you will recall that in the case wherein the disk guesses the cards the possibility of occurrence with reference to the deck shifts as cards are removed. Possibility of correct matching on the part of the disk, however, remains constant. *Probability* of correct matching is also constant. In the case of the person guessing the cards the possibility of occurrence with reference to the deck shifts and the possibility of correct guessing remains constant, even as with the disk.

It is the conviction of the writer, however, that the probability of guessing correctly does not remain constant in the case of the person and that, since no explanation is available within the empirical set-up, it must be sought beyond—in the mind of the agent guessing. Since probability is a state of knowledge or hypothesis, and since knowledge and hypothesis are indisputably a part of our ideology, it can readily be seen that probability is a function of ideology. Differences in degree and kind of knowledge have been shown to change probabilities with reference to the culmination of a given event. But what about hypothesis? Just how can inference, prejudice, and memory of past hypotheses affect probability? When we hypothecate possibilities different from those which actually exist, and react in the light of these hypotheses as if they were reality, does this process affect our probabilities of correct prediction? Can two people with equal knowledge of the possibilities of the culmination of an event be said to have different probabilities of correct prediction with reference to the event because of differing hypotheses?

Take the case of John and Bill, for example. Both boys set out to guess the deck of ESP cards, having been shown the five symbols and informed that each symbol occurred five times, making a total of 25 cards. The deck was shuffled and Bill began to guess. He made a mental note of each response as he called it and was careful to call exactly five of each symbol. When he had guessed five Crosses, for instance, he would not guess Cross again. He happened to call all five Crosses in the first 15 guesses, and since three Crosses occurred in the deck in the last 10 cards, there was no *chance* of his scoring a hit on those three trials, although the *possibility* always existed. In other words, he was responding both in the light of actual knowledge and in the light of an hypothetical series which he employed as if it *were* actual knowledge.

John, on the other hand, guessing the same deck with the cards in the same order, had a preference for Stars, and, deciding that a Star could appear at one point in the pack just as well as another, he called "Star" on 10 of the 25 trials. This meant that on 5 of the 10 calls he *had* to be wrong. The probabilities for the two boys differed because they combined the same knowledge with different hypotheses, resulting in differing ideologies. Obviously, the more nearly the empirical series and the ideological series coincide, the more hits will result.

To use an extreme illustration, suppose that the deck had been described to Bill before he started guessing, but had not been shown to him. If he imagined the set-up to be a hoax and suspected the cards of being blanks, calling "Blank" 25 times as a consequence, his hypothesis would rather materially affect both the probabilities and the end results.

In the long run, the effects of such hypotheses may cancel one another, but such cancellation over a large number of trials does not deny the existence of the effects before they are cancelled, and by no means vitiates the theory of their existence. In fact, instances of freak or 'sport' runs, either of 'hits' or 'misses,' in guessing situations, might well find explanation in this light.

As a final example of probability as a function of knowledge and hypothesis, consider the following set-up. Blanks for a True-False test are distributed among students in a college class. One hundred spaces are provided on each blank for the recording of answers, but no questions are presented. As a matter of fact, no questions exist. The subjects are instructed merely to guess, writing either "True" or "False" in each of the 100 spaces. Suppose that a key for checking responses is so constructed that 100 items are True and none is False. The subjects are acquainted with the fact that all the items run one way, but the direction of the bias is not revealed. Hence, providing they react in the light of the information supplied them, each student will make a score of zero or of 100. Next day the experiment is carried on again. Precisely the same key is used as was used the day before, but the subjects are otherwise informed. They are told that this time the distribution of True and False items is 50-50. If the subjects do not suspect this piece of misinformation and allow themselves to be guided by a false hypothesis, every student will make a score of exactly 50, neither more nor less. The empirical key is the same on both occasions. In the first case there is a possibility that the ideological series will coincide with the empirical series; in the second case there is none.

Assume that the distribution is 80-20 and that the subjects are told that it is 80-20. Then their scores may fall anywhere between zero and 100. On the other hand, if they are told that the distribution is 50-50 and are again confronted with a false ideology, the range of scores will be 30-70. They will not make less than 30 nor more than 70. Such examples can be continued *ad infinitum* and can be worked out for any desired combination of distributions.

In conclusion, let us look for a moment at the questions of subjective and objective probability and the nature of uncertainty. Keynes[6] quotes the following statement of the philosopher, Hume: "Probability is of two kinds, either when the object is in itself uncertain, and to be determined by chance; or when, though the object be already certain, yet 'tis uncertain to our judgment."[7] Upon reading this declaration the question uppermost in the mind of your author was: just what sort of uncertainty can there be except that which is uncertain to our judgment? How can an object be "in itself uncertain?" What meaning can the term "uncertainty" have except with reference to our judgment? If we state that it is uncertain whether or not it will rain, we mean that it is uncertain *to us*, or to the weather man, or to someone else. Possibilities exist apart from us, but uncertainties and probabilities do not. Just as there would be no sound if there were no organ capable of hearing it, so there can be no uncertainty without mind capable of *being uncertain*. Uncertainty is not an objective phenomenon; uncertainty is a subjective experience. It is not the event which is uncertain; it is *we* who are uncertain *about* the event. If we knew all the facts there would be no uncertainty.

Perhaps that probability in which only knowledge is a factor might be considered more objective than that probability in which hypothesis is likewise a factor. Knowledge, with reference to many situations involving human beings, is a factor capable of control, but the hypotheses with reference to the same situations may be entirely beyond control. With a machine doing the guessing, however, we can eliminate the hypothesis angle from the field of the response. Hence, the machine situation might be considered an example of a more objective probability than one involving a human responder. This conception of two kinds of probability might partially satisfy Hume, although perhaps his differentiation may be better expressed in the phrases "probability before the fact" and "probability after the fact."

We have rambled a long way in this discussion, finding that truths are still elusive and significances nebulous. Just how much do we really know, after all, concerning what we are pleased to term 'chance' and 'probability'? Have we been so intent upon the ultimate smoothing out of distributions of 'chance' events that we have failed to understand the nature and significance of the single events composing the distributions? Have apparent simplicities blinded us to the real complexities? Without ideology probability is meaningless; it cannot exist. Likelihood of the culmination of a given event is only another name for what we know and what we think about the culmination of that event.

It becomes increasingly evident that before we venture to make comprehensive generalizations in this field, we should look a little more closely to the untraveled labyrinths of ideology.

Florida State College for Women ELIZABETH ANN BECKNELL

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[6] Keynes, *op. cit.*, 282.

[7] David Hume, *A Treatise on Human Nature*, Book ii, part iii, section ix.

The Stability of the *IQ*

It is evident that science does not go far without theory. There are times when it is truly remarkable how far it does go; at such times, however, the theory is present but unexpressed. That is to say, we act and design experiments as if we explicitly believed in a given theory. The current discussion about the constancy of the *IQ* is a case in point. The facts would gain a great deal in clarity with the aid of a little explicit theorizing. We have here a set of well-authenticated facts which prove that the *IQ* is not influenced by the environment. There is also another set of well-authenticated facts which demonstrate that the *IQ* is influenced by the environment. Everyone knows that 'facts' never lie; they do, however, have the distressing habit of contradicting each other unless they are embedded in a conceptual matrix.

Instead of attempting to construct the necessary matrix it happens only too often that we stubbornly deny the existence of the facts we do not wish to believe. At all events, we try to belittle them; the critical ratio is too small, or there are uncontrolled variables.

As regards the *IQ* it is difficult to understand what the 'hereditarians' have in mind when they contend that existing data does not clearly demonstrate a co-variation of environment and *IQ*. No data are needed other than the fact that if a person is coached on the test he is to take five minutes before the appointed time, his *IQ* is certain to be increased. It is to no purpose to say the test material does not fulfill its functions if the testee has had *direct* experience with it. For the test material also does not fulfill its function if the testee has had *no indirect* experience with it; if, for instance, he does not speak English, if he doesn't understand the word which is used, if he has never seen or heard of the object or event which is in question. Thus, if we ask a child a question involving apples, and he has never heard of apples, it is contended the question is an unfair one as a test of his native capacity; but then if we turn right around and ask him the same question shortly after giving him the correct answer, we still insist that it is an unfair test of his capacity! Surely there is something amiss here. How much experience must a person have with the object involved in a question, before the question tests his native ability? However we attempt to resolve this difficulty it is obvious that we are forced to conclude a person's answer to a question is always influenced by his environment.

It would seem that the whole difficulty arises from the failure to appreciate the fundamental fact that ultimately a psychological test measures *performance* and can never measure anything else. This fact, taken in conjunction with another equally fundamental fact, namely, that hardly any human performances are instinctive, leads inevitably to the conclusion that practically all performances must be influenced by the individual's past history, and hence that the scores in all psychological tests are inevitably influenced by the individual's past history.

The impulse at this point is to make the accusation of extreme 'environmentalism.' There can be no doubt, of course, of the ubiquitous influence of the environment upon performance; but to go from this to the proposition that an intelligence test score reflects only a person's environment is analogous to saying that the area of a rectangle is due to its length. To put the matter formally, the preceding considerations merely demonstrate that environment is a *necessary* factor determining the *IQ;* it is not yet demonstrated that the environment is also the *sufficient* factor.

The question, then, is not whether the *IQ* is or is not influenced by the environment. Rather, we must ask, "When may the *IQ* be taken as measure of the 'native ability' of the individual?" It would seem that many of the discussions on the *IQ* constancy beg the question; it is frequently assumed that the *IQ* measures 'native capacity' willy-nilly. If we are to answer the above question, however, it is evident that we must ask and give some sort of answer to a more fundamental question; namely, "What are the variables which determine the *IQ*?"

Fortunately, the question is not as formidable as it sounds. We approach it by assuming the most general case; namely, that an individual's mental age (*MA*), *i.e.* his performance, is dependent both upon his native ability (or heredity) and his past history (or environment). Utilizing the familiar symbols H and E for these two classes of variables, we write accordingly

$$MA = f_o\ (H, E) \ \ldots\ldots\ldots\ldots\ldots\ldots [1]$$

where the factor of chronological age (*CA*) is kept constant for all the subjects described by the equation. We do not need to know the exact nature of the relationship between these three variables; for the task which faces us here, the equation as it stands is sufficient.

The problem now resolves itself into the answering of the specific question: "Is equation [1] correct?" This may be determined by testing the two following deductions: (1) The *MA* of two persons having the same *CA* and the same E may not be identical; (2) the *MA* of two persons having the same *CA* and identical H's may not be identical. If both these deductions are correct, the assumption of equation [1] is justified, in which event the whole *IQ* constancy controversy is immediately shown to be trivial and out of focus, since it becomes analogous to a debate as to whether a force is uniquely determined by the acceleration imparted to an object.

Presumably the second deduction has been corroborated to everyone's satisfaction by the well-known instances of identical twins raised apart. Our problem, then, degenerates into the still more specific one of determining the correctness of the first deduction.

The testing of this proposition, however, is a peculiarly difficult task, and its difficulty has been exploited to its fullest extent by the extreme environmentalist who feels free to contend that it is never possible to state that two past histories are identical. This makes it possible for him to hold that ever discrepancy in *MA* (with *CA* fixed) reflects a discrepancy in environment. Even the 'constancy' of the *IQ* cannot be used as an argument against him, since all he needs to do is to point out that the constancy of the *IQ* in a given case merely reflects the 'constancy' of the environment. Hence if there be two persons whose *IQs* are *x* and *y*, respectively, and these remain *x* and *y* even for long periods of time, this constant discrepancy reflects a constant discrepancy in the environments of the two individuals.

All the critical ratios, correlation coefficients, and *t* values in the world are helpless against a fundamental logical obstacle of this sort, since they are completely irrelevant. It is plain, however, that the 'insuperability' of the environmentalist's position is its greatest weakness. As it stands, it becomes dangerously near to that class of hypothesis which asserts that if one had sufficient faith he could defy the force of gravity. The complete uselessness of the hypothesis lies, of course, in the undefined word *sufficient,* since its lack of delimitation always permits one to explain a given failure by asserting that the faith was not sufficient in that case.

In order, therefore, to break the vicious circle it is necessary to define the phrase *equality* or *equivalence of environment.* Once a definition of this is given, Deduction (1) becomes verifiable, since an unambiguous answer is available through the ordinary channels of experimentation.

With this end in view, the following definition of the equivalence of two environments is proposed: Two environments E_1 and E_2 are equivalent with respect to a given test when a pair of identical twins raised in E_1 and E_2, respectively, give the same *MA* on the test in question. We may, indeed, formulate a more generalized definition of what might be called *Environmental Distance* of which the above definition is a special case.

The distance between two environments E_1 and E_2 is equal to the difference in the *MA*s of a pair of identical twins raised in E_1 and E_2, respectively, when the lesser of the two *MA*s is equal to *j* (*j* being an arbitrary value). It will be noted that while the second of the two definitions appeals by virtue of its greater generality and usefulness, it becomes necessary to 'fix' the *CA* and one of the *MA*s in order to make the definition unambiguous.

It now becomes possible to make definite experimental tests of Deduction (1); namely, that the *MA*s of two persons having equivalent Es and the same *CA* may not be identical. The only consideration remaining is that of satisfying oneself that the subjects of the experiment fulfill the specification of the equivalence of E. If this is done, then we merely need a group of children of identical ages which are brought up in equivalent Es. The above proposition is corroborated if the standard deviation of the *MA*s of the group is greater than zero. In this event, it is demonstrated that the *MA* reflects native ability under conditions of equivalent environment and equation [1] is justified. If, on the other hand, the standard deviation is not significantly greater than zero, we must conclude that *MA* reflects environment alone and that equation [1] is not true. We should then be forced to write

$$MA = f_1(E) \ldots\ldots\ldots\ldots\ldots\ldots\ldots [2]$$

and the environmentalists would be complete masters of the field.

We are now in a position to consider the matter of the 'constancy' of the *IQ* directly. From actual studies we know that the *IQ* is not too constant, since correlations between *IQ*s which have been obtained at intervals of more than three or four years are seldom over +0.80. It is customary to attribute this to 'errors of measurement' and to 'uncontrolled variables,' but this is not desirable until we have done everything possible to straighten out the matter conceptually.

In discussing the question as to whether the *MA* could be considered as reflecting H or E or both, it was possible to keep *CA* constant, and hence the function for *MA* took on the simple form given in equation [1]. In considering the question of the *IQ* constancy, however, this is obviously not possible, since we have to deal here with what happens to the *MA* of the individual with an increase in age. In this instance we must take into account the factor of maturation, and hence we must alter equation [1] accordingly. We might, therefore, be tempted to write

$$MA = f_2(H, E, M) \ldots\ldots\ldots\ldots\ldots\ldots [3]$$

where M represents the maturation factor.

It is, however, desirable to examine variables rather sharply, since equations, even when expressed quite formally, as in this case, require something more than the mere writing down of letters within parentheses. It is clear that the factor of maturation, stripped of all its barnacles, implies nothing more than a change of something with a change in time. Hence we should substitute the dimension T for the dimension M. By so doing, we say in effect, that when H and E are held constant, a change in the value of T is accompanied by a change in the value of *MA*. If maturation is to mean anything as a separate variable, it ought to mean this. We shall write, therefore,

$$MA = f_3 (H, E, CA) \ldots\ldots\ldots\ldots\ldots [4]$$

if it is assumed that heredity as well as environment is a factor in the determination of *MA*. Utilizing all of the considerations which have preceded, we may now define the *IQ* in the following manner,

$$IQ \equiv MA/CA = f_3 (H, E, CA)/CA \ldots\ldots [5]$$

We are in a position now to ask a very instructive question: "How is it possible for the *IQ* to stay constant at all?" We may answer this question quite simply if we are willing to make an assumption; namely, that the H for a given individual does not change during the course of his lifetime. This may seem a rather drastic proposition, but it is a common-place fixture of genetic theory. If H is assumed to be constant, it is evident from equation [5] that the *IQ* for any given individual remains constant only when E changes in time by an amount such that the ratio of the numerator to the denominator maintains a constant value.

This may be expressed in different language by saying that equation [5] implies what might be called an 'Environment Schedule' for the individual if the condition of an *IQ* constancy is to be maintained. We thus see that the *IQ*, far from being uninfluenced by the environment, requires, on the contrary, that the environment change at a definite rate if it is to stay constant at all.[1] The corroboration of such a view is to be found in such work as that of Gordon with Canal Boat children. The corollary of the above is also evident, namely, that if the 'Environment Schedule' is exceeded, the *IQ* will fluctuate upward; if it falls short, it will fluctuate downward.

In light of these considerations, it is not surprising that the correlation coefficients between *IQ*s obtained at different intervals are not higher than those reported. On the other hand, the very process of evaluating the items in a Mental Age Scale and the procedure of standardization itself guarantees a rough constancy of *IQ* unless the individual's environment suffers a marked dislocation. Consequently, if an individual's 'Environmental Schedule' does not fall outside the limits of those characterizing the persons forming the group upon which the test was originally standardized, we can expect that there will be no dramatic alteration in his *IQ*. As in the case of the *MA*, however, it is clear that the identity of *IQ* in two persons can be taken as an indication of equivalence in their native ability (or H) only in so far as their environments may be said to be 'equivalent.'

It would seem to be indicated by the foregoing, that the current debate in regard to the stability of the *IQ* suffers from a lack of sufficient conceptual dissection. Even a superficial analysis disposes one to feel that statements like "The *IQ* reflects an individual's native ability" or "The *IQ* reflects an individual's environment" are in the nature of "incomplete sentences" and come very close to repeating the old squabble as to whether it is "Heredity or environment that determines human nature."

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[1] In the event $f_3 (H, E, CA) \equiv (H)\ (E)\ (CA)$, then the *IQ* remains constant only when the environment does not change its value.

University of California at Los Angeles J. A. GENGERELLI

A CAUTION ON THE USE OF THE METHOD OF CORRECT MATCHINGS

The mathematics of the procedure by which two sequences of items are paired for correct matchings has been obtained and quite adequately presented by Chapman.[1] In order to inform the reader of the general nature and use of the correct matching method the following paragraph is quoted from Chapman:

> A method common to many experimental and testing procedures in psychology and education is to require an individual to match, as best he can, members of one series of items with members of a second series of quite different items certain of which are in some sense true apposites of items in the first series. Thus the experimental psychology of personality has often investigated the ability of graphologists or laymen to pair samples of handwriting produced by a group of persons with, say, character-sketches of these same persons; and the excess of correct matchings thus produced over the number to be expected by chance has been used as evidence that the expressive movement of handwriting affords characteristics diagnostic of personal traits. Fortunately, the excesses experimentally obtained have often been so large as obviously to exclude the operation of chance alone. But many empirical results show small excesses only; and the interpretation of such findings has not hitherto been subjected to rigid statistical analysis.[2]

In their book on *Studies in Expressive Movement* Allport and Vernon remark that "it is not easy to eliminate all the spurious elements which make for correct matchings."[3] But Chapman does not point out the possibility of these spurious elements in the application of his theory noted by Allport and Vernon and to which his examples and those of Powers are subject.[4] The particular use of the method to which I would give caution is that of Powers and Chapman in using multiple judgments on the same data as a means of obtaining an indication of some functional relationship other than chance. The use of many judges is evidently intended to give statistical significance through numbers of cases in a matching situation where the effects of chance cancel out sufficiently while retaining the possibly slight true functional effect. But is this necessarily or even probably achieved? Let us note briefly the mathematical situation.

The mathematics which Chapman derives pertains to two identical packs of distinct cards randomly matched. Let such decks be thus matched N times. He obtains the moments of the mean number S of correct matchings per time. Of these the mean is 1, and the standard deviation of S is $1/N^{1/2}$ which can be made as small as desired by choosing a sufficiently large N (the number of judges in the applications). Then utilizing a Type III approximation he obtains the probability that the average would have been as large as S.

Now suppose that N judges take the same two sequences of items and attempt to pair similarities. To consider an extreme case suppose there is one accidental similarity occurring at a correct or matched pair, there being no other such similarities. Then, the judges observing it as they are supposed to, the expected average is now 2 instead of 1. The observed mean will then obviously be significantly different from 1 with even a small N. The small probability obtained in this case, while a reliable measure of the reactions of the judges collectively to properly paired stimuli, cannot be said to be indicative necessarily of anything but the one chance similarity, which might itself have a high probability of occurrence.

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[1] D. W. Chapman, The generalized problem of correct matchings, *Ann. Math. Stat.*, 6, 1935, 85; The statistics of the method of correct matchings, this JOURNAL, 46, 1934, 287ff.; The significance of matchings with unequal series, *ibid.*, 48, 1936, 167ff.

[2] Chapman, *op. cit.*, *Ann. Math. Stat.*, 85.

[3] G. W. Allport and P. E. Vernon, *Studies in Expressive Movement*, 1933, 202.

[4] *Op. cit.*, 212.

In two arbitrary sequences of items to be paired (correct pairings pre-determined) there usually exist pairs (either matching or non-matching) which several of the judges would likely adjudge as similar, in particular if the basis for judging similarity is a rational one. Thus a great variety of possibilities exists in which it is not correct to assume that the chance mean is 1. It might be greater and it might be less than 1.

A use of the method to diminish the effect of the random similarities is to increase the number of sets of data and to allow effectively (as far as the evaluation is concerned) only one judgment to each set of data. The standard deviation of each set is then 1 instead of $1/N^{1/2}$. The several *sets* of data could then be evaluated properly using Chapman's method wherein N equals the number of sets of data instead of the number of judges. The resultant probability would then be a measure of the aforementioned functional connection.

The use of many judges to each set of data would furnish a reliable measure of the number of correct judgment matchings which the two sequences of items to be paired exhibit, be they chance or caused by a true functional relationship. This estimation which is an average number of correct judgments (the estimation is performed on each set of data) will usually be non-integral, but since a Type III curve is used in the final evaluation, interpolating S (which is then the average of all the above averages) would appear to be quite in order. The necessary use of 1 as a standard deviation for each set of data regardless of the number of judgments upon it is a crude though safe value but may nullify the utility of the method.

University of North Carolina JOSEPH A. GREENWOOD

THE FIFTEENTH ANNUAL MEETING OF THE MIDWESTERN PSYCHOLOGICAL ASSOCIATION

The Midwestern Psychological Association held its fifteenth annual meeting at the University of Chicago, Friday and Saturday, May 3 and 4, 1940. The official register was signed by 565 members and visitors.

At the annual business meeting on Friday afternoon, 82 new members were elected, bringing the present membership list to 517. The election of officers was announced with E. K. Culler, University of Rochester, President for the year 1940-1941; members of the Council, H. F. Harlow, University of Wisconsin, 1940-1943, and A. W. Brown, Institute for Juvenile Research, Chicago, 1940-1941, to fill out the unexpired term of E. K. Culler. It was announced that the next annual meeting of the Association will be held at Ohio University late in April or the first week of May, the exact date to be announced later.

Eleven sessions were held at which 82 scheduled papers were read. The sessions included those on general psychology, electrophysiology, animal learning, mental tests, attitudes, sensory and motor phenomena, clinical psychology, psychometrics, human learning, genetic and comparative psychology, and personality. In addition, nine symposia were conducted on conflict and adjustment, analysis of variance, current behavior theory, professional problems in psychology, contemporary social problems, the first course in psychology, motivation and tension, mental hygiene, and a summary of laboratory reports.

At the annual banquet held on Saturday evening, E. S. Conklin presided. Vice President Emery T. Filbey of the University of Chicago, gave the address of welcome. J. P. Guilford gave his presidential address on "Human Abilities," a critical review of recent experimental and statistical findings.

A resolution passed at the business meeting requested the Secretary to thank the University of Chicago, the host department, and members of the local committee headed by Professor Forrest A. Kingsbury for the excellent facilities and entertainment provided in these meetings, which set a new record for attendance and new members.

Northwestern University ROBERT H. SEASHORE

THE LOS ANGELES MEETING OF THE WESTERN PSYCHOLOGICAL ASSOCIATION

The 1940 meetings of the Western Psychological Association were held June 14 and 15 in the new Psychology Building at the University of California at Los Angeles. The new building, with its 73 rooms devoted to psychology, was to be formally celebrated on June 13 by appropriate ceremonies. On this occasion R. B. Loucks talked on physiological psychology, E. R. Hilgard on experimental psychology, Milton Metfessel on criminal psychology, and G. M. Stratton on the social psychology of war.

Thirty-seven papers were presented during the meetings in addition to the address of retiring President Howard R. Taylor, who spoke on "Dimensions of Scholastic Aptitude," and a symposium on the topic "Should Departments of Psychology Have a Service Function in the College Personnel and Mental Hygiene Program?"

At the business meeting, a resolution was tabled which requested that the Association seek to restrict in California the use of the term "psychologist" in the classified directory of telephone books to persons certified by heads of departments of psychology in universities. A resolution was passed calling for the support of the "American Standard" Work and Assistance Act, H.R. 8615, having to do with relief appropriations for professional workers. A motion was made by Knight Dunlap that a committee be appointed to investigate what functions psychologists should fulfill in national military organizations in times of peace and war in the interests of national defense. After considerable discussion, it passed with the amendment that the task had also to do with the preservation of peace, and with the understanding that this act did not assume that our participation in the present war is inevitable. The motion also authorized the committee to advise, consult, and coöperate with the American Psychological Association, the National Research Council, and other organizations as may be necessary for the development and effectuation of plans. The committee includes: K. Dunlap, E. R. Hilgard, and M. Metfessel, Chairman.

Officers elected for the coming year were: President, Mary B. Eyre, Scripps College; Vice-president, Roger B. Loucks, University of Washington. The Association is scheduled to meet at Berkeley, next year, at the University of California.

University of Washington RALPH H. GUNDLACH

William Frederick Book: 1873-1940

William Frederick Book died May 22, 1940, at his winter home in Long Beach, California. He was born in southwestern Indiana near Princeton, on June 10, 1873. His experience as a teacher in the public schools prior to his study at Indiana University, from which he graduated in 1900, influenced him to become a student of educational psychology, the field of work in which his contributions are well known.

After studying at the University of Chicago, he entered Clark University in 1903, and was awarded the Ph. D. degree by that institution in 1906. In that same year he was appointed Head of the Philosophy Department at the University of Montana and established soon thereafter the psychological laboratory in that institution. He left there in 1912 to return to Indiana University as Professor of Educational Psychology. In the period 1913 to 1916, while on special leave of absence, he organized under the State Board of Education the department of Vocational Education in Indiana. From 1917 until being forced to retire in 1934 because of poor health, he served as Head of the Department of Psychology at Indiana University. He was co-editor of the Journal of Applied Psychology from 1920 through 1927.

His best known contributions were in the field of human learning, particularly the study of the development of skills represented by his now classical studies in learning to typewrite. The results of his early work in the analysis of the learning curves which were derived from these studies have had broad application to educational psychology in general.

Although intensely interested in the theoretical aspects of the psychology of learning, his activities were directed since early in his professional career towards the very practical matter of developing skills in the individual. He was keenly appreciative of the significance of basic individual differences as they are usually considered, but at the same time he was emphatic in the point of view that, for the great majority of persons, the differences in levels of efficiency and adjustment are to be accounted for primarily in terms of their skills. This point of view he applied not only to performance in the basic school subjects but also to such broad areas as those which he termed personal efficiency in the day's work—areas to which, as his publications clearly indicate, he gave considerable attention during the later years of his professional activity.

Under his direction the laboratory at Indiana University made definite contributions to the fields of mental testing and clinical psychology. Over a period of years he was an influential member of several important committees concerned with educational and social programs. Although his goal was never quite achieved, he worked assiduously for the establishment of a Child Welfare Research Station at the University. The establishment of the Psychological Clinic in the James Whitcomb Riley Hospital for Children, Indianapolis, in 1925, however, was a prized achievement of his administration.

Indiana University

EDGAR L. YEAGER

BOOK REVIEWS

Edited by JOHN G. JENKINS, University of Maryland

General Psychology. By L. E. COLE. New York, McGraw-Hill, 1939. Pp. 12-688.

If a psychologist writes a textbook nowadays, he should address it to some well defined class of students. This book seems to be intended for juniors, who have at least a sound cultural acquaintance with the natural sciences and with the elementary mathematics which they presuppose. The author obviously strives to employ the laws of physical causation wherever he can. I personally question whether the facts which interest most psychologists are best described in accordance with these causal laws; but if anyone feels bound to try to use them, to the neglect of laws of mnemic telic causation, this book is written for him.

Cole's background is physiological; and most of his book is written from the viewpoint of an observer of other persons. He does not, however, choose a single set of postulates, such as those of a consistent behaviorism, and try to describe everything according to the rules which they determine. In this, I think he has done wisely, although one might wish that he had specified his rules of description and mentioned some alternatives.

Some 144 pages, comprising about one-fifth of the book, are devoted to certain problems in nerve-physiology in relation to behavior and conscious experience, and to a description of the structure and functions of receptors, conductors, and effectors. Pre-medical students, fresh from courses in comparative anatomy and physiology, probably will like this treatment.

Eventually, we may all ask what light physiological procedures and results can throw on human actions and conduct, except on a few simple reflexes. It is, of course, mentally stimulating to think of a person's behavior and mental content as expressions of some hypothetical temporal pattern of receptor-conductor-effector activity; but unfortunately there are no feasible operations that enable us to test the hypotheses. One, therefore, wonders whether the hypotheses, after all, are fertile. Meanwhile, we should ask whether the author has presented a set of accepted facts of observation consistent with these rules. Usually, at least, he seems to have done so.

The first chapter purports to be a history of brain physiology in relation to "doctrines of animism." The material seems to be derived principally from sources of the second order and higher. The chapter does not ornament the rest of the book. In it, and thereafter, Cole often asks where the mind, consciousness, thinking, emotion, personality, etc., "reside." The true answer, as he occasionally suggests, is "nowhere." I wish that he had pointed out, once and for all, that *relationships* (such as these) do not *exist,* anywhere or at any time; that they simply *are,* and that they are merely exemplified in those particular instances in which they appear as connectors of particular terms. When one talks, as some neurologists do, of functions "residing" in some bodily locus, one is reifying and personifying functions. For example, one neurologist recently suggested that *because* certain animals can be retrained in certain acts of skill more readily than when they were originally trained, provided certain regions of the cortex remained intact but not if the same regions were surgically destroyed; *therefore*, the "seat of retention" of these acts lay in these specific regions. One critic pointed out that this is only another way of asserting that an if-then relation connects the integrity of the brain structure to the persistence of retention. He compared it to an incident in the apocryphal history of the boyhood of Sir Walter Scott. Young Walter, who was then a prig, desired to stand at the head of his class, but another boy always surpassed him in rote-recitation. But young Walter, noticing that this boy always fingered a certain button on his waistcoat while he recited, seized the opportunity to cut off the button one morning while the boy was saying his prayers. When the boy next stood up to recite, he felt for the button and missing it, failed ignominiously in his recitation, allowing young Walter to move up to the head of the class. "Now," asked the critic, "according to the neurologist's rule of description, must one not say that the boy's memory 'resided' in the button? When he has it, he remembers again; when he loses it, he fails; when he recovers it, he remembers again. Since his memory comes and goes with the button as the rat's memory remained or vanished with the cortical patch, are not the two instances perfectly analogous?"

Cole presents sensation, not as a separate topic, but incidentally in the chapter on receptors and their functions. This chapter is well done except for a few outstanding inaccuracies. For example: the mechanism of accommodation is described in Helmholtzian terms and not according to modern fashion; the millimicron is mistakenly denoted by the symbol $\mu\mu$ instead of $m\mu$; it is asserted (p. 126) that if two complementary lights are mixed in suitable proportions, their characteristics of hue are mutually cancelled, while the total brightness of the mixture lies between the brightnesses of the components. The fact is that the two brightnesses summate according to the laws of arithmetic addition, except for distortions which characterize Purkinje's effect.

The chapter on development classifies the nature-nurture and heredity-environment problems as spurious. The one on motivation includes a discussion of bio-energetics; the telic characteristics of motivation; the rôle of socialization; attention, including postural sets and preparation; Adlerian compensation; and volitional resolution of conflicting tendencies. For a 'free' will, Cole has no use; but he does not tell us what 'freedom' is.

The chapter on learning is 70 pages in length. Cole would like to explain learning in terms of physical changes in the organism resulting from prior stimulation and responses, but he admits (pp. 373, 440) that this cannot now be done. He exhibits and condemns the trial-and-error hypothesis, a Gestalt hypothesis, and a conditioned reflex hypothesis finding them all unsatisfactory. He does not consider the hypothesis that one skill can be learned by practicing others.

In the chapter on thinking, which includes memory, hypnosis, rationalization, etc., Cole devotes 25 pages to Freud's proclamations. I think he appraises them justly; in other words, he has no use for them. Again in this chapter, he asks where is the locus of thought.

His chapter on emotion is chiefly a description of the autonomic nervous system, smooth muscles and glands and the functions which physiologists now tentatively assign to them. He does not care for James, Lange, or their theories.

Fig. 76 (p. 458) presents a map of the teleo-stereoscope showing a path which light has never been known to take. Like many other authors, he borrows this diagram from Titchener without examining it.

The chapter on intelligence indicates many important discernments; but it does not mention the very strong evidence against constancy of the *IQ*. The chapter on personality is disappointing in that it assembles the results of many sham experiments uncritically. Examples are found in the treatment of Jung's, Kretschmer's, Heidbreder's, and D. A. Laird's 'types.'

For the class of students which I mentioned at the beginning, the book is likely to be useful. The style is for the most part clear and unembellished. The format is excellent; the index fair.

Tulane University H. M. JOHNSON

L'hygiène mentale et l'education. By L. M. RÉGIS, A. BARBEAU, E. C. WEBSTER, NOËL MAILLOUX, A. G. BILLS, A. MARCOTTE and J. A. LONG. Ottawa, Canada, Les Éditions Du Lévrier-College Philosophique et Théologique Des Dominicains, 1940. Pp. 187.

The first annual report of the new association initiated by members of the Dominican order for the study of scientific methods in education, represents a happy collaboration, between Canadian and American psychologists, as well as a sane and balanced combination of the philosophical and scientific points of view. The object of the association seems to be primarily the study of educational problems in their French Canadian setting, so that the articles of Régis, Barbeau, Mailloux, and Marcotte are in French, and have a local reference as well as general value. The other papers are in English. All of the papers, but from very various angles, are concerned with the problems of mental hygiene and education.

The introductory essay by Father Louis-Marie Régis treats of the exact significance of this new Canadian initiative in scientific education. It is naturally an essay in orientation, discussing in a broad philosophical way "the genetic elements in the concept of education, the relation between education and the science of the individual, and the value of scientific methods completing the science of the individual." Father Régis sketches accurately the proper rôle and limits of metaphysics, the philosophy of human nature, and experimental psychology, in the study of the individual, and finds ample justification for all these approaches. Within the broad framework of the program suggested by Father Régis, a number of papers bearing on the relations of mental hygiene and education appear at the three following conferences.

Dr. Barbeau's study of "The Child and Criminology" includes discussions of the child at the bar of justice, as witness, victim, or delinquent, outlines the principles, governing the institution and conduct of children's courts, and offers statistics of child delinquency at Montreal. He studies the psychological, biological and sociological courses of delinquency, dismisses all concepts of biological fatalism, and relates the problems of prevention and cure of delinquency to the more general problem of an education at once in sympathy with the child and scientific in its methods.

Professor Webster of the Psychological Institute of Montreal arrives at the same conclusions through an analysis of the concepts of personality and character. He briefly discusses the trends of research and personality, criticizes the impractical character of most definitions of the term, and urges the acceptance of a workable definition in terms of actual behavior and concrete goals—a procedure illustrated in recent studies by Doll and Link.

The reports of the third session of the Congress are of particular value and broad theoretical import, appealing as they do to all psychologists and psychiatrists, whatever their special field of interest.

Father Noël Mailloux presents a concise yet comprehensive study of the concept of sexuality. He offers a subtle philogical and logical analysis of the term and its component elements in their genetic setting. While acknowledging the valuable services of psycho-analysts as observers of the sex life of man, he does not subscribe to their fundamental doctrine, and corrects certain concepts, such as that of the polymorphic-perverse character of infantile sex. For this he would substitute the terms polymorphic pervertible, suggesting mere possibility under certain circumstances, rather than an inherent nature governing the process of integration. Mailloux, believes that the problems of infantile sexuality may be solved by a tolerant and sympathetic application of modern methods for studying the individual. The child here as elsewhere must be guided always in the light of his actual capacity to understand, through the concrete material of socially and morally desirable habits, to the sense of values and ideals, and the final possession of responsibility, moral competence, and social usefulness.

In another paper of great theoretical value, Dr. A. G. Bills, discusses the principle underlying the hygiene of mental work. This is a remarkably concise and luminous account and interpretation of recent studies of work and fatigue, many of them by the author, or by his pupils. A number of unpublished researches are included. The author classifies methods of studying fatigue, points out their various limitations and the conflicts and agreements in data yielded by the various methods. It is a field that presents basic facts for mental hygiene, and, brings out clearly the point generally agreed upon by members of this congress, *viz.*, "that the mind is so intimately associated with the entire organism in its operation that it is impossible, to consider the one independently of the other" (p. 146).

The papers of the fourth and last session deal respectively with the "Practice of Mental Hygiene in the School," and "The Rôle of the Teacher in Popular Education." These papers are of a more popular character but present, nevertheless, points of view and criticism of much value. Under the first heading, Dr. Marcotte offers a classification and analysis of abnormal types of children found in the school, *viz.*, those handicapped by bad physique, or low intelligence, and discusses the behavior problems arising from the presence of these two types. A brief discussion of the neurotic and the socially mal-adjusted is followed by the sketch of a possible program for mental hygiene in the schools. In conclusion Dr. Long offers some considerations on the formation and education of character based on recent experimental studies and the view that "character is a phase of personality comprising especially the more enduring traits which are of ethical and social significance" while the "goodness of character is determined by the degree to which conduct is directed intentionally to the common good" (p. 175). Dr. Long concludes that behavior among children is highly specific, and that character training must build up good habits by supplying specific situations for their functioning. He thinks that "misdirected intention rather than inadequate understanding is at the root of most misconduct" (p. 186). In short, character training in children must be concrete and practical—by attractive examples rather than by formal precepts.

It may be said that the papers read at the meeting present a noteworthy agreement concerning the chief principles of the mental hygiene of childhood in relation to education. All agree on the need for scientific approaches, tempered, however, by an enlightened sympathy for the condition and point of view of the child. All agree on the necessity of concrete and practical procedures, adapted to the comprehension of the individual child, in whatever situation he may be. There is a common rejection by the authors of the fatalistic implications of any one sided point of view, and a common perception of the complexity of situation in which the mental hygienist works. And all urge the continuity of education and mental hygiene.

These papers then, constitute a kind of symposium of the subject abounding in useful summaries of recent literature, as well as reflective interpretations designed to promote the further advance of mental hygiene in its educational applications.

University of Cincinnati CHARLES M. DISERENS

La Pensée préconsciente. By C. KONCZEWSKI. Paris, Félix Alcan, 1939. Pp. xxi, 273.

This book, provided with an introduction by Pierre Janet, a foreword and preface by the author; but true to French type, with no index, is an attempt to prove for the "manyeth" time the rôle of affectivity in reasoning and all creative work. In a sense it is a metapsychology, in that it builds up a superstructure comprising man and the world, on the strength of analysis, largely introspective and of the armchair variety.

Janet, in his introduction, makes some pertinent critical remarks, telling us that the Polish author has exaggerated the neglect of affective psychology, and that these reflections on all sorts of great questions are not always very clearcut, nor very original, and that it is to be regretted that the author has scarcely given a historical outline of the matter. Apparently the author is a philosopher who likes psychological problems; and Janet's hope that he would some day, after continuing his studies, present us with a somewhat more precise research on a more definite subject is quite apropos and receives our approbation.

What strikes us at once as we dip into the volume is that much of this we had read in our youth. No, this is not a *déjà-vu* phenomenon. It can be accounted for the fact that M. Konczewski has apparently thought along the lines of his studies at the Collège de France and the Sorbonne. The book is dedicated to the memory of H. Delacroix, as "homage of my profound veneration." There are references to French philosophers and psychologists. In a tome on preconscious thinking, and assertedly dynamic psychology, Freud is mentioned parenthetically once! No German, British, or American psychologist is found in these pages, except for William James. (There is a discussion of substantive and transitive states, which should have been credited to William James.) Bühler is referred to, but his *Aufgabe,* in connection with imageless thought, which figures in the volume, appears as *tâche.* It is doubtful whether the author has read anything in German.

In short, it would seem that the book is a very much expanded seminary paper based on the views of the author's professors and their colleagues. The fact that much in the essay reminds us of James's writings and of the American pragmatists of 25 years ago should not surprise us in view of the kinship between James and French thought at the turn of the century.

The author goes to great length to demonstrate that most of our reasoning is grounded in subconscious elements, that there is a conflict between the environment and the "deeper self," a constant opposition of the external reality to the internal reality. Sometimes the one dominates; at other times the other. There is, therefore, a duality of consciousness and also a duality of time; an inner time and an outer time, and the past is important in shaping the future.

In trying to find a striking idea to quote, we are confronted with what could have thirty or forty years ago been regarded as significant but now must be considered as either boringly commonplace or dangerously speculative, if not exploded. Let us take a few instances:

"The direct sentiment of our self is founded on that which one experiences at the time *(actuellement). Every desire, every intense sensation is the affirmation of a self* which manifests itself and which realizes itself through the mediation of the sentiments." (p. 99, italics his).

At the end of the chapter on "Preconscious Thought," the author formulates the following conclusions: "Preconscious processes directed by a sentiment of harmony are often very complicated and subtle operations. They cannot be regarded as inferior to conscious processes. . . . It may readily be seen that conscious judgments constitute most frequently the smallest part of mental work. They are very inexact and for the most part commonplace attempts at interpretation of a much vaster subconscious activity."

The most that can be said about the volume is that there are a number of stimulating passages, interesting quotations, that the filigree for the somewhat outmoded propositions, or else almost truisms in our present generation, is elaborately developed. As psychology, it is good impressionism, which leads us to the final bit of criticism. The author writes as if he had never heard of types or differential psychology. It seems as if all the various processes which he unfolds are supposed to function in the same way with every one, but perhaps he is thinking of facts that are superindividual, superpersonal and, therefore, superempirical—a sort of phenomenology in Husserl's sense.

Cambridge, Mass. A. A. ROBACK

The Psychology of Physics. By BLAMEY STEVENS. Manchester, England, Sherratt & Hughes, 1939. Pp. xvi, 282.

The psychologist who turns to this book expecting to find a discussion of how the physicist work and thinks will be greatly disappointed. In fact, he will probably wonder why the word *psychology* appears in the title. The book is a discussion of theoretical physics, not of physical thinking.

According to Stevens, the fundamental postulates and concepts of physics are 'subjective' and not 'objective.' His 'psychology' of physics consists in stating these postulates and working out a theoretical system from them. Any concept which is in logical agreement with the postulates is called a *percept.* If Stevens had proceeded from such statements to an examination of how our perceived world of spatial and temporal objects governs our thinking about the abstract space and time of physics, we might accept Stevens' title. This, however, is never done. To be sure, he has stated in his conclusions that he has *shown* "that the laws of physics depend upon the psychological organization which has been evolved in man's ancestors by natural selection" (p. 266). The demonstration consists, however, in the flat assertion that the basic postulates are subjective.

The significance of the word 'subjective,' an ambiguous term at best, is more confused than usual in Stevens' writing. In one meaning of the term one might argue that anyone may make his own physical theory of space and time and that one theory is as good as another. Such an interpretation might be suggested by Stevens' criterion of the objective existence of the sun. The sun exists because we all see it, we also feel it, and we note its effect on climate (p. 5). This would seem to imply that subjective things are things upon which we do not agree. Stevens does not go this far, however. The postulates must be formulated in such a way that inferences derived from them will agree with the findings of modern experimental physics.

An alternative interpretation might be that these 'subjective' postulates are dependent upon the common features of man's thinking, and that they would be granted as obvious by all men. Stevens insists, however, that his postulates are not axiomatic and that they can be derived only by a process of search and testing.

Even if we grant that the statements made in this book about the subjective character of temporal and spatial postulates are meaningful, there is still a question whether a work which deals with the 'subjective' is necessarily psychological. If it were, all scientific writings would be psychology, since frames of reference of one kind or another must be introduced. The admission that one is using a system of classification or a coördinate system which is arbitrarily chosen for convenience in handling the facts of a science is scarcely a psychological description of the scientist's performance as he goes about his task.

Whether the book makes any important contributions to theoretical physics must be left for the physicist to decide. For those who may be interested in the work from the physical side it should be noted that Stevens presents an extensive theoretical system in a simplified form for students. By means of his system of definitions and 'perceptual' reasoning he claims that "one after another the main axiomatic props of modern physics are shown to be fallacious; namely, the mass-velocity law, the Doppler effect theory, special relativity, . . ." (p. vii) and so on, for a list of seven more laws and concepts.

It is unfortunate that Stevens has decided to use the terms *psychology* and *perception* in his discussion. Aside from disappointing the psychologist interested in thinking, it may bring about more misunderstandng than ever about the nature of psychology among scentists in other fields.

Cornell University T. A. RYAN

Psychologie und Psychiatrie der Conversion. By ERNST HARMS. Leiden, A. W. Sijthoff, 1939. Pp. 120.

In this brief work dedicated to C. G. Jung, Dr. Harms makes a substantial contribution to the rather meager literature on the psychology and psychiatry of conversion, regarded in a wider sense than in the classic work of De Sanctis. As Dr. Joh. Van Der Spek remarks in a special preface, the great significance of the treatise lies in its emphasis on social psychiatry in the treatment of disturbances produced by any fundamental change in the personal viewpoint of the individual. Dr. Harms proceeds very systematically discussing in succession the psychology, the psycho-pathology and the psycho-therapeutic treatment of conversion, with a final chapter presenting the attitude and guiding principles of a scientific social psychiatry.

Unlike De Sanctis, Dr. Harms does not limit the concept of conversion to that of religious conversion. A conversion, for him, is any transition or change of an individual from one world view to another. It may be philosophical, scientific, economic, social, or political as well as religious provided the change produces a new orientation of the psyche, a new distribution and balance of personal energies, a new play of forces in the reciprocal actions of the person and his environment. In some cases this may amount to a disruption of the entire personality since the individual in question is faced by a persistent conflict between old habits, tendencies, beliefs, and social bonds, and the wholly different system of activities imposed by his new attitude. Dr. Harms, following Robert Sommer and C. G. Jung, conceives of the individual as a relatively closed system of psychical energies, a psychic monad, in relation, however, with other psychic monads and systems of monads, in the form of society and super individual systems of concepts, and moral tendencies. The point of view has much in common with that of Wm. McDougall. It is evident, therefore, that in cases of severe conflict due to a change in the individual's *Weltanschauung* of whatever kind, the effort of individual psychiatry may fail unless supplemented by a profound and sympathetic study of the successive and competing social environments of the individual subjected to the strain of conversion.

From the standpoint of individual psychology conversion is an exchange and transformation of the psychic contents of a personality in which thought, feeling, and will, may all be involved. It commonly begins in the unconscious and culminates in more or less conscious conflict. In the conversion process, which may or may not involve sublimation, the transformation of will and affect proceeds, most slowly and with the greatest difficulty and danger to the personality, a point of particular importance in the psychiatric treatment of such cases. Conversions of a reflective character, *i.e.* which shift the center of gravity of the personality from feeling to thought are safest from the psychiatric standpoint. Indeed such conversions may even stabilize the personality and improve mental health.

Conversion is, from the standpoint of the author, a phenomenon of social psychology, characterized by the conflict of the individual with, the super individual religious, political or philosophical setting in which he has grown up, which so to speak has formed his personality.

This is the pathogenic situation, and it is at this point that the psychopathology of conversion appears in many cases, and Dr. Harms finds that these are classifiable as schizoid or even schizophrenic, and paranoid types, which conform in appearance to the types recognized by individual psychiatry, but can only be improved or cured by an exhaustive study of social causation and a judicious adjustment of the social environment of the patient. A number of interesting pages are devoted to the depressions, phobias, paranoid phenomena, amnesias, and demented states of victims of psycho-pathological conversion. He even suggests that the perennially interesting case of Nietzsche may be an example of the type.

In the third section of his work, Dr. Harms offers a detailed study of eight patients suffering from psycho-pathological disturbances due to some type of conversion, and describes the methods of social psychotherapy actually employed with good results in all cases. The cure depends upon insight into the individual case, through a complete knowledge of his social relationships followed by the special social therapy of adjustment. We may call this process psycho-synthesis, or better reconstructive therapy, which aims at the redintegration of the individual with the social and mental world in which he is obliged to live. This must be pursued in the light of social characterology, *i.e.* a theory of types of social behavior, such as Willy-Hellpach proposes.

In conclusion, Dr. Harms urges psychiatrists to view their problems in the light of philosophy and sociology as well as of psychology and medicine, and to develop to a kind of medical religion, a "Charitas-Religion," with no dogma save that of being beyond dogmas. His book may be warmly recommended to psychologists and psychiatrists not only for many interesting details and case studies but for its methodological value, and useful suggestions as to further studies in a field which has been comparatively neglected.

University of Cincinnati CHARLES M. DISERENS

The Psychology of Common Sense: A Diagnosis of Modern Philistinism. By A. A. ROBACK. Cambridge, Mass., Sci-Art Publishers, 1939. Pp. 350.

At a first glance Dr. Roback's book seems to lack unity, although the titles of the fourteen essays, half of which have appeared in magazine form, are intriguing to the psychologist as well as the general reader. As one reads, however, a single theme will be found running through all of them, namely the *Leitmotif* of reason. The author only claims this for most of his essays, but even such light essays as those on "Marriage Lore" and "Music and Moronity" are eminently reasonable, even if somewhat temperamental. Particularly good are the essays on the definition of sanity, sex in dynamic psychology, the neurotic through the ages which presents an excellent brief history of the subject, the quack in psychology, the psychology of success, and the concept of significance.

Dr. Roback remarks that there are few or no books on the psychology of common sense, and that few psychologists, since the time of the Scotch school have made any appeal to it. To remedy this defect and illustrate the vagaries produced by the absence of common sense, he makes a brave attempt to achieve an adequate definition, by calling upon all the resources of etymology, logic, law, social science, and several other disciplines. In a sense he approaches his problem indirectly, by examining the meaning of a number of what are supposed to be related terms. In the last analysis common sense turns out to be good sense, sanity of a superior order, the ultimately rational—for much that passes as reasonable in any particular age is later seen to be absurd. There is, however, a kind of reasonableness which is perennial. If not immediately grasped as reasonable, it will be adjudged so in the course of ages. There is a fundamental significance to which thinkers in various ages are striving, which unfolds itself gradually as the result of human experience and which clear minded intellects not necessarily technical philosophers can intuitively perceive. Possibly this is what is to be understood by Spinoza's concept of Scientia "intuitiva" (p. 318). Here is the fundamental passage of the book.

Common sense is just this capacity to grasp what will finally prove perenially reasonable. As such it is very rare, indeed much more so than intelligence which can exist in the absence of common sense or sanity. As an authority on Characterology, Dr. Roback, contents himself, with a simple division of the intelligent into the sound and the surd, the latter a vast majority. Good tests of common sense, the capacity of the sound and thoroughly sane, would apparently be of far more importance than intelligence tests, and their future appearance is confidentially predicted.

Dr. Roback sometimes seems paradoxical in some of his statements, while others may be unwelcome to close adherents of particular schools, or to those who emphasize "newness' in psychology and entertain contempt for the psychology of the past. Actually psychology represents a continuous growth, and differences in terminology or predilection for particular methods do not constitute any important novelty. "There is only one science of psychology although there are many methods, theories, and schools. The trunk of the great tree had taken root thousands of years ago. The 'new' psychologies of today are only branches that bid fair to conceal from view the stout trunk that was already in existence at the time of Aristotle." Although our laboratories have furthered our knowledge tenfold, "there is sill a good deal in the 'Anthropologie' of Kant, in the 'common sense' observations of the Scotch school, not to mention the monumental textbooks of James, Wundt, and Ebbinghaus which will bear examination today; and furthermore, there is much in the so-called new psychologies of today which will with justice be forgotten in a few decades" (pp. 189 f.). Some contemporary psychologists at least will recognize that this is an eminently reasonable statement meriting frequent repetition.

Other points deserving attention in these essays are the recognition of Freud as the real founder of dynamic psychology, the protest against the contemporary child cult with its tendency toward the infantilization of adults, the frank declaration that much modern music—and the author might have added modern art in general—is significant of a low grade of intelligence or rationality, and an interesting discussion of freedom and the psychology of dictators. The author also criticizes popular conceptions of success, unfortunately promoted by certain psychologists. All such popular views and vagaries are typical products of Philistinism, or rather of the debased and short-sighted pragmatism which is the basis of the Philistine's attitude. Dr. Roback's essays are full of wit and wisdom and may even attract and convert some of the Philistines, whose follies afford him a background for his psychological picture of common sense.

University of Cincinnati CHARLES M. DISERENS

Child Psychology. By MARGARET WOOSTER CURTI. Second edition. New York, Longmans, Green & Co., 1938. Pp. vii, 458.

In the opinion of the reviewer this edition of Dr. Curti's book is a decided improvement over the first. It is written in an interesting style, and is stimulating to the advanced and scholarly student. The author made no attempt to survey the literature in her field but she did report a sufficient number of the more important studies to cover the major areas and these were accompanied by pertinent suggestions for further research. The second chapter, in which one child is followed through her first five years of development is of considerable value, especially when elaborated in the class room by a general discussion of the preschool child's development. The discussion of hereditary and environmental factors in prenatal development and of the nature of the behavior of the prematurely born and of the neonate is tedious and, because of its highly technical nature, is of little interest to students taking their second course in psychology.

The sections of the book which especially appealed to the reviewer are those on the conditioned response, preceptual motor-learning of complex habits, reasoning, problems in mental life, social and ethical behavior, and the discussion of the integration of personality. The author's discussion of the origin of meanings, their growth, and their organization in reasoning is good, though too long and too involved for undergraduates. The section on reasoning centers largely around the work of Piaget.

The most serious defects of the book are as follows. The author presents an unanalytical and biased discussion of the effect of training upon pitch discrimination (p. 150 ff.), in which the ability to discriminate pitch is confused with the ability to reproduce various pitches audibly. In the discussion of the doctrine of inherited intelligence, she has accepted the results of Newman, Freeman, and Holzinger's study on twins without taking cognizance of the criticisms that had been made of their work. Furthermore, she has accepted at face value the Iowa studies on environmental effects upon the *IQ*. A section on the interests, play activities, and motivation of the child would have increased the usefulness of the book. A table of contents, rather than a mere listing of chapter headings is called for in a textbook.

Pennsylvania State College EDWARD B. VAN ORMER

The Individual and His Society: The Psychodynamics of Primitive Organization. By ABRAM KARDINER. With a foreword and two ethnological reports by Ralph Linton. New York, Columbia University Press, 1939. Pp. xxvi, 503.

This volume represents a significant advance in the psychology of personality and belongs on the 'must' list of every social psychologist. In the tradition of Malinowski it examines Freudian theory against the background of anthropological evidence, but it goes beyond both Freud and Malinowski in integrating the functional societal emphasis of the ethnologist and the psychobiology of the analyst. The author develops constructively the implications of the Freudian neglect of the institutional pattern of a culture.

The thesis is that instead of a basic structure of personality, universal to all cultures, the ego is the product of the individual's reactions to specific institutions. This view is not novel but its elaboration and implementation by Kardiner are noteworthy. To the reader who has been impressed by the static nature of most dynamic theories of personality the four chapters comprising Part II will be the most valuable part of the book. Here, after Linton's ethnological reports of the Marquesan and Tanala societies, Kardiner works out a genuinely dynamic account of personality development. The interaction of specific institutional pressures, especially familial, and the needs of the individual are well described in terms of the anxieties and frustrations created in the personality, the defenses mobilized and the accompanying psychic elaborations. In place of the orthodox Oedipus complex appears an informative portrayal of the child's dependency upon adults as it relates to the particular disciplines and training of the culture.

The rest of the book is devoted to methodological and theoretical considerations. An excellent critical review of the social psychology of Freud is included. Not all psychologists will be completely satisfied with Kardiner's methodology which centers about genetic interpretation similar to that of the analyst. While Kardiner may not, however, make us all into neo-Freudians, he has made his psychiatric approach comprehensible, useful, and sensible.

From a systematic point of view objection may be raised to Kardiner's dichotomy which contrasts fixed, objective social institutions with the changeable, subjective patterns of personality. Social institutions are after all only patterns of human attitude and habit. In justice to the author, however, it might be added that he is aware of the problem. Moreover, even if all the unsolved problems of social psychology are not answered in this book there is ample reason for satisfaction in its rich suggestiveness, its penetrating insight, its wise interpretation and its theoretical contributions.

Princeton University DANIEL KATZ

Tides in the Affairs of Men: An Approach to the Appraisal of Economic Change. By EDGAR LAWRENCE SMITH. New York, Macmillan Co., 1939. Pp. x, 178.

This book is of interest to the psychologist primarily as an addition to that relatively small body of literature of which the studies by Huntington and Dexter are probably the most familiar. The author undertakes to show a relationship between weather and general business conditions. The latter he measures chiefly through the Index of Stock Market Prices, although certain other corroborative economic data are also included. The term weather is used to mean not the day by day changes, but the broader variations which might be described as "kind of weather" and are illustrated by such expressions as "a cold winter" or "a dry summer." A large part of the book is devoted to statistical evidence for a recurrent decennial pattern in stock market fluctuations, as well as to the comparison of annual changes in stock market prices which the concomitant variations in rainfall, temperature, and barometric pressure. At the same time, the author quotes data from astronomical writings which suggest a 3- to 4-yr. cycle in solar radiation, of which the changes in sun spots are a partial indication. The author points out that a 10-yr. cycle would result from the combination of a 12-mo. and a 40-mo. cycle, both of which are correlated with solar conditions. The underlying thesis of the book is that changes in the amount and quality of solar radiation may have a direct effect upon man's physiological and psychological condition which would in turn be reflected in the confidence, optimism, and other attitudes manifested in his economic behavior. Thus both the weather conditions and the economic changes would be direct results of the variations in available solar energy.

The most obvious criticism that can be leveled against the author's approach is the familiar one that causation cannot be inferred from correlation. None of the evidence presented shows more than concomitance. The aspect of human behavior under consideration, furthermore, is so complex and subject to so many influences, as to make it very unlikely that all other factors could have been adequately recognized or ruled out in the statistical comparisons which were made. The data presented fit the hypothesis only approximately and the generalizations are at best tentative, as the author himself points out. The author places much emphasis upon the fact that his predictions for 1937 stock market prices were subsequently verified and two chapters are devoted to these analyses. The predictions for 1938 and 1939, however, did not check so closely with subsequent developments and are dismissed in two pages. The book contains two chapters entitled "Tides in Mass Psychology" and "Speculative Psychology," respectively. The only psychology to be found in these chapters is in the titles. It would have been better for the general feeling tone and prestige of the book if the material in these chapters had either been omitted or condensed into one or two paragraphs and presented as the author's personal opinion rather than in the name of psychology. Regarded as psychology, many of the statements in these chapters are naïve or absurd, but it is perhaps fairer to the author to ignore the fact that he labels this sort of thing psychology.

Queens College ANNE ANASTASI

Predicting Success or Failure in Marriage. By Ernest W. Burgess and Leonard S. Cottrell, Jr. New York, Prentice-Hall, 1939. Pp. x, 472.

This book, based on 526 returns to a questionary concerning marriage, is essentially a research report but it is written in a style which should make it appeal to a fairly wide audience. The essential facts regarding the marriages and the social backgrounds of the participants were supplied, anonymously by 153 husbands, 317 wives, 30 husbands and wives together, and 15 husbands or wives with the assistance of an interviewer. The group studied represents a sample from 7000 couples to whom the questionaries were distributed. For the most part, they are urban, middle class, Protestant, and native white. All were married from one to six years.

An index of marital adjustment based on 26 items in the questionary was used to divide the marriages into three criterion groups (good 43%, fair 28%, poor 29%). The response of each group to each of the other questions was then analyzed and the significance of differentiating responses evaluated by finding the critical ratio of the difference in proportion. On the basis of these differences, weights were assigned to various responses and a scale for predicting marital adjustment was constructed. For the couples on which the weights were derived, this prediction score correlates +0.51 with the adjustment score of the marriages. For a different sample of 155 cases, this validity coefficient was +0.48.

In line with their attempt to make the book readable, the authors have placed nearly half of their 114 tables in an appendix and all of the critical ratios in footnotes. Fifty-eight charts (bar diagrams) are used to portray the proportions of each criterion group making each response to the differentiating items. Five detailed case studies are also presented. The appendix contained a chapter bibliography of about 500 titles.

Methodologically, the research has flaws, most of which the authors point out themselves. One wonders if the findings would have been the same had the 6500 non-cooperating couples also returned their questionnaires. Appropriate analytic techniques were employed, but one wishes more attention had been given the patterning of husband-wife backgrounds and the relationship of these patterns to compatibility.

One cannot but make certain comparisons of this book with Terman's "Psychological Factors in Marital Happiness," published a year before. Both studies sought to find correlates of varying degrees of marital felicity and both resulted in "prediction scales" of marital compatibility. Terman studied the larger population, 792 marriages, and received responses from both husband and wife. Burgess and Cottrell studied 526 and in most instances secured responses from one spouse only. Terman's method of collecting his data probably resulted in franker responses and less collusion between spouses. Terman's data included information on personality, social background, and sexual adjustment where as Burgess and Cottrell's data deals almost exclusively with social background. Both studies, however, offer encouragement to those who believe that even complex social problems involving multiple causation can profitably be made the subject of research.

Purdue University E. Lowell Kelly

A Study of Retarded Children in the Elementary School. By William McGehee. Nashville, George Peabody College for Teachers, 1939. Pp. 128.

This monograph presents the statistical findings of the investigation of retarded children in 36 states, 310 communities, and 455 schools in the United States. The children were of both sexes, from fourth through eighth grades. For the purposes of comparison, four groups are recognized. Group one is normal, and is here considered purely for purposes of comparison with the other groups. The second group consists of the 4,627 children whose scores on Kuhlmann-Anderson tests comprise the lowest 10% of the distribution. The third group consists of 3,359 children whom the various teachers had classified as "retarded" before the above mentioned intelligence test had been administered. The fourth group is composed of the children who fall in both the second and third groups mentioned above, and they are called the "lowest 10% and retarded" group.

Chapters III to VII are concerned with comparisons of the mental status as revealed by the intelligence test and the following factors: (a) Scholastic achievement as revealed by certain standardized educational tests arbitrarily chosen. (b) The personalities of these children as revealed by scores on the B. P. C. Personal Inventory and teachers ratings on some 70 personality traits. (c) The interests and social adjustment as revealed by extra curricular activities, hobbies, and play interests. (d) The environment as revealed by the father's occupational status, certain social economic criteria, size of family and rank in birth order.

The work as a whole is top heavy with expansive tables, the significance of which would have been much more easily grasped in graphical representation. The body of the book is padded with obvious comments on the content of these tables. The ground which the study covers has been cultivated for many years, and nothing whatever new is discovered, but one of the conclusions might be restated here, even if it only corroborates well established truisms.

There is a preponderance of males in all the 'retarded' groups. Insignificant differences in intelligence as revealed by the test can not account for this. The explanation of this condition seems much more closely related to personality conditions, in which the boys show significant tendencies toward maladjustment. Since this distinction is based upon estimates of children by teachers, it seems likely that the traditional "school ma'am" is ruffled unduly by the more aggressive, truculent, and defiant young males and shows a decided preference for the more docile "nice little girls."

This monograph confirms a number of previous findings, and may serve as a valuable source book for investigators with more advanced statistical techniques who may wish to analyze more fully the 'retarded' children.

Iowa Child Welfare Research Station N. J. VAN STEENBERG

Their Future Is Now. By E. M. LIGON. New York, MacMillan Co., 1939. Pp. iv, 369.

Their Future Is Now sets forth its author's system for correcting personality maladjustments and for child guidance, in terms of the teachings of Jesus Christ. Eight "personality goals" entitled, "Vision," "Love of righteousness and truth," "Faith in the friendliness of the universe," "Dominating purpose," "Being sensitive to the needs of others," "Forgiveness," "Magnanimity," and "Christian courage," form the basis of the system proposed. Each personality goal is traced through nine age-levels which cover the period from birth to maturity. In addition to a preface and historical introduction, there are 15 chapters which bear the following titles:

"The Nature of Character and Personality," "Dynamics of Character," "The Personal Equation," "Their Future Is Now," "Before the Nursery," "Character Development in the Nursery," "The Kindergarten Child," "The First School Years," "The Third and Fourth Grades," "Middle Childhood," "In Junior High School," "The High School and College Age," "The Mature Personality," "Social Integration" and "Happy are They."

One is impressed upon reading this volume with the intense sincerity of the author, and of his highness of purpose. The principle, often emphasized, of supplanting socially undesirable reactions with socially desirable ones is sound and workable. The style of writing is clear and direct. Yet the book contains entirely too many religious quotations and references to the Bible, and would be improved if most of them were omitted.

Despite the fact that the author has much to say about a scientific approach to the problem of personality, the present reviewer seriously questions the adequacy of the scientific attitude demonstrated. The ideal of personality measurement is excellent, but the means of measuring personality are confined in the main to the author's own rating scales and to his own questionaries which are given at the ends of several of the chapters. We have been unable to find any reference to the reliability of the rating scales or questionairies, beyond the suggestion that two or more raters are better than one. The ability of the raters to make valid ratings seems to be taken wholly for granted.

Statements like the following will be particularly objectionable to the scientific reader and make one wonder just how far the author pressed the scientific method in the evaluation of his own writing. On p. 4, for example, is the definition that, "Character consists of the characteristic forms of reaction of the personality." On p. 166 we are told that, "Sympathy is a natural, inherited attribute in human nature." We learn on p. 183 that, "During the years from eight to fourteen physical courage is the most desirable of all personality traits." And p. 185 informs us that the period of the third and fourth grades "is the first period at which true poverty of spirit can be developed." In making such statements it would appear that the author has let his religious fervor run away with his scientific judgment.

The group that *Their Future Is Now* is likely to interest most is the church laity, rather than technically trained psychologists.

Indiana University W. N. KELLOGG

Sexual Pathology: A Study of Derangements of the Sexual Instinct. By MAGNUS HIRSCHFELD. Revised edition. New York, Emerson Books, 1940. Pp. 368.

The first part of this book consists of case records of fetishism and anti-fetishism collected by the late head of the Institute of Sexual Science of Berlin. It is not constructive, either from the point of view of theory or that of therapeutics, but forms an interesting and useful background for the rest of the book.

Abnormalities of the quantitative variations of the sexual urge are more scientifically discussed in the chapters on hypereroticism and its deviations, polyeroticism and superfixation. Much consideration is given to divergences from normal cohabitation, and to masochism and sadism. Sadism is "a sort of atavism"—a reversion to primitive violence more or less inherent in the nature of the sexual act, but "overcome normally through the development of restraining or sublimating mechanisms." The true "lust murder" is usually committed not by the oversexed, but by the undersexed, in a frenzied rage at his own relative impotence and the resistance of his victim. The treatment of hypereroticism should be "symptomatic and casual, with medicinal and psychic sedatives" (suggestion). Dr. Hirschfeld never *advised* castration, but found it beneficial when voluntarily undergone.

Part III of the book is devoted to a thorough discussion of the causes, manifestations treatment of impotence. There may be cerebral impotence, with lack of urge or pleasure; spinal impotence, with disturbances to erection or ejaculation; genital impotence, with organic defects which prevent cohabitation; and germinal impotence or sterility. All of these may be accompanied by psycho-neurotic strain. There should be a trial period of a year or two before impotence is judged chronic and incurable. Treatment should be based on exact diagnosis. Organically conditioned disorders should combine general treatment with that of the basic ailment, but local measures forcing anxious attention on the region may increase psychogenic factors and should not be overemphasized. In spinal disorders, strychnine, galvanization of the vertebral column, and Roentgen rays, are used. Functional disorders must have *active* psychotherapeutic treatment of the whole neurotic condition. The author has "never seen final cures result from psychoanalysis alone," though it is useful in revealing restraining psychic factors. Suggestion during hypnosis and also masked behind physical methods such as stimulating medicines, is curative. *All sexual intercourse* was forbidden during treatment. He regards the use of glandular extracts as less effective than expected, but hopes their use may be perfected.

University of Michigan BERENICE BARNES SHEPARD

Correcting Nervous Speech Disorders. By MABEL FARRINGTON GIFFORD. New York, Prentice-Hall, 1939. Pp. 197.

Within the past few years there has been a growing interest on the part of physicians, psychologists, educationists, endocrinologists, and speech pathologists in the subject of stuttering. The explanations of the etiology of stuttering are at least 16 in number, and the recommended therapeutics are even more numerous. Speech correctionists of various schools of belief concerning stuttering claim that their methods beget results. There is a growing belief among speech specialists that stuttering and allied speech disorders may be caused by a variety of conditions—physical, psychological, and environmental—and that a variety of therapeutic techniques are needed to correct the speech of various stutterers.

Since all workers in the field of speech correction are in need of references and descriptions of various techniques now being used in the field, it is a real help to have a description of the therapeutic procedures used by the Bureau for the Correction of Speech Defects and Disorders of the California Department of Education. "Correcting Nervous Speech Disorders" is the result of Mrs. Gifford's work as director of the California program.

Mrs. Gifford states that the purpose of her book "is to bring into focus the technique for the correction of stammering that is practiced in the California public schools." She has written the book primarily for speech therapists, teachers, and administrators who are interested in what is being accomplished in California with regard to remedial procedures. The principles of Mrs. Gifford's technique are based on the premise that stuttering has no organic or functional origin, but is a problem of emotion maladjustment involving the total personality.

The book is conveniently divided into six parts: I. The Teacher and the Field;

II. The Principles of the Remedial Technique; III. The Remedial Technique; IV. Home Cooperation; V. Psychological Readjustment and VI an appendix of thirty pages dealing with record forms, case studies, etc. Mrs. Gifford's psychological principles are eclectic. She selects from various schools of belief those psychological principles that she has found helpful. Conditioning, mental hygiene, suggestion all find a place in Mrs. Gifford's corrective program. Psychologists interested in a way of treating stutterers, stammerers, and clutterers will find this book helpful. It should also find a place in the library of every speech correctionist.

Queens College JAMES F. BENDER

A Textbook of Clinical Neurology. By ISRAEL S. WECHSLER. Fourth edition. Philadelphia, W. B. Saunders Co., 1939. Pp. 844.

A knowledge of clinical neurology is a very useful adjunct to the teaching of psychology. It is often hard to motivate students to study the structures and functions of the nervous system, because these topics seem dry and academic. The normal functions of the cerebellum, for example, may fail to elicit the understanding and enthusiasm of college students, but the effects of a cerebellar tumor are quite striking, and a brief description of this abnormality may turn a dull topic into an interesting one. Even in introductory classes, a judicious use of a little clinical neurology may be profitable.

Wechsler's textbook is a new revision of a standard work, the preceding edition of which appeared in 1935. The greater part of the book (577 pages) is devoted to exhaustive descriptions of the disorders associated with organic pathology of the nervous system. The approach is distinctly clinical, with the emphasis on the symptoms, diagnosis, and treatment of each disorder. The writing is competently done, and, except for medical terms that will cause most psychologists to reach for a medical dictionary, the book is not unduly difficult to read. The index is very serviceable. The bibliographic references are not extensive and few of them are very recent, but this will not detract from the use of the book by psychologists.

Psychology is directly represented in the book at two points. A brief section (15 pages) on psychometric tests is contributed by David Wechsler. Even with allowance for the limitations of space, this section has serious shortcomings. A number of concepts, as, for example, that of the average adult mental age, are treated with conspicuous lack of precision. At one point it is recommended that psychometric examinations be given only by qualified psychologists, yet at the end of the chapter instructions are given for administering a brief battery of tests. It is doubtful that these "correlate very highly with general intelligence as measured by the Binet-Simon tests," as is asserted.

A second psychological topic is found in an extended section on the psychoneuroses (64 pages). These are considered entirely from a psychoanalytical point of view. The only mention of any objective or experimental approach to the psychoneuroses consists of a few disparaging remarks. Psychologists may well conclude from this section that the kind of psychology known to neurologists is an inadequate as the brand of neurology known to the average psychologist! The psychological shortcomings of the book do not detract, however, from its value as a compendium of reference material on the organic disorders of the nervous system.

Carnegie Institute of Technology LAURANCE F. SHAFFER

The Psychology of Parent-Child Relationships. By PERCIVAL M. SYMONDS. New York, D. Appleton-Century Co., 1939. Pp. xiv, 228.

This book is an attempt to study the rôle of emotional security within the home in the development of child personality. The greater part of the volume is concerned with a statistical compilation and discussion of findings from case studies representing extremes of two psychological continua regarded by the author as basic aspects of family relationship. The first of these, the acceptance-rejection continuum, is here represented by 31 matched pairs of *Ss* selected by former students of the writer, who also prepared the case studies under his direction. One member of each pair was regarded as a rather extreme instance of parental rejection, the other as an equally marked example of parental acceptance. The two members of each pair were matched roughly as to age, school grade, sex, intelligence and social status. The complete age-range of the group was from 5 to 23 yrs.

The second aspect of family relationships studied was dominance or submission on the part of the parents. Again the method of matched pairs was employed. Twenty-eight pairs of *Ss* with an age range from 6 to 17 yrs. provided the data. In each of the two studies, the characteristics of the *Ss* in the contrasted groups are subjected to statistical comparison. Evidence for the better social and emotional adjustment of the "accepted" children when compared to the "rejected" cases in the first study seems very clear-cut. In the second study, fairly marked differences in the direction of greater aggressiveness on the part of children of "submissive" parents with greater docility shown by those whose parents were "dominating" appeared.

Remaining chapters in the book are devoted to reviews of the literature on parent-child relationships, teacher-pupil relationships, and councillor-client relationships. Although the author makes no claim to having solved the riddle of interpersonal relationships within the home and school, his discussions of the complicated interlinkages of these factors with the developing personalities of children will be read with interest by clinical workers and students of social psychology.

University of Minnesota FLORENCE L. GOODENOUGH

La conduite humaine et les valeurs idéales. By D. PARODI. Paris, Félix Alcan, 1939. Pp. 138.

In the present volume, a sequel to *En quête d'une philosophie* (1935), the author analyzes the ideals in the classical triad: Truth, Beauty, Good. On the postulate of a "tendance universelle," or continuity of inner tension (Bergsonian "élan"), attempt is made to fuse Plato's "being and becoming," certain nineteenth century teleologies, and dynamic psychology. The whole is reduced to a system superimposed by a mind-will. Logical eclecticism on a strong transcendental bias pervades the piece.

Short shrift is made of pragmatism and psychological determinism as too delimiting for the cadre in which the philosopher seeks to operate. Possibilities in an empirical approach are suggested only to be discarded, less the contemplation of ideal values be reduced to an analysis of intellection, esthetic pleasure, and moral sentiment in their "éléments sensoriels." There is further concern, lest such an approach render religion, like metaphysics, "a vain mirage condemned to vanish before the progress of positive science."

Despite the author's brilliant use of dialectics and nuance (with which his native language is fecund) one detects a kind of noble psychoanalytic scheme wherein the method for determining ideals is as transcendental as the ideals themselves. At times M. Parodi narrowly risks getting inside his system and intellectualizing. In psychological conceptualism he leans toward dynamic psychology as more fertile to his method.

To decry the philosophical import of the work would be tantamount to mental parochialism, for the master logician is everywhere evident in the coherence of his structure. His most provocative insights lie in his historical treatment of the genesis, unification, and divergence in ideal values. On the literary side, the style is always clear. Occasional lyrical passages, which in less mature hands might conceivably slip into the evangelical, are saved by the dialectic severity the author has imposed on his task.

In short, if, instead of offering his philosophy as a handmaiden of the sciences, M. Parodi turns the tables—he must be assailed not as a philosopher, but as a psychologist.

University of Maryland WALTER M. SPARKS

Beyond the Clinical Frontiers. By EDWARD A. STRECKER. New York, W. W. Norton & Co., 1940. Pp. 210.

Dr. Strecker is Professor of Psychiatry in the Undergraduate and Graduate Schools of Medicine, University of Pennsylvania, and Consultant and Chief-of-Service, Institute of the Pennsylvania Hospital. The book is one of the annual series of lectures of the Thomas W. Salmon Memorial Lectures of the New York Academy of Medicine. In a foreword to the lectures the writer says, "these lectures will discuss briefly a few of the crises which are menacing our civilization and its cultures. There will be some attempt to accent the psychiatric implications of these crises. And, finally, there will be sketched in, at least, the crude outlines of an alleviating plan of hygiene that psychiatry might be able to offer to a mentally sick world."

The chapters are a series of non-technical essays insofar as language goes and should prove to be interesting reading to a large group of thoughtful professional people outside the medical world: sociologists, psychologists, biologists, historians and all who must reckon with man's frailties in ratiocination, his mob-crowd escape mechanism, his weakness for propaganda, and his stubborn adherence to a world of shadow with all of its cruelty, social lag, and flight from reality.

The book is easy to read but far from easy reading and hardly a page but provides a challenge if one reads without trying to escape the inevitable logic involved in the implications. Pushed to its logical conclusion there is always the philosophically unthinkable blow to the ego; that is a thoroughgoing determinism which makes of volition and free will a shadowy concomitant of what is already a biological *fait accompli.* Even bio-hereditary matters may be but minor cogs in a more cosmic determinism. This Strecker ignores but skirts dangerously. Such an idea would require a super-escape mechanism and would curdle any "alleviating plan of hygiene" that (Dr. Strecker's) psychiatry might be able "to offer to a sick world."

The social-psychiatric diagnosis in which Strecker compares the emotionalized thinking of crowds (blocks, nations, races, creeds, pressure groups) to the behavior of unsagacious neurotics is quite convincing. His therapeutic suggestions of a psychiatric nature are not so convincing.

University of Pittsburgh W. T. ROOT

Lecture Demonstrations for General Psychology. By NORMA V. SCHIEDEMANN. Chicago, University of Chicago Press, 1939. Pp. x, 241.

Dr. Scheidemann describes, in this little book, 60 demonstrations which she has adapted and condensed from experiments widely scattered in the psychological literature of the last 30 years. Due acknowledgment and complete citations are given to the source material, hence the interested reader may readily obtain further information concerning the facts and principles exhibited.

Besides the selection of the topics to be demonstrated, Dr. Scheidemann's contribution consists in presenting the materials in a convenient and usable form. The following outline is followed in every one of the 60 demonstrations: (1) the purpose is briefly and clearly stated; (2) the materials required are listed; (3) the procedure is fully described; (4) points of special interest to the class are enumerated; and (5) comments are made upon the original investigator's work.

The demonstrations require no special apparatus nor equipment, beyond that readily available to all teachers, and they may easily and quickly be made. They cover a broad field: sensation, reaction, perception, attention, memory, learning, recognition, recall, suggestion, thinking, belief, testimony, personality traits, etc. Teachers of general, educational, experimental, and applied psychology will find material suitable for their use.

The pertinence and significance of the demonstrations vary widely. Some are first rate, others are trifling and of doubtful value. Despite the obvious unevenness and inequalities, Dr. Scheidemann's book supplies a long and widely felt want. It is particularly useful to teachers lacking the facilities of a well equipped psychological laboratory.

K. M. D.

The Dream World: A Survey of the History and Mystery of Dreams. By R. L. MÉGROZ. New York, E. P. Dutton & Co., 1939. Pp. xvi, 319.

The author brings together in most readable form the beliefs and practices which have been associated with the marginal consciousness of dream experience. The book covers a wide range, from the early historical period to the present and from many cultures both advanced and primitive. There is a wealth of illustrative material; an anthology of dreams, it might be called. Preference in selection is naturally given to dreams that are extraordinary in themselves or are associated with persons of note.

Apprehension of the unknown past and divination of the unknown future, clairvoyance of the present, and telepathy, all seem credible as one reads these narratives. Yet the author makes no dogmatic evaluation of past or present theories, but concludes "that the serious consideration of certain aspects of dreaming beyond the scope of psychoanalysis must compel philosophy to take new bearings and dissipate the new materialism that befogs a Freudian machine-age."

Himself a literary critic, the author discusses "creative dreaming" and its influence on the work of men like William Blake and others. Of Christina Rossetti he says, "Few poets have been so haunted by dreams and unfulfilled longings."

The psychologist will find this book a valuable source of illustrative material. The "general reader" will find it as interesting as his favorite mystery story magazine and better written at that.

Ohio State University FRANCIS N. MAXFIELD

Toward Proficient Reading. By JAMES ALEXANDER HAMILTON. Claremont, Calif., Saunders Press, 1939. ix, 152, 4 pl.

This concise little book contains a wealth of practical information for the reader who wishes to improve his reading rate and the comprehension of what he reads, and for the teacher who must deal constructively with reading problems. It has short but adequate chapters on the visual mechanism, analysis and improvement of eye movements, developing comprehension and speed, vocabulary development, and the hygiene of vision. Case histories reveal the effectiveness of the methods employed by the writer in his clinical treatment. An appendix of 35 pages includes a vocalization survey, saccadic exercises, and progress reports.

There are any number of explicit directions for self-analysis of difficulties and numerous exercises and tests which require little apparatus and no more than average intelligence to try out on one's self. For the busy teacher, it is an excellent book to place in the hands of students who complain of poor skill in reading.

Dr. Hamilton proves himself a good psychologist in writing this volume. He recognizes that one essential of a guide book for poor readers is briefness; otherwise the material would be less likely to reach the audience for which it is intended. The reviewer also likes the modesty of the title. It does not promise a sure cure for all reading ills. It merely suggests that one may advance in the direction of the goal by applying the information found within its covers.

Western Reserve University CALVIN S. HALL

An Introduction to Experimental Psychology. By C. W. VALENTINE. 3rd ed. London, University Tutorial Press, 1939. Pp. x, 283.

This is the third edition of a manual which first appeared in 1914. It is intended for students of education who have had no previous training in psychology. Of the 53 exercises included, 18 deal with learning and mnemonic processes, 8 with imagery and association of ideas, 7 with perceptual processes, 6 with motor dexterity, 4 with attention, 3 with intelligence, 3 with thought processes, 2 with mental work and fatigue, 1 with concept formation and 1 with correlation. All necessary materials are included. Part I gives the experimental procedures and materials while Part II discusses the results to be expected from the exercises performed.

Of the 166 appended references only 26 are from American sources. Nevertheless, instructors called upon to teach an elementary course in experimental educational psychology will find this manual quite useful, either as a text or for reference purposes. The manual contains much more material than any other of its kind that has come to the reviewer's attention. Although the comments in Part II are brief, they serve as valuable orientation for the student who may wonder concerning the relation of the various exercises to the problems of teaching.

Vanderbilt University NORMAN L. MUNN

A Psychology of Music: The Influence of Music on Behavior. By CHARLES M. DISERENS and HARRY FINE. Cincinnati, Ohio, College of Music, 1939. Pp. 405.

The subtitle of this interesting book is more appropriate to the treatment than the main title. It is not a technical psychology of music but rather the practical musician's evaluation of theories of the origin of music, its rôle in mythology, folk-lore, magic, religion, medicine, psychiatry, and daily tasks. This comprises the first ten chapters. The eleventh chapter, "Contemporary Experimental Work on the Psychology of Music," is comparatively superficial, groping for every floating straw that may have practical value and ignoring the type of work represented in scientific laboratories.

State University of Iowa CARL E. SEASHORE

Eine tiefenpsychologische Grundlage zur Klages'schen Graphologie. By S. V. MARGADANT. Amsterdam, N. V. Noord-Hollandsche Uitgeversmaatschappij, 1939. Pp. 153.

The author is a Dutchman who writes from the Dutch East-Indies in the German language, because he discusses the theories of a German graphologist. He attempts to point out what is common in the psychological doctrines of Klages, Freud, Adler, and Jung, in so far as these doctrines can be applied to the field of graphology. Among the peculiarities of handwriting discussed the reader finds regularity, irregularities, height of script, speed, pressure, breadth, angle, method of tying, direction, simplicity, ornateness. Those psychologists who are interested in graphology in general and in that of Klages in particular, will find the book clearly written and easily readable. There is much more in it than mere graphology.

University of Miami MAX F. MEYER

Indétermination et création. By LEONE VIVANTE. Translated by L. E. Lanza Paris, Fernand Sorlot, 1939. Pp. 270.

This book presents a theoretical discussion of universal determinism, with especial attention to the conflicting views of philosophical thought and the biological and physical sciences toward determinism. Universal determinism is defined as a principle of rare quality on which depends the sensible and intelligible world, and is likened to psychic truth. The book contains little of especial value to the psychologist.

Beaver College ELINOR J. BARNES

Der Wille als Weg zu Leistung und Persönlichkeit. By ERICH GRASSL. Leipzig, J. A. Barth, 1939. Pp. 58.

A short pamphlet on "Will as a way to achievement and personality," based on a series of popular lectures. There is emphasis upon the genetic, the pathological and the educational aspects of the problem. For the scientific reader this booklet is of little interest.

University of California EGON BRUNSWIK

Emotions and Bodily Changes. By H. FLANDERS DUNBAR. Second edition. New York, Columbia University Press, 1938. Pp. xi, 601.

This book, representing "a survey of the literature on psychosomatic interrelationships," is not a revision of the original publication, which appeared in 1935 but merely "a second edition with supplementary introduction and additional bibliography."

The approach to "the problem of the psyche-soma" is largely clinical. The case studies and specific syndromes are highly informative and illuminating. As a text or reference book it is of interest to the psychiatrist and the medical man. From the standpoint of the psychologist it is less satisfactory. As in the first edition, the extensive research and quantitative treatment of emotional processes undertaken by psychologists is given little or no attention.

In his introduction to the second edition the author justifies this omission on the ground that the psychologist interested in measurement, has tended "to concentrate on the parts without reference to the whole," and that his findings, therefore, have "added nothing fundamental in point of view and to the purpose of this book." Just how the description of specific syndromes and of physiological changes arising under conditions of emotional disturbance give a clearer picture "of the whole" than do the measurement of these same processes under laboratory conditions is not explained.

The material of the text is organized under three main heads. In the first division attention is given to orientation and methodology. In the second, the various organs and organ-systems affected by emotions are described and changes induced under clinical conditions are noted. The third division is devoted chiefly to therapeutic considerations.

Temple University FREDERICK H. LUND

My Mind a Kingdom. By GEORGE THOMAS. New York, E. P. Dutton & Co., 1938. Pp. 293.

This is the story of the everyday doings of a shut-in family living in a tenament district of London, four of whose members, including the author, are afflicted with progressive muscular atrophy. It is told in the form of a very readable, 365-day diary. It is mainly, however, the story of the author's struggles against insuperable difficulties in becoming a successful writer, and an exposition of his philosophy of life.

While the book is not a treatise on psychology, as the title might suggest, the student of the psychology of adjustment will find here a rare example of the integration of personality under circumstances that might easily overwhelm the average person. "Everything in life, or nearly everything, encourages me to lie down and die, but I shall not do so—even to please the *Times Literary Supplement.*" He can say this because all the elements of life fit snugly into his basic philosophy of life, which in this case is the Christian philosophy in its most ancient form, "because death, not life, is the problem."

University of Cincinnati L. A. PECHSTEIN

Psychiatric Clinics for Children. By HELEN LELAND WITMER. New York, The Commonwealth Fund, 1940. Pp. xix, 437.

In this account of psychiatric work with children, Dr. Witmer has admirably fulfilled a three-fold aim. First, she has given a clear account of the basic philosophy underlying the establishment of psychiatric clinics for children, together with a historical account of the social and cultural developments that pointed the need for some kind of organized program of preventive and remedial work. This is followed by a detailed account of the present status of clinical work for children in America, especially that conducted under state auspices. The final section of the book is devoted to an unusually straight-forward attempt to balance the accounts of the clinical work thus far accomplished with view to ascertaining which aspects of it have yielded sufficiently worth-while results to warrant their continuance and which have been relatively unproductive. On the basis of a careful study of the available reports, together with her own very extensive acquantance with clinics and clinicians throughout the country, Dr. Witmer comes to the conclusion that there is little factual basis for the claim that the establishment of psychiatric service for children has had or is likely to have any measurable effect upon the frequency of psychoses or the extent of juvenile crime. Greater emphasis upon the improvement of the present mental health of children and less preoccupation with future contingencies appears to be a more promising line of attack.

University of Minnesota FLORENCE L. GOODENOUGH

Mythes et dieux des Germains. By GEORGES DUMÉZIL. Paris, Ernest Leroux, 1939. Pp. xvi, 157.

A scholarly study of the prehistoric system of Germanic religion. After an introduction dealing with the relationship between Indo-European and Germanic mythology the author presents and discusses the myths of "sovereignty," of "the warriors," and of "vitality" (the latter dealing with fertility, drinking, etc.). There is a great deal of reference to the writings of the Romans, especially Caesar. The concluding chapter emphasizes the militarism inherent in Germanic mythology, as embodied especially in the figure of Odhinn (Wotan), and points toward similarities with the present "magico-military" structure of society and of political attitude in Germany.

University of California EGON BRUNSWIK

Statistics Applied to Education and Psychology. By CLARENCE T. GRAY and DAVID F. VOTAW. New York, The Ronald Press Co., 1939. Pp. xiv, 278.

In this book the derivation of formulae is kept at a minimum, but extensive empirical evidence is presented as to validity for many of the formulae. The arguments are simply and clearly stated, and this, in conjunction with the many experimental demonstrations of why the formulae function, make it an excellent text for beginners in the subject. Numerous problems are presented at the end of each chapter, many of these sufficiently ingenious to make even the better students think.

This text treats the usual measures of central tendency, variability, sampling, and measures of relationship, but does not include such topics as analysis of variance, multiple regression, and factor analysis. In reading the book, one minor error, at least by implication, was noted on p. 169 where the authors state, "By mathematical definition, rectilinear correlation is the slope of the straight line which best fits the y values of the points in a correlation chart." This textbook, nevertheless, deserves careful examination by those teachers whose students lack an adequate training in mathematics for more advanced works but who desire a working and reading knowledge of elementary statistics.

University of Rochester JACK W. DUNLAP

Out of the Running. By G. GERTRUDE HOOPES. Springfield, Ill., Charles C Thomas, 1939. Pp. xvii, 158.

Out of the Running is the intimate, very frank, highly speculative, well-written autobiography of a victim of intracranial birth lesions, who resolves the violent conflicts between her pronounced ambitions and love of life and her physical limitations by seeking refuge and solace in the authority of religion, which has brought peace and comfort to her turbulent life. Although the orthodox student of psychology will hardly agree with the author's decidedly psychoanalytical explanation of her reactions, he will find here an excellent case study, since the author gives us a detailed picture of her overt behavior and most intimate reflections.

University of Cincinnati L. A. PECHSTEIN

The Story of Yiddish Literature. By A. A. ROBACK. New York, Yiddish Scientific Institute, 1940. Pp. 448, with 12 portraits.

The Yiddish language, which came into being when Jews settled on the banks of the Rhine (possibly even before Julius Caesar saw that river) and learned the speech of their German neighbors, is now the family idiom of most Jews. Although Yiddish uses the Hebrew alphabet, the two languages are very different. Hebrew, the language of the Bible, belongs to the Semitic group of languages, being akin to Arabic as well as to the now extinct Phoenician, Aramaic, Assyrian, and Babylonian; while Yiddish is a European language related to German, Dutch, English, and Scandinavian.

Lucid and informative, the *Story of Yiddish Literature* begins with the early medieval period of poetry, romances, *Purim*-plays, homilies, and medical treatises. The bulk of the book is devoted to the modern period from 1864, when Mendele wrote *Dos Klein Mentshele,* to 1940. Several chapters deal with Yiddish literature in America. Another chapter deals with scientific literature.

A product of 19th-century nationalism, modern Yiddish literature is sociologically significant in two other ways. It is the most international literature in the world. While English literature is domiciled in half a dozen countries, Yiddish literature is created and appreciated in nearly every country of the world, Old and New. This literature, moreover, thrives under unique competition. The competition comes from modern Hebrew and also from the language of the country. Thus the literate American Jew looking for a periodical or book of fiction may choose among English, Hebrew and Yiddish.

An important problem in social psychology is broached on p. 41 ff., where the factor of imitation in cultural borrowing is qualified by the principle of national or cultural selection. It may be added that the selective principle is also operative in the individual's appropriation of the culture of his group.

Two bibliographies and three indexes are usefully appended to the book. The first bibliography includes a list of works (poetry, short stories, novels, and plays) available in English translation. Most fully represented in the list are the novelists Asch and Singer and the playwright Pinski.

Cornell University S. FELDMAN

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By F. NOWELL JONES, University of Alabama

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